# 1 Reviewer 2

We thank Dr. Hawkins for the thought and time he invested in his comments, and his efforts to improve the article. At the outset, we can perhaps address two of his comments above.

1) regarding the HPol information, as noted in our response to reviewer #1, this has been addressed in four previous articles (DOI: 10.1103/PhysRevD.105.123012, DOI: 10.1140/epjc/s10052-023-11238-y, DOI: 10.1016/j.astropartphys.2022.102737, and DOI: 10.1017/aog.2020.35). The Jordan et al article specifically input the measured COF from Voigt, 2017 and then calculate the expected VPol/HPol time delays for a given SPD/ARA receiver geometry, finding reasonably good agreement with ARA observations of $\sim$20 ns time delays between VPol vs. HPol signals propagating cross-flow vs. co-flow, as also reported in Allison et al. The reanalysis of RICE data similarly found consistency between bistatic radar cross- and co-flow signal polarization propagation with the COF reported by Voigt et al.

2. The amplitude analysis is, well, difficult. There are some ice propagation effects that are not well-understood in the SPD data set, as reported in Allison et al (2020) and (in greater detail) in https://doi.org/10.1140/epjs/s11734-025-01504-3. Among the most prominent are i) despite the signal transmitter being VPol, for ARA station A1, for a range of signal/receiver geometries, the HPol signal strength exceeds that of VPol at a level that cannot be accommodated by a simple birefringent rotation model, ii) for a range of signal/receiver geometries, the ratio of the R signal amplitude can vary from factors of half to twice as much as the D signal amplitude; this is not predicted in any of the extant simulations, iii) the received signal frequency content for both VPol and HPol shows large variations that, again, are not accommodated by a simple ice permittivity model, and iv) as reported elsewhere, 'shadow zone' signals are observed, while they should be forbidden for a smoothly varying refractive index profile (according to Finite-Difference Time Domain simulations, introducing density fluctuations, which are known to be present in glacial ice, should lead to shadow zone signals). We can say with some confidence that there are unanticipated lensing and defocusing effects that we are still trying to parameterize. For this reason, the paper focuses on a measurement of the refractive index (real part of permittivity), which should be calculable simply from signal timing measurements. This is not to minimize the importance of the amplitude analysis, however, bringing that to completion will, we feel, require synthesizing results on long-baseline horizontal propagation from both ARA (which we have, per the SDP data presented here and in other publications) and also RNO-G at Summit Station, Greenland, which remains to be taken (with analysis following in the subsequent 2-3 years). In short, we feel that the timing analysis stands on its own; a comprehensive amplitude analysis, incorporating all available data (including that taken on-site at Summit in June, 2026), and showing agreement between simulation and data, is still TBD.

Our responses to individual comments are shown below.

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.

Thank you for this input, we had not considered the Looyenga model for converting between density and permittivity. Following the reviewer's comment, we've included mention of this conversion and a direct comparison plot of the SPICEcore density data to the timing n(z) models of the two conversion methods. The Looyenga model, in general, gives a poorer fit to our dt(D,R) data, as is evident from the now-included plots.

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.

Following the reviewer's recommendation, we've now adopted this naming convention, thanks.

Section 5.2: This section needs further discussion and expanding, using data from the figures.

Thank you; we have now added text describing the deviation between linear density - n(z) conversion in bubbly ice region

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.

We've combined the channels of each station as the Vpol channels local to each station are similar; they were previously only shown individually primarily for completeness. Station A5 was kept as a separate figure given its greater distance to the source and the larger geometric extent of the shadow zone, and we've now included discussion of this separately relative to the discussion of data and analysis for stations A3, A4.

There is no discussion about increased residual at small receiver depths - why is this? Why is -500m the upper limit for the SPUNK depth?

The residuals are discussed in lines 188-192, we added some additional description here. The 500 m upper limit is due to the overlaying of the direct and refracted signals as the timing differences get smaller, and are correspondingly increasingly overlapping and difficult for our double-pulser pattern recognition algorithms to distinguish (this happens when dt(D,R)<35-50 ns or so). Figure 6 shows these timing differences for station A2, which fall below 50 ns above 600 m 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.

Thanks, in light of some additional comments as well, we've moved this section briefly describing the 'shadow zone' to an earlier section describing the simulated ray paths to avoid disrupting the flow of the results. Introducing the shadow zone earlier also facilitates discussion of the single and dual stage shadow zone of station A5.

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.

As mentioned above, the amplitude analysis has been considerably more challenging than the timing analysis and thus far does not conform to a simple model (as expected from the Friis Equation, for example) of signal propagation. We have thus far investigated whether in-ice anisotropies (density fluctuations, and/or ice layers, e.g. producing waveguide effects) can reproduce the published 'anomalous' behavior although thus far a self-consistent picture has not yet emerged.

References: The references to glaciological literature are limited and the manuscript would be improved by including additional references throughout.

Thanks, we've added additional references to Brandt and Warren 1997, Lazzara et al. 2012, Looyenga 1965, Glen and Paren 1975 throughout Section 2, Allison et al 2019 in section 3, Oeyen et al 2022 in section 4, and Barwick et al 2018 in Section 5.

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.

Added the text "These experiments aim to detect neutrinos via radio waves emitted by Askaryan radiation after a neutrino collides with an ice molecule. That collision produces a subsequent in-ice 'shower' of particles, each of which travels super-luminally, with velocity greater than the local light velocity. As the shower develops over several meters in the ice, pre-existing atomic electrons are scattered into the forward-moving shower, which therefore acquires a negative charge excess. At wavelengths greater than the ~20 cm lateral scale of the forward-moving shower, the Cherenkov radiation emitted by the charge excess adds coherently, resulting in a signal amplification at radio wavelengths."

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 and hence increase the effective refractive index ().

Yes, true. We are assuming that ice is a sufficiently lossless medium that we can ignore this contribution. We have added a comment to that effect in the article

L42: Does this statement need to be in parentheses? It feels like a sensible caveat to have in the main text.

We've removed the parenthesis for this caveat.

L47: Is the symbol equivalent to the pore close-off depth used in other literature?

We've adjusted this symbol to $\rho_i$ to indicate the density of pure ice

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).

Thanks, we've replaced this with a table of parameters.

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?

Yes, perhaps, although we admit have not investigated this issue in detail. In principle, this could perhaps affect the density to n(z) conversion. Although the relative contributions of temperature vs. impurities to attenuation are discussed in the 2021 Summit Station attenuation length paper, we have not considered a systematic dependence of refractive index on temperature or moisture content

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.

Thank you, we've reformatted the legends and limited the density sets to include only the compilation necessary to cover the entire depth range, making discrimination easier. It is indeed true that South Pole ice from SPICE core is preferable, but the limited depth range in our available SPICE core data proved a barrier in applying a fitting algorithm for the firn to bubbly ice boