Response to RC2 of 'Mapping age and basal conditions of ice in the Dome Fuji region, Antarctica, by combining radar internal layer stratigraphy and flow modeling' (tc-2023-35)

May 19, 2023

We thank Anonymous Referee #2 for forwarding these very helpful and thoughtful review comments. We are most grateful for the time the reviewer spent providing feedbacks on how to improve our manuscript. In our revision, we have tried to address the reviewer's suggestions as much as possible, specified in detail below. For clear tracking of changes, individual issues raised by the referees are referred to as (**Revision I**) below, where "I" is the number of the comment in the attached table.

This manuscript seeks to map out properties of the ice near the bed around Dome Fuji, East Antarctica. The authors trace layers in an airborne radar survey from 2017/2018, and date those layers using the age scale from an ice core drilled at Dome Fuji proper. They then fit a 1D pseudo-steady model (that has been applied extensively to Dome C) to the isochrones at each trace in the radar survey, from which they get an average accumulation, shape factor, and effective ice thickness (indicating stagnant ice or basal melting) at that point. The results suggest very old ice in the area, albeit with enormous (and, according to the authors, underestimated) uncertainty.

In the end, the conclusions here are rather thin. The issue with taking the method of Chung et al., in review, and using it on this survey is that the isochrones used here are much less than half as old (170 ka vs 476 ka), and the resultant impact on the reliability of the results is enormous. Essentially, the issue is that a 170 ka isochrone tells us very little about 1.5 Ma ice. Multiple problems can occur: small violations in model assumptions will result in inferred ages that are unrelated to reality (e.g. if ice flow, ice thickness, or accumulation varied in unexpected ways), and overfitting to measurement errors on these young isochrones will cause incorrect results at older ages. Indeed, when the authors check this possibility with even a 230 ka isochrone, they find that there is a huge change in ages as we would expect if the young isochrones simply do not carry much information about deeper ages. In my view, that this problem is occurring is demonstrated conclusively since the model does not match the Dome Fuji ice core's age scale. This is the only really available test of the model reliability—and not only does the model miss the age scale, but it does so outside its reported uncertainty! That is to say, the model is both wrong and confidently so. As a result, I do not think that much can be concluded from this paper, other than that old ice may or may not exist at Dome F. To their credit, the authors describe the limitations in the discussion and conclusion (though it should be better disclosed in the results), so I think that after revision the work will be publishable in The Crysosphere.

I think the presentation could be improved in both the paragraph-scale structure and at the sentence level, and the figures could be improved as well. The paper is in need of quite a bit of grammatical work, as there are a lot of missing or extra articles and some subject/verb mismatch. I did not enumerate these in my review. At times I found the paper hard to read, although I think I could eventually discern the meaning, so this could probably be handled by a copy editor. At some points, paragraphs wander away from the thread of the manuscript (see general comments). I agree with the other reviewer that the figures are pretty difficult to make out given their size. Throughout the manuscript, colorbars start and end at arbitrary values and demarcations are difficult to read—well chosen start and end values, arrows to indicate whether the colorbar values are inclusive, and enlargement would help.

To avoid the misunderstanding of the dataset we have used in this study, it's important to clarify the radar dataset was collected with the AWI radio-echo sounding (RES) system during 2016-2017 Antarctica season, not the dataset collected with the UWB radar in 2017-2018 season. As stated in the system description, this is a pulse system which records rectified waveforms, i.e., it does not record any phase information, thus yielding lower resolution, especially of internal layers.

Because of the data quality, the continuous internal reflection horizons (IRHs) can only be traced in the upper part and dated back to 170 ka. This limitation couldn't be solved from the data side. We agree with the comment that the unexpected variations of thinning in the deep part could lead to the mismatch between the model and reality. As the reviewer pointed out, there is a significant overestimation of age of basal ice at DF (Figure 7), however, we think that this figure actually proved the reliability of the model to a certain extent.

In Figure 7, we show the age-depth scale derived from models at Dome Fuji. We pointed out the age of basal ice was overestimated by a factor of 2 at DF. This figure shows, for RUN II (the run with 7 IRHs), that the deepest IRH was traces at the depth of 2238.22 m at DF, and dated back to 232.65 ka BP. We consider that the model has a reasonable performance (agreement with the timescale DFO2006+AICC2012) to the depth of 2759 m, which is dated back to 540.48 ka BP. This depth is about 290 m above the bed, which is exactly the depth of the inflection point in the timescale DFO2006+AICC2012. As a model assumption, the age-depth profile of the model follows the exponential distribution below this depth. Thus, the large overestimation is actually caused by the curvature reversal below this depth in the timescale of the ice core.

