

Dear Reinhard and the Copernicus Editorial team,

Thanks for your invitation to review the manuscript "*Modelling the refractive index profile $n(z)$ of polar ice for ultra-high energy neutrino experiments*". It presents results from radio propagation experiments conducted at the South Pole ice core site and discusses how a three-component piecewise exponential model is better suited to describe the refractive index profile than typical single- or dual-component exponential models used widely in the literature (Herron and Langway, 1980) and the neutrino observation community (Oeyen et al., 2022).

In its present form, I would encourage the authors to undertake significant revisions to the manuscript. While the results presented are of interest to the ultra-high energy neutrino community and wider glaciology community, I think that there are several key components of the data presented which are not fully addressed in the text and instead offer opportunities for developing the manuscript into a more complete study of the dielectric-depth profile at South Pole.

A key area for the authors to address is the discussion in sections 5, 6 and 7 which is brief and does not fully reflect the results that are presented (i.e. residuals from different channels, lack of quantitative analysis). The authors mention that analysis of the amplitude data is underway - and if possible - I think that combining that work with this manuscript offers a more complete treatment of the experimental data. Even without the amplitude data, it is not clear why the authors focus only on the vertically polarised dataset, where comparison with the horizontally polarised data should be exploited to investigate anisotropic components of the refractive index profile at South Pole, which have been previously recorded from c-axis (Voigt, 2017) and time delay measurements (Allison et al., 2019).

Below I have outlined specific comments that the authors might consider addressing.

Kind regards,

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General comments:

- Section 2.1: can you consider a purely 'physical' model of deriving the refractive index based on the relative volume of snow and air such as the Looyenga model [citation]? Typically this gives a lower estimate of firn permittivity than the Robin/Kovacs formulation but is more 'physical' in keeping with the exponential 'gravity driven' Herron & Langway model.
- Throughout the text, replace "1 stage", "2 stage" and "3 stage" with "single stage", "dual stage", "triple stage" or similar, avoiding the use of numerals in the text.
- Section 5.2: This section needs further discussion and expanding, using data from the figures.
- Section 5.3: In my view this section doesn't provide an "analysis" of the timing residuals and instead very briefly discusses the datasets. Consider discussion on the difference between difference channels is necessary if you are going to present the data for each of them, otherwise, consider combining the figures (8, 9, 10) for A3, A4 and A5 into a single Figure. There is no discussion about increased residual at small receiver depths - why is this? Why is -500m the upper limit for the SPUNK depth?

- Section 6: This section is very short and at present doesn't warrant its own discussion. It would be interesting to see results on how the shadow zone in depth between A2, A3, A4 and A5 - perhaps this could be represented as a top-down map with interpolated values? This would be a valuable tool for planning of future boreholes or to allow for quick checking against existing results. At present, it is no clearer, except for the brief discussion in the previous extent, about the extent of this 'shadow zone' - consider combining this with Section VII.
- Conclusions: There are some points within the conclusions that are introduced for the first time. A large section of the conclusion doesn't reflect key points raised in the discussion and instead focusses on future work. It is for this reason that I think combining this manuscript with the outcome of the amplitude analysis has the potential to improve it.
- References: The references to glaciological literature are limited and the manuscript would be improved by including additional references throughout.

Specific comments:

- L15: Consider adding a brief description of the mechanism for how radio waves are generated in the context of neutrino detection for people coming from glaciology backgrounds.
- L27: Strictly speaking, it is also dependent on the imaginary part of the refractive index. Not typically relevant for cold-ice and likely safe to ignore, but acidic inclusions and temperate ice have the potential to contribute to large imaginary components ($\sqrt{\epsilon_i}$) and hence increase the effective refractive index ($\sqrt{\epsilon_r + j\epsilon_i}$).
- L42: Does this statement need to be in parentheses? It feels like a sensible caveat to have in the main text.
- L47: Is the symbol ρ_f equivalent to the pore close-off depth used in other literature?
- L76: Equation (4) is difficult to read with the constants in line with the text, consider moving these to a Table and simply referring to equation (2).
- L82: Compared to South Pole/Greenland summit, these sites might one expect these sights to have a higher equivalent permittivity through warmer ice and the presence of water or impurities?
- Figure 1: Reformat legends in Figure 1a to match reference formats in text. Panel a.) is very busy - are all the core density sets needed? If so, is there a way where these can be shown so that it is easier to discriminate between them (i.e. different marker styles?). It's not immediately clear why the Greenland datasets are presented when the experimental results are from South Pole.
- L93: As above, consider moving these parameters into a Table and present the equation symbolically.
- Figure 2: Consider labelling horizontal $\sqrt{\Delta\text{East}^2 + \Delta\text{North}^2}$ or combined horizontal and vertical $\sqrt{\Delta\text{East}^2 + \Delta\text{North}^2 + \Delta\text{Up}^2}$ distances from each A station to SDP to give a reference for the lateral and total distance travelled by a direct ray.
- L107: There is not any discussion of the difference between H-pol and V-pol data.
- Figure 3: Is this figure from this manuscript or has it been adapted from another source? If it has been adapted, provide a reference.
- L112: The waveforms are not symmetrical, so is a Gaussian the most appropriate kernel to use for this fitting approach? One assumes that the asymmetric tails come from multipath effects and hence why they are more pronounced for the refracted (R) wave?

