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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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One primary influence on dynamic ice behavior is ice sheet hydrology, i.e., the storage and transport of water in the englacial and subglacial environments, and the development of transport pathways. Hundreds of subglacial lakes have been detected in Antarctica since 1967 by radio-echo sounding (RES), based on their characteristic strong basal reflections caused by the specular reflection of the water surface. Research in recent years has shown that the hydrodynamics at the base of ice sheets is complex and time-varying. Highly accurate altimetry data now allow us to monitor active plumbing dynamics beneath ice sheets, including large water mass exchanges be-

tween adjacent reservoirs. Detailed spatiotemporal reconstruction of drainage events and numerical investigation of the stability of coupled ice sheet-lake systems indicate that drainage events are a ubiquitous phenomenon in Antarctica. A recent Antarctic inventory identified 379 subglacial lakes (Wright and Siegert, 2012), a third of which were active in 2003-2008, during the ICESat campaign (Smith et al., 2009). However, as the manuscript points out only a few of the active lakes have RES signatures as well, making the reconstruction of subglacial storage and pathways difficult.

It is well written paper that presents new results for better characterizing the subglacial environment. The authors combine satellite altimetry, depicting the temporal evolution of the ice sheet surface, and a RES survey to characterize hydrological potential and the extent of water on the ice-bed interface. They demonstrate some of the limitations of currently available data, (1) RES surveys lack the spatial resolution to derive accurate hydropotential-maps, needed to identify lakes controlled by the ice sheet surface slope rather than the bedrock topography, and (2) the bedrock-ice interface conditions during the RES survey (2010-2011) could have been different from those occurred during the active water transport detected by ICESat satellite altimetry (2003-2009)

As the other reviewers have already noted, the manuscript could be further improved by including (1) details of the bed and surface elevation measurements (for example, by showing the locations of all flight lines), (2) the description of the computation of the elevation grids, and (3) error estimates of bed and surface elevations, hydro-potential maps and hydrological pathways.

As Dr. Carter already mentioned in his review, it is unfortunate that no RES surveys were flown along the ICESat ground-tracks and thus the authors missed an opportunity of effectively fusing the radar measurement with the ICESat altimetry record. The current flight design was probably driven by logistics as well as by the approximated shape of the Institute E2 as it was inferred from surface topography and ICESat altimetry (Smith et al., 2009). Overflying ICESat ground tracks would have enabled the estimation of radar altimetry errors (comparison of ICESat and radar altimetry over stable regions) as well as the extension of the altimetry record until 2010/11.

Additionally, ICESat elevation data outside the "active lake" can be used to quantify the error of the radar altimetry DEM, thus allowing an objective assessment of the errors of the different DEMs instead of the qualitative statement in page 2984, line 1-5. Moreover, the surface elevation derived from radar altimetry is accurate enough (better than 1 m according to the manuscript) to determine elevation changes after the termination of the ICESat mission (2009) at the intersection of the RES transects with the ICESat ground-tracks. Such a comparison can show if a sudden drainage occurred between 2009/2010 and 2010/11, indicating different subglacial conditions during the ICESat mission and the RES survey.

I believe that rather than having discontinuous water in Institute E2 (2985, lines 20-25), the extent of Institute E2 is much smaller than originally anticipated. The 2D extent of Institute E2 was derived from elevation changes estimated along a single ICESat reference ground track, from the fact that neighboring tracks did not show ice sheet elevation changes, and from the surface and bed topography. There is no evidence that the suggested center of Institute E2 (intersection of RES survey lines shown in Fig. 5) is indeed located at the center of an "active lake". It appears that the shape of the lake was not well defined, but it is rather the result of the interpolation procedure applied by Smith et al., 2009, which preferentially reconstructed circular or oval lakes. Actually, the regions of high RES strength, located W, NW to the assumed lake center, coincide with hydro-potential maxima. They are also consistent with the elevation change detected by ICESat. Taken together, the observations indicate that the extent of the "active" basal water body, located to WNW from the suggested lake center, is much smaller than the originally estimated lake extent. It is difficult to establish the correspondence between the radar transects and the maps (no coordinates on the maps and only distance is shown on the transects), but it seems that there is a small bed depression near A' with a bright radar returns – could this mark the extent of the basal water body? Showing a revised boundary of Institute E2 based on the new results

would be a great addition to the manuscript.

I was somewhat disappointed that the RES profiles were only used to estimate the reflection strength and the bed topography. There are intriguing englacial structures as well as subglacial geomorphologic features that might reveal details on the location and evolution of the draped basal water. Also, as recent results have shown (Schroeder et al., 2013), the angular distribution of reflected energy could be used to further characterize the subglacial water system. Finally, I wonder if the radar signatures of the surface of a "draped" water body are indeed identical to those obtained over the flat surfaces of topographically controlled lakes as the authors assume in the manuscript.

Technical comments/questions: Page 2983 line 16: is ice thickness error an RMS error? Line 18: what gridding algorithm was used? Was the thickness gridded and subtracted from the surface elevation grid or was the bed elevation gridded directly? Page 2984 line 16: it would be useful to show the direction of the ice flow in Fig. 4 Page 2984 line 17: what is a typical surface slope over the lake?

Figures: It is difficult to establish the correspondence between different figures. Using the Easting/Northing shown in Fig. 2 on the other maps would be helpful.

Fig. 3 Was the 2009 volume change computed from the last ICESat missions as part of this study?

Fig. 7 Showing the basal reflection strength and hydro-potential together with RES transects would facilitate the comparison and the joint interpretation of the different results.

Reference (not included in manuscript): Schroeder, D. M. & Blankenship, D. D. Evidence for a water system transition beneath Thwaites Glacier, West Antarctica. in (2013). doi:10.1073/pnas.1302828110/-/DCSupplemental

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