A similar phenomenon was also observed in our companion study (Chung et al. in review) at EDC though much older IRHs were dated there. They pointed out: "The modelled age at the deepest dated point for the EDC drill site which was around 100-200 kyr older than would be expected from the AICC2012 age-depth profile ... Looking at the AICC2012 profile determined by experimental measurements, it follows an exponential profile until the lower 200 m of dated ice, perhaps meaning that the thinning is for some reason lower than the model would expect…".

Figure R1 here simply shows the comparison between the age-depth profile derived by the model and the timescale at EDC (AICC2012). The model result agree with the timescale perfectly until the timescale deviates from the exponential form. At DC the overestimation is more reasonable since the timescale at DC doesn't change as drastically as that at DF. In the DF case, to solve this overestimation problem, only more continuous isochrones below the inflection point, i.e., the lowest 300 m could provide better constraint for the model, which is not possible in our dataset, and in fact most likely also not easily possible with other data sets (Tsutaki et al., 2022).

The drilling of the DF deep ice core indicated melting at DF (Motoyama et al., 2021), which could explain the inflection point in the age-depth scale of the ice core. This would imply that the significant overestimation likely occurs in the area with basal melting. Given that there is only one deep ice core in the DF region, we lack an additional timescale extending towards the bedrock to prove our hypothesis. At the same time, we find it unjustified to draw the conclusion that the model approach does not work in the DF region. According to what we have found by now, the model works quite well in the upper 2/3 of the ice column by calculating our reliability index (the standard deviation of the age difference between observation and model results), and it also seems appropriate in the deeper part at DF, until reaching the depth of the inflection point of timescale.

Therefore, we proposed "the reliability of our model is probably overestimated" in the manuscript as a responsible statement. In fact, this probability is higher in the area with basal melting. We will highlight this hypothesis and emphasize the importance of considering basal thermal state while finding old ice by our model more in the revision. Since we have found that melting prevails over stagnant ice in the DF region by using our model approach, it is possible that significant overestimations of age occur in the deep ice in the ice) at various places, as what has been observed at the DF. Taking this into consideration, we will include an additional Figure 4d in the manuscript that depicts the age of ice at a depth of 250 m above the bed. This figure could provide relatively accurate age values while excluding the lowest part of the ice. **(Revision 23)**

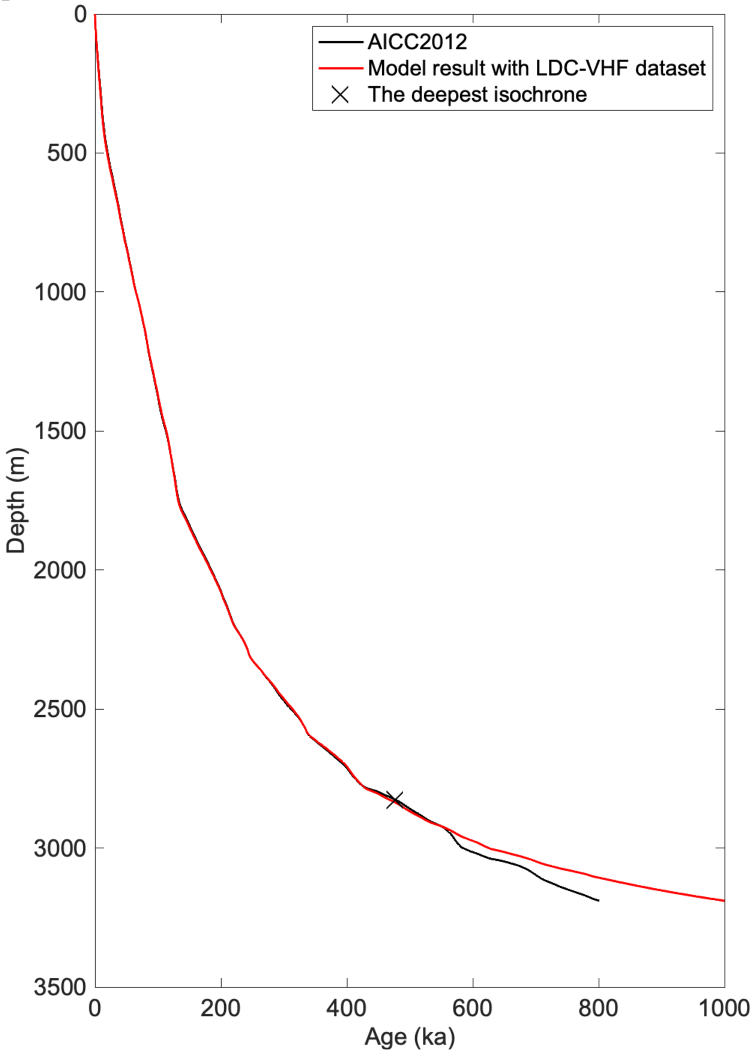

Figure R1. Model derived age-depth scale and AICC2012 timescale at EDC.