- Figure 4: Add a label on the figure itself to indicate the direct (D) and refracted (R) components, Which polarisation is shown here (HH, HV, VH, VV) and why not all of the above? In the caption, consider using D and R, consistent with your earlier definition in the text. Consider directly annotating with the (approximate?) arrival time i.e. "Typical SDP waveform recorded at stations A2-A5 consistent of D (160 ns) and R (480 ns) pulses."
- Figure 5: Figure 4 and Figure 5 could reasonably be combined into a single figure to help understand the origin of the delayed R waveforms shown in Fig. 4. The shadow zone isn't immediately intuitive from the figure - could you make this a shaded/filled area above the line in the figure to represent graphically that this area is obscured or corresponds to non-physical ray paths, rather than relying on the text description alone.
- L141: Appreciating that the work is underway, I think it would still be information to give some examples of how the two ray paths might end up with different amplitudes (system effects such as radiation pattern, propagation medium such as the ice-surface reflection coefficient or internal scattering/reflections within the firn, total travel distance resulting in accumulation length, birefringent effects).
- L146: Why is A3 displacement given and then A2 used as the example - this feels inconsistent/repetitive. A better more consistent use of vocabulary to refer to the (vertical) depth, (horizontal) displacement(?) and (radial) distance - would be useful.
- L151: Over the 5km separation between stations it's very possible there is variation in $n(z)$ with depth in the upper layers - can you provide some evidence that this isn't the case and it is reasonable to say that $n(z)$ is homogeneous.
- Figure 6: What is "SPUNK" depth? This isn't defined in the manuscript.
- L166: This needs more discussion - your three-stage model results in an increased value of $n(z)$ at ~160m compared to the one-stage and two-stage and this is greater than the SPICE density data for that region. Unless your one-stage, two-stage and three stage models converge at a similar value at greater depths (i.e. 1300m or so), then a key difference between this three-stage model and the others is that it results in an increased overall density.
- Figure 7: There is an increased $n(z)$ with one-stage and two-stage models up to ~100m which will increase travel times, whereas there is an increased $n(z) > 100m$ for three-stage. If you consider a vertical (or angled) direct ray path and integrated to convert to travel time, how do these effects compare (i.e. does the increased $n(z) < 100m$ for one-stage and two-stage compensate the increased $n(z)$ for $> 100m$ for three-stage? What is RICE IoR measured - not defined in text? Consider extending depth to $> 170m$ so that $n(z)$ can be compared at depths relative to the receiver depths described later on.
- L169: Can this be described before introducing the symbols to add some context - it isn't clear from the symbol definition whether this delta is calculated the residual between the difference in D and R arrival and that expected from the simulated results. An additional equation here might be helpful and allow for rephrasing of the following paragraph.
- Figure 8: There is no discussion for the difference between different channels so I'm not clear why they are all presented for each station - what are the causes of different residuals for different channels?
- L174: This isn't obvious without a clear definition of $dt(D,R)$ - consider an equation.
- L179: I assume this justification for the 850-1300 m depth is why the limits for figure 8 and 9 is 500m also - but you don't mention that.

- L181: This feels like a key result that only the three-stage model is capable for resolving the shadow zone at greater distances and should be more explicitly signposted - at the moment, this is better described (but hidden) within the caption of Figure 10.
- L186: Is this also a pattern of constructive/destructive interference between the two signals? I assume this results in some intermediate/additional 'shadow' region at depths where there is destructive interference (i.e. D and R are $\lambda/2a$ out of phase) - perhaps this is what you will discuss in your amplitude analysis?
- Figure 11: Why is two-stage model not shown here? Expand "radial distance (r) and vertical depth (z)" and include in the axis labels of the figures.
- L195: By how much? This is shown in figure 12 but not described in the text - how does it vary with radial distance etc.
- Figure 12: Why are the neutrino energies relevant here? Again - not described in the text - and without references to relevant literature this part of the discussion is very opaque to readers in TC.
- L202: I'm not sure this is a 'conclusion' of the manuscript as presented, and instead better suited to discussion of future work.

Typing comments:

- L71: avoid use of forward-slash in the main text.
- L111: Slightly awkward phrasing - perhaps reconsider along the lines of "A typical waveform originating from the englacial pulse and received at the Hpol and Vpol antennas is shown in Figure 4".
- L145: Mix of units - 1-5 km and then 3230 m, make consistent? I'm not sure adding the exact distance for A3 here adds much to the text and a summary for all of the distances (incorporated into Figure 2, for example) might be more informative.
- L165: Is there a missing space before > symbol?
- L191: Consider renumbering Section 7 as Section 6.1.

References

Allison, P., Archambault, S., Auffenberg, J., Bard, R., Beatty, J.J., Beheler-Amass, M., Besson, D.Z., Beydler, M., Chen, C.C., Chen, C.H. and Chen, P., 2019. Measurement of the real dielectric permittivity ϵ_r of glacial ice. *Astroparticle Physics*, 108, pp.63-73.

Herron, M.M. and Langway Jr, C.C., 1980. Firn densification: an empirical model. *Journal of Glaciology*, 25(93), pp.373-385.

Oeyen, B., Plaisier, I., Nelles, A., Glaser, C. and Winchen, T., 2022. Effects of firn ice models on radio neutrino simulations using a RadioPropa ray tracer. In 37th International Cosmic Ray Conference (ICRC), JUL 12-23, 2021, ELECTRONETWORK. *Proceedings of Science*.

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