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Interactive comment on "Radio-echo sounding measurements and ice-core synchronization at Dome C, Antarctica" *by* Anna Winter et al.

Anonymous Referee #2

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This manuscript presents a first comparison of radar data collected with five different systems in the vicinity of Dome C. All of these airborne radar systems (AWI, UTIG, CReSIS and BAS) have generated the majority of the radar data in Antarctica and Greenland so that I see an overall merit to compare these datasets, not only for the oldest ice site survey, but for the ice-sheet research in general. However, the comparison presented in this manuscript is not at all rigorous. It is more or less just a visual inspection to develop fuzzy impressions (that many people already have, I believe), rather than a careful, scientific comparison to rigorously see what can be said and what should not be said by synthesizing different radar datasets together. The goal of the analysis is to compare the RES and synthetic radar data in terms of identifying distinct reflectors that can be found in all datasets and that can be confidently be matched in between the different datasets (P6L10-12). The analysis presented in this paper is

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inadequate to make this point. As I point out below, I see many not-well-justified procedures in the data/method section. Also, relevant information are shown in many places in the paper, so it is very hard to develop a confident understanding on the analysis presented here.

Individual comments (there are editorial comments as well, but only a few):

Title: With this title, readers cannot find that there is the first comparison of the radar data collected with five different systems. How about "Comparisons of radar data collected by different systems to synthesized radar data using the Dome C ice core, Antarctica"?

P1L3: bedrock -> bed, bed can be sediment, and not always rock.

P1L4: quality -> capacity?

P1L10-13: please improve the manuscript, otherwise the statements here are not well supported.

P1L13: Add "EDC's" before AICC2012. Is it necessary to be so specific on the timescale in the abstract?

P1L17: perfect -> valuable. Nothing is perfect.

P1L18: "air bubbles and hydrates"

P2L3: revise "this new, older core". The oldest core is not drilled yet.

P2L8: I understand that age structure refers three-dimensional age distribution within the ice sheet. However, it is not necessary to identify the oldest ice. Please clarify what is needed for the oldest ice survey and separately for more general interests of the ice-sheet research.

P2L9: delete "dielectric properties" in front of "density".

P2L10-17: Fujita and Mae (1994, Ann. Glaciol.) is the first paper to present the fre-

quency dependence of reflectivity in the ice. In the East Antarctic inland, the major reflection cause can be acidity or COF, depending on the radar frequency, ice temperature, and acidity/COF contrasts (Fujita et al., 1999 in author's reference list). In general, acidity-based reflection is more dominant at lower frequencies than 50-100 MHz, and COF-based reflection is more dominant at higher frequencies than 100 MHz. Such radar frequency dependence should be briefly mentioned here. And in a later section, the authors should address how 60 MHz data (UTIG) and 150-200 MHz data (other systems) can be compared, even if reflection causes are not necessarily identical.

P2L16: COF-based reflections do not necessarily constitute isochrones by definition, but Fujita et al. (1999) argued that COF contrasts can possibly be initiated by acidity contrasts so that regardless of the reflection cause IRH detected at any frequencies can be used as isochrones. This view is supported by a wide range of agreements between modeled isochrones and IRH observed at different radar frequencies. This work also supports this view.

P2L19: change to "from any ice core, if the isochrones. . ."

P3L7: "In the sections below we describe, the ICE-CORE data used for. . ."

P3L11-14: Please add adequate references to characterize EDC core sites. I don't think that Augustin et al. (2004) alone shows the full range of information presented here.

P3L19: If I understand correctly, ice temperature is assumed to be -15oC uniformly throughout the core. It is not the case. If the authors just need to have conductivity contrasts to identify acidity-based IRH depths, please say so clearly here to justify the uniform temperature assumption. Anyway, I cannot really understand the motivation of quite complicated (CPU expensive) modeling under such extremely simple assumption.

P3L20: why can the conductivity at the surface be assumed as 4.05 micro S/m? This

interpolation is made only for about 7 m so it does not make any major difference. However, I cannot see a reason why the conductivity at the surface has this value. If it is not well justified, why don't you assume it constant over this 7-m-long segment?

P3L27: add space to 5mm.

P4L2: Is Equation (2) necessary?

P4L8: permittivity is assumed to be 3.17; any reference? Is it reasonable for the temperature range measured at Dome C? Please present an ice temperature profile measured in the borehole.

P4L10-11: I don't agree. IRH is a result of many individual reflections caused at conductivity contrasts located close to each other. To calculate such interference of many reflected waves, phase is important and phase is dependent on conductivity (its value, not only the contrasts). If author's argument is really the case (i.e. only conductivity contrast is necessary), the authors can simply use the DEP results without any modeling. The bottom line: please clarify what "for the purpose of reproducing reflections" really mean.

P4L11-13: hard to understand; please revise. Radar data are collected in the two-way travel time domain. Do authors want to say "reflections in the depth domaion" (not the TWT domain)?

P4L14: Add space, 0.2 m

P4L14-15: It is a reasonable approach, but explicitly say that in this way only conductivity-based reflections are modeled, and permittivity-based (i.e. COF and density based) reflections are not modeled (Fujita and Mae, 1994, AGlac).

P4L19: model's depth/time increments are interchangeable in this context, I believe. 0.02 nsec and 20 mm are not equivalent if the propagation speed is for e=3.17.

P4L21: What's the exact purpose to use EMICE? Ice temperature is assumed uniform.