In the revised version of the manuscript, we will improve the section 4.3.3 in the sense of what we have discussed here. **(Revision 24)**

As the reviewer suggested, we will also improve our conclusion. **(Revision 25)**

We will follow the comments and refine our figures by adding the start and end values to the color bars and placing the ticks outside the color bars. **(Revision 3-6, 26)**

We will conduct a thorough review of the manuscript, carefully addressing and correcting the grammar and vocabulary errors. **(Revision 27)**

General comments

Basal layer is a sticky term. As used on line 26, it sounds like generic deep ice. However, in other literature, it refers specifically to ice that has a distinct radar character, to the ice near the bottom of EDC that has little discernible paleoclimatic information, or to ice that is inferred to be stagnant. The situation is further muddled on line 70, where the authors suggest that their method can detect a potentially stagnant basal layer—but this is incorrect. The method can only detect a stagnant basal layer (at least in terms of vertical velocity), since the whole premise of the detection is that the basal layer is stationary for the purposes of the depth-age scale. This work should be consistent on its usage of the term basal layer and it should define what it means by that in the introduction. Imprecise usages such as that in line 26 should be removed. Some discussion should be added on whether there is any correspondence between the areas where stagnant ice is inferred and any characteristic of the radargrams.

Thanks for pointing out the confusing use of 'basal layer'. We decide to use 'basal unit' as the bottommost part (which has been used in the literature before) in which there are some peaks in return power but no continuous or coherent reflecting horizons. We use "stagnant ice" as the stagnant bottommost ice above the ice-bed interface derived from the model.

We will change "basal layer" to "deep ice records" in Line 26. **(Revision 28)**

We will change "a (potentially stagnant) basal layer" to "the stagnant ice" in Line 70. **(Revision 29)**

Except for the two changes suggester by reviewer, we will also change "...there is a basal layer of stagnant ice…" to "...there is stagnant ice…" in Line 153. **(Revision 30)**

We will change "basal layer" to "bottommost part" in Line 367. **(Revision 31)**

In the radar dataset we used, there is an echo free zone (EFZ) above the bed, with a thickness of several hundreds of meters. EFZ could be caused by various reasons, e.g., system sensitivity of the radar system, deformation, recirculation and recrystallization of ice (Drews et al., 2009 and Franke et al., 2023). We think the EFZ in our dataset could most likely be caused by the performance of the radar system, since in the same region more modern radar can detect somewhat deeper, more coherent horizons, as we have pointed out in our manuscript (L340), 'It implied that at the same depth modern systems would provide not only a higher resolution, but most likely also a deeper detection of continuous IRHs (Rodriguez-Morales et al., 2020).' We will point this out in section 4.2 when we discuss the basal thermal state and 4.3.4 when we discuss the limitation of radar system. **(Revision 9)**

Given the wide pulses of this radar system, and the lack of pulse decompression, I am skeptical of the vertical precision that the authors are claiming and I would suspect a bias. Just by two way travel time, the pulse is 50 m long (as the authors correctly point out, this is the resolution, defined here as the separation needed to identify two targets as distinct). Again, as the authors correctly identify, this resolution is different than the precision (i.e. the depth-accuracy of a target). However, the authors trace in a fairly standard manner (picking *the strongest return), but given the processing of these data the depth of the reflector should really be off the first return, assuming that time zero is defined as the start of the pulse (indeed, this is what you would get if you could do pulse decompression). The problem is that I would expect the strongest return to lie below the first return from a reflector (most likely 25 m below, but this offset is somewhat arbitrary), but never above. Thus, I think that there is likely a systematic bias in the ages and depths used. This may have a small effect on age-depth scales in the end, since it may affect an isochrone in the same manner along its length, but it should be accounted for carefully.*

In the next figures R2 and R3 we show some screenshots of the returns of different IRHs, the horizontal axis shows traces, the vertical axis shows Time (μs) (the unit is s on the figure, because the software is originally used for seismic data).

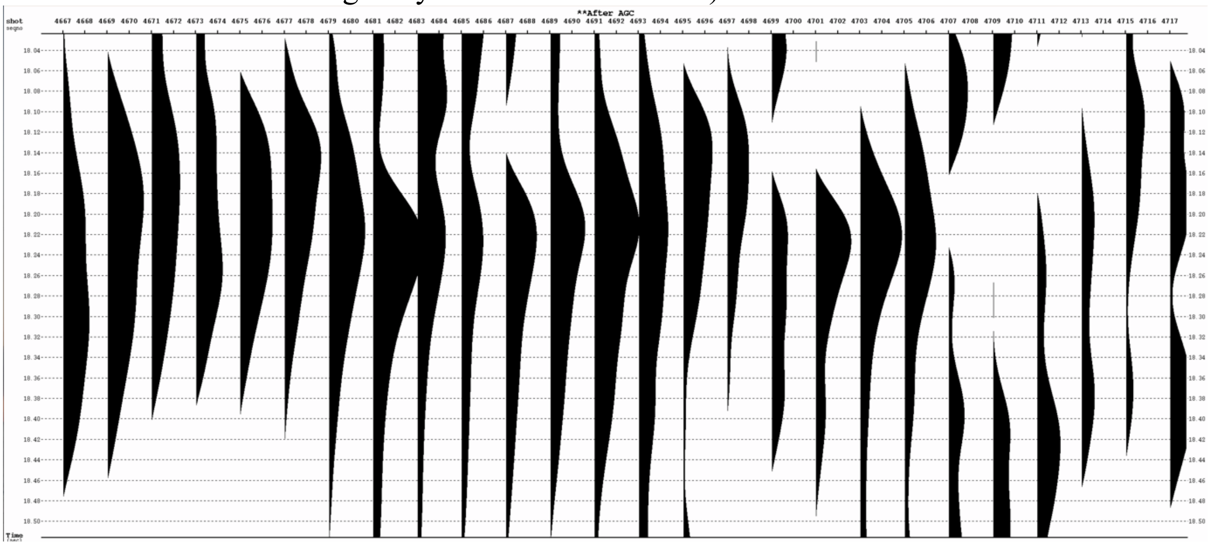

Figure R2. Zoom-in view of IRH4 returns in Profile 20172029.

For a single pulse, the bias between the first return and the strongest return varies, but generally ranges from 50-200 ns.

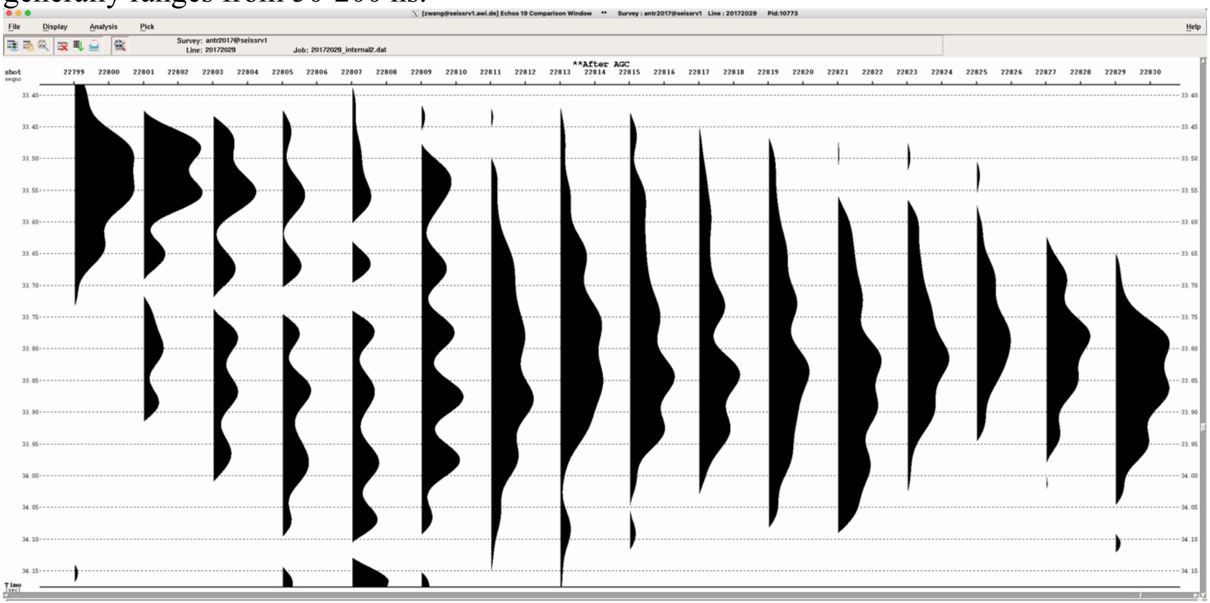

Figure R3. Zoom-in view of IRH7 returns in Profile 20172029.

For IRH7, in most traces, multiple return pulses overlap. E.g., for trace 22815, the first return occurs at 33430 ns, but the strongest return we traced in Paradigm could be at 33850 ns, what makes the offset around 420 ns. The bias varies from 50-420 ns in the best case, which is too big to be a systematic bias regarding the pulse length of 600 ns.