Permittivity is smoothed out. Input radar waveform to EMICE is inconsistent with any of the radar system. Is it really necessary to use EMICE? Why is the model output smoothed over 150 nsec (equivalent to about 13 m in ice)? If so, please use the model for more realistic conditions.

P4L27-31: This is a major tuning. It is assumed that AWI data (not exactly at the core site) and synthesized results show consistent englacial reflections and permittivity is tuned. Such assumption should be mentioned more explicitly. What is the corresponding ice temperature to e=3.17? What is the range of permittivity along the core associated with the ice temperature variations? Overall is e=3.17 a reasonable assumption here?

P4L32: which IRH are compared between AWI's data and synthesized results? All IRH?? Revise "For this value, the identified reflections occur at the same TWT for both traces". Again, permittivity is temperature dependent. And, somewhere further down in the manuscript, firn correction of 10 m is made (Fig. 3 caption). Then I am really puzzled; information necessary to understand the TWT/depth conversion scattered many places in this paper so it is really hard for me to follow author's logic.

P5L8: "(CReSIS) at the University of Kansas, . . ."

P5L11ff: Please use Table 1 more effectively. The range of information given in the text is also given in the table. References should be added to identify the processing procedure; descriptions here are too vague and brief to have the full understanding of individual datasets. In addition to items currently presented in Table 1, it is useful to show flight height, stacked distance, reference to processing procedure, etc.

P5L19: What does "unprocessed" exactly mean?

P5L28: What is "pulse envelope radar"? Is it a pulse-modulated radar that records only the returned power, not phase?

P6L6: What is an automatic gain control? I guess that it is a way to compensate

geometric spreading and attenuation within the ice, but no details are given. Is this gain adjusted correctly so that the gain is not increased once the received power reaches to the noise level?

P6L10-12: Here, the goal of the analysis is clearly articulated: “our aim is to compare the RES and synthetic radar data in terms of identifying distinct reflectors that can be found in all datasets and that can be confidently be matched in between the different datasets.” However, as I pointed out above and will do so below, the analysis presented here is not adequate to meet this goal.

P6L24: Physics behind the sentence “The exponential trend is removed from every trace” is that (1) ice temperature is uniform from the surface to depths, (2) chemistry is also inform, and consequently (3) attenuation rate is uniform. This feature may be seen in the model results, but if so it is only because the ice temperature is assumed to be uniform in the model. Again, I am really puzzled; what do the authors want to replicate by the model and for that goal what can be simplified? Information on these points are scattered many places in the manuscript so it is very hard to read.

P6L26-27: Please revise. I cannot understand. Figure 2 shows the returned power in arbitrary scale; is it linear scale or dB scale? If the latter is the case, does the panel show log of log??

P7L1: remove approximately. One third is approximate anyway.

P7L10-11: repeated/duplicated information. Delete.

P7L13-14: I cannot agree at all with the authors. Not all of ten IRH are confidently matched correctly. My quick view found no H6/H10 in AWI, no H3/H6/H9/H10 in UTIG, no H2/H3/H6/H9 in CReSIS, no H1-H6/H10 in INGV, and no H3/H6/H9 in BAS. I don’t expect that all of these features match pretty well between the all datasets. This level of agreement is something I don’t surprise and it is indeed a new finding of this analysis. Please articulate what you found; don’t stretch your results. I did not comment the

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rest of Section 5.1 and most of Section 5.2; please revise it accordingly based on my suggestions above.

P8L25: the term “echo-free zone” is misused here, if the authors follow the original definition made by Fujita et al. (1999, JGR). It does not simply refer the ice from which no echo is received. It’s upper surface is associated with a significant, sharp drop of the returned power, indicating the sudden loss of reflection even if the incident radio wave is strong enough (not attenuated so much).

P9L2-4: Again I’m really puzzled. What is the sensitivity test? AWI data were already used to have a best estimate permittivity/propagation speed. Why is conductivity mentioned here, though it is irrelevant to depth estimate? I did not comment on the rest of Section 5.3.

P9L29-30: Please make more rigorous discussion. For me, Sections 5.1 and 5.2 are inadequate to draw this conclusion.

P9L21: same -> similar.

P10L8: give the vertical sampling intervals in distance, not in time (permittivity is assumed to be uniform!)

P11L5: Revise. Reflector is an interface, with zero thickness. Do the authors refer the thickness of the layer bounded by two reflectors? Widen –> thicken??

P11L7-8: I don’t follow the logic. The SMB varies so density varies as well near the surface. But the density variations get smaller as it becomes deeper so spatial variability of the depth-integrated feature may not be so big (but I don’t know whether it can be very small or not).

P11L8-12: At such great depths, not only SMB but also ice flow affect the IRH’s shape.

P11L15-: Please reorganize. Bed topography is completely out of the context, and it is indeed confusing. If necessary please change the section’s name.

P11L31-33: This ice is called the “echo free zone.” If your interpretation is correct, this ice is not useful to reconstruct paleoclimate. It seems not consistent with the great success of the EDC ice core. . .

P12L5-7: I don’t follow where your confidence comes from. Table 1: It is useful if the table includes some features (center frequency, bandwidth, resolution) of the modeled radar data. Also, it is helpful if the table includes references of individual datasets, flight heights, lateral sampling intervals, etc.

Figure 1: Is the CReSIS line continue behind the inset?

Figure 2: unit of the lower panel is probably micro S/m. See my comments about the exponential trend.

Figure 3: please include the firn correction in the main text, and present all relevant information together. Rescale the INGV dataset so that horizontal structure is more visible, I think. What is “extended focused”?

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