Hence, we prefer to keep our revised description for the age uncertainty. **(Revision 8)**

Overall, uncertainty deserves a more prominent place in the results and discussion. First, how is the basal age uncertainty that the authors report calculated? I am guessing it is as in Chung et al., but there is not even a passing mention in this manuscript of how the authors obtain anything other than a best-fit value—it is critical to add this to the manuscript. Then, there is the issue of uncertainty in basal age ice—the number reported for Dome F itself is plus minus 500 kyr. This is enormous, over a third of the age. Figure 4 should plot the uncertainty on the basal ages—without this, the reader has no idea if there are any areas at all with reliably old ages. This gets addressed later by table 1 and the sensitivity analysis, but I see no reason that it cannot fit in Figure 4.

Thanks for pointing this out. The theory is the same as the one described in Chung et al., which provides a detailed account of the approach. Nevertheless, we will add more details of the age uncertainty at the end of the Method session to make it clearer. **(Revision 1)**

We consider the value of the uncertainty of the basal age as a significant quantity in the entire region. We have compared it with that from the Dome C region. We gave the age uncertainty almost the same order of magnitude for each isochrone, but the basal age uncertainty is much smaller in the Dome C region. We relate this to the fact that the number of isochrones used as constraints for the model is larger in the Dome C region. We will add the age uncertainty of the basal ice as Figure 4c. **(Revision 32)**

The comparison with Karlsson 2018 and van Liefferinge 2018 deserves more consideration. While the approaches are different, what can we learn by comparing them? Where should we believe their results over the results here (for example, are the results here thermodynamically tenable)? Where is there agreement?

Karlsson et al. (2018) and van Liefferringe et al., (2018) used a thermodynamical model to identify old ice sites, but their approaches did not include any constraint from the age of radar internal layers. The model we are using does not take into account the thermal dynamics at all, thus being more independent of GHF estimates. However, the sites with potentially "old ice" suggested by the different approaches show a considerable correspondence in some places, especially around DF and NDF. We think that the underlaying reason for this consistency is the use of ice thickness in both models, and the crucial impact of ice thickness on the age distribution of the ice.

In the figure, we put the sites with old ice suggested by the thermal model on top of our results. In this way we aim at giving readers a visual impression, which site is more likely to hold old ice, both mechanically and thermodynamically. We will rephrase the paragraph to make it more logical. **(Revision 33)**

The introduction could use some work. We would benefit from more focus on what the reader should take away. This is most obvious in the paragraph beginning at line 40 wanders between detailed analysis of timescales, studies that concluded something about basal thermal state and potential old ice sites (without ever stating those conclusions). I suggest a careful culling of the introduction, focusing on the goals of each paragraph.

We will split the paragraph to two parts, the research conducted on the Dome Fuji ice core and other studies performed in the large Dome Fuji region. We will add the conclusions of the previous studies. **(Revision 13)**

Detailed comments

L36: If efforts beyond BE-OI are discussed, it seems strange to include Dome A but not other countries' oldest ice efforts. For example, Beem et al. (2021) would then also be appropriate here. Also on L36, the sentence needs to be rephrased—is the point that the age is limited or that it reaches 1.5 Ma?

We will add the reference as suggested to have a complete overview. **(Revision 34)**

We will change Line 36-39 "In the Dome A region, Sun et al. (2014) estimated ice age around Kunlun station by applying a three-dimensional, thermomechanically coupled full-Stokes model, which indicated that in the area without basal melting the ice age at 95 % depth could be limited to 1.5 Ma."
to
"In the Dome A region, Sun et al. (2014) estimated the age of ice around Kunlun station by applying a three-dimensional, thermomechanically coupled full-Stokes model assuming different geothermal flux and fabrics. They imposed a 1.5 Myr limit to the age solver, thus they did not get the actual age of the oldest ice, but the distribution of ice potentially older than maximum run time of their model." **(Revision 12)**

L81: I find this system description to be a bit vague. I was under the impression that the AWI system is multi-channel and phase-coherent. Is this a single channel power, or the total transmit power? Pulse-limited is not a term we see that often for ice-penetrating radars (perhaps more for radar altimeters), so it would be helpful to say the importance explicitly—that it acts much like a chirp system where you cannot decompress the chirps (thus the very low vertical resolution). Is there no reported bandwidth because there was no frequency sweep in the chirp? If the chirp did sweep frequencies, the bandwidth should be reported.

The dataset we have used in this research was collected by AWI radio-echo sounding (RES) system during 2016-2017 Antarctica season, not the dataset collected by UWB radar in 2017-2018 season.

The AWI RES system is a burst system, with center frequency of 150 MHz. The pulse generated has length of 60 ns or 600 ns, as we referenced in our manuscript (Nixdorf et al., 1999). The signal transmitted by the system is not chirp with a frequency sweep, but a short, high-power pulses with specific widths, which is in addition rectified after reception. We will also mention the bandwidth in Line 82, we will change
"The AWI RES system transmits radar waves with a center frequency of 150 MHz and an amplitude of 1.6 kW."
to
"The AWI RES system transmits radar waves with a center frequency of 150 MHz, a band width of 20 MHz and an amplitude of 1.6 kW." **(Revision 35)**

L89: Should explicitly state what kind of 2d filter

It is a particular filter in the software “Echos”, which we use for analysis. Since the filter could be adapted to the radar data in both trace direction and time direction, it is called a 2D filter. This filter returns a weighted running average. It is summing up amplitudes at the same travel-time, which could remove the horizontal noise. We will change
“…a low-pass filter and a two-dimension filter are…”
to
“…a low-pass filter and a running average filter are…” **(Revision 15)**.

Figure 1: “examplary” implies that it is an ideal or “the best” profile, while I think simply “example” would be more accurate. Different colors for contours and survey would help readabilility. An increase in size would be nice too.

We will change “exemplary” to “example”. **(Revision 20)**

We will change the color of the contours. **(Revision 3)**

Figure 2: The horizontal black lines make this almost impossible to interpret. I would also like to see a zoom in on some of the tracing, so that we can see how the traced depth relates to the width of the returns. Why not just use a plus/minus for the uncertainty and avoid the confusing double parenthetical?

We will remove the black lines and improve the figure.

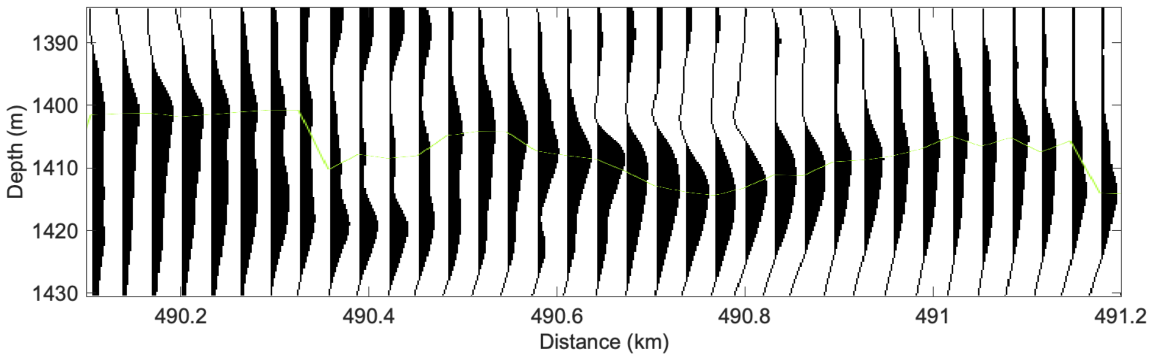

Figure R4. Zoom-in view of IRH4 picked in Profile 20172040.

We show a zoom-in view of profile 20172040 in Fig. R4. Same as the returns of zoom-in view of different horizons in Profile 20172029, which we have shown in General comment, there is no trustworthy systematic bias that could be adapted to all the picks.

Thanks for the comment, we will use plus/minus to replace double parenthetical. **(Revision 36)**

L140: This needs to state how accumulation is inferred for ages that predate the oldest ice in the core

Thanks, we will state how accumulation rate inferred. **(Revision 37)**

L141: “Inverted” means “was turned upside down”—but this was both inverted and integrated. Here “inferred” or simply “calculated” would be correct.

Thanks, we will change “inverted” to “inferred”. **(Revision 38)**

L142: This needs to be rephrased to make clear that the presence of stagnant ice is undetermined

Thanks for the comment. From Line 147, we reorganize the paragraphs, by moving the description of H_m forward, and then describe the optimization.

We will change
“…where H_m is the mechanical ice thickness, which means the effective ice thickness above the stagnant ice, and p is a shape factor controlling vertical deformation (Lilien et al., 2021).”
to
“…where p is a shape factor controlling vertical deformation (Lilien et al., 2021), H_m is the mechanical ice thickness, which is different to the observed ice thickness H_{obs}. When H_m is greater than the observed ice thickness H_{obs}, we have melting conditions at the base. Otherwise, there is stagnant ice. If the basal ice is melting, the melt rate m can be obtained by…” **(Revision 39)**

L149: Considering that Chung et al. And Lilien et al. use inverse methods that produce uncertainties, it is important to state here whether this method does the same or whether this work simply finds the single solution with the lowest misfit.

We will add a few paragraphs to describe the optimization methods. **(Revision 1)**

L156: Could this be renamed? It is essentially the inverse of the reliability, so it is quite confusing to call it the reliability index.

For consistence with our companion manuscript, we prefer to keep the same naming. But to avoid misunderstanding, we add “σ_R” after “reliability index”. (**Revision 40**)

3.1: Same issue with meaning of “exemplary”

We will change “exemplary” to “example” in Line 167 and 168. **(Revision 41)**

L171: Delete parenthetical—thickness cannot be deep

Done. **(Revision 42)**

Figure 5: The Van Liefferinge data need a different color—gray on a gray background is unreadable.

We will change the gray polygons to (another) color. **(Revision 5)**

Final paragraph: I do not think this is a conclusion, nor is it a logical way to approach the limitations of this work. Overall, it is a rather weak note to end on—why not move it to the discussion, which is what it really is anyway?

We will rewrite the conclusion. **(Revision 25)**

References

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Lilien, D. A., Steinhage, D., Taylor, D., Parrenin, F., Ritz, C., Mulvaney, R., Martín, C., Yan, J.-B., O'Neill, C., Frezzotti, M., Miller, H., Gogineni, P., Dahl-Jensen, D., and Eisen, O. (2021): Brief communication: New radar constraints support presence of ice older than 1.5 Myr at Little Dome C, The Cryosphere, 15, 1881–1888.

Motoyama, H., Takahashi, A., Tanaka, Y., Shinbori, K., Miyahara, M., Yoshimoto, T., Fujii, Y., Furusaki, A., Azuma, N., Ozawa, Y., et al. (2021): Deep ice core drilling to a depth of 3035.22 m at Dome Fuji, Antarctica in 2001–07, Annals of Glaciology, 62, 212–222.

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Van Liefferinge, B., Pattyn, F., Cavitte, M. G., Karlsson, N. B., Young, D. A., Sutter, J., and Eisen, O. (2018): Promising Oldest Ice sites in East Antarctica based on thermodynamical modelling, The Cryosphere, 12, 2773–2787.

Revision number	Reviewer	Position	Before	Rephrase	Revision		
1	1 2	Section 2.4			Add further details of the model		
2	1	Line 211			Add the caveats		
3	1 2	Figure 1			We increase the size of data points. We increase the size of markers of DF and NDF in the figure. We give the values of colorbars at start and end. We put the ticks outside the color bar to see them clearly.	Change the contours color of the surface elevation.	
4	1 2	Figure 4				We replace the gray scale bed elevation background with colored contours with different line style (only in Fig. 4a, test).	
5	1 2	Figure 5				Add ellipses to show the old ice sites.	Change gray polygon to another color.
6	1 2	Figure 6				Make stagnant ice clearer.	
7	1	Flank flow			Rephrase this part and provide velocity map		
8	1 2	Section 2.3			We rephrase this part in a clearer way in revision.		
9	1 2	Section 4.2/4.3.4			We point out the limitation of radar system, i.e., basal unit couldn't be observed in the radargrams.		
10	1	Line 6	basal layer	stagnant ice			
11	1	Line 26	feasible	useful			
12	1 2	Line 37-39	In the Dome A region, Sun et al. (2014) estimated ice age around Kunlun station by applying a three dimensional, thermomechanically coupled full-Stokes model, which indicated that in the area without basal melting the ice age at 95 % depth could be limited to 1.5 Ma.	In the Dome A region, Sun et al. (2014) estimated ice age around Kunlun station by applying a three-dimensional, thermomechanically coupled full-Stokes model assuming different geothermal flux and fabrics. They imposed a 1.5 Myr limit to the age solver, thus they didn't get the actual age of the oldest ice, but the distribution of ice potentially older than maximum run time.			
13	1 2	Line 56-	…Karlsson et al. (2018) presented an updated subglacial topography…	… In the large DF area, Karlsson et al. (2018) presented an updated subglacial topography…	Start a paragraph about the researches in the large Dome Fuji region. Add the conclusions of the previous studies.		
14	1	Line 56	…based on an airborne radar surveys…	…based on airborne radar surveys…			
15	1 2	Line 89	...two-dimension filter...	…running average filter…			
16	1	Line 98	We trace 6 or 7 relatively distinct and continuous IRHs (H1--H7) in all survey lines, the third IRH H3 is not clear and continuous enough to be traced in some profiles	We trace 6 (H1, H2, H4 – H7) or 7 (H4 – H7) relatively distinct and continuous IRHs in the radar profiles, since the third IRH H3 is not clear and continuous enough to be traced in some profiles			
17	1	Line 128	The estimate of the range precision is always higher than the resolution	The estimate of the range precision is always numerically smaller than the vertical resolution			
18	1	Line 264	…We show the reliability index in the…	… We show the reliability index (described in section 2.4) in the…			
19	1	Line 99		Add "This ice thickness data is available on Pangaea (Karlsson et al., 2018)." after "…through semi-automatic detection routines in Matlab..."			
20	1 2	Figure 1 caption	...from Greene et al. (2017) and Morlighem et al. (2017, 2020) ...examplary…	from Morlighem et al. (2020) and Morlighem (2022) ...example…			
21	1	Section 3.3			Compare the lakes distribution with DC.		
22	1	Figure 7			Use different color to make it clearer.		
23	2	Figure 4d			Add figure of age of ice at the depth of 250 m above the bed in results.		
24	2	Section 4.3.3			Rewrite this section.		
25	2	Conclusion			Make it more conclusive		
26	2	Figure 3			We give the values of colorbars at start and end. We put the ticks outside the color bar to see them clearly.		
27	2	Grammer			Revise carefully.		
28	2	Line 26	basal layer	deep ice records			
29	2	Line 70	a (potentially stagnant) basal layer	the stagnant ice			
30	2	Line 153	..there is a basal layer of stagnant ice…	...there is stagnant ice…			
31	2	Line 367	basal layer	bottommost part			
32	2	Figure 4c			Add age uncertainty of basal ice as c in the figure.		
33	2	Line 224			Rephrase the paragraph to show the importance of addiing the comparison with the previous work.		
34	2	Line 39			Add the reference Beem et al., 2021.		
35	2	Line 82	The AWI RES system transmits radar waves with a center frequency of 150 MHz and an amplitude of 1.6 kW	The AWI RES system transmits radar waves with a center frequency of 150 MHz, a band width of 20 MHz and an amplitude of 1.6 kW			
36	2	Figure 2			Remove the black lines. Use plus/minus to replace parenthesis.		
37	2	Line 140			State how accumulation is inferred for ages that predate the oldest ice in the core.		
38	2	Line 141	inverted	inferred			
39	2	Line 147	where H_m is the mechanical ice thickness, which means the effective ice thickness above the stagnant ice, and p is a shape factor controlling vertical deformation (Lilien et al., 2021)	where p is a shape factor controlling vertical deformation (Lilien et al., 2021), H_m is the mechanical ice thickness, which is different to the observed ice thickness H_{obs}, When H_m is greater than the observed ice thickness H_{obs}, we have melting conditions at the base. Otherwise, there is stagnant ice. If the basal ice is melting, the melt rate m can be obtained by…			
40	2	Line157/Fig 5 caption/Line 264/Line 265	reliability index	reliability index σR			
41	2	Line167/168	exemplary	example			
42	2	Line171	where the mechanical ice thickness Hm (purple dash line) is larger (deeper) than the observed ice thickness (black line)	where the mechanical ice thickness Hm is larger than the observed ice thickness			
43		Line 6	...do not exist, the basal thermal conditions, including the thickness of the stagnant ice surface accumulation rates	...do not exist, the surface accumulation rate and the basal thermal condition, including melt rate and the thickness of the stagnant ice.			
44		Line 99	Matlab	MATLAB			
45		Line 52	inverted	inferred			
46		Reference	Chung,A.,Parrenin,F.,Steinhage,D.,Mulvaney, R.,Martin,C.,Cavitte,M.,Lilien,D.,Helm,V.,Taylor,D.,Gogineni,P.,Ritz,C.,Frezzotti, M.,O'Neill,C.,Miller,H.,Dahl-Jensen,D.,andEisen,O.:StagnanticeandagemodellingintheDomeCregion,Antarctica,submitted.	Chung, A., Parrenin, F., Steinhage, D., Mulvaney, R., Martín, C., Cavitte, M. G. P., Lilien, D. A., Helm, V., Taylor, D., Gogineni, P., Ritz, C., Frezzotti, M., O'Neill, C., Miller, H., Dahl-Jensen, D., and Eisen, O. (2023): Stagnant ice and age modelling in the Dome C region, Antarctica, EGUsphere, 2023, 1–31.			
47		Line 75	...BaslerBT-67aircraft…	...BaslerBT-67aircraft (Wesche et al., 2016)…			
48		Reference		Add reference: Wesche, C., Steinhage, D., and Nixdorf, U.: Polar aircraft Polar5 and Polar6 operated by the Alfred Wegener Institute, Journal of large-scale research facilities, 2, 1–7, 2016.			
49		All the orientations			All directions should be referring to true north, thus, we will add "grid" in front of all the words representing orientations, e.g., we will change Line 180 "to the north-west of the DF drill site" to "to the grid north-west of the DF drill site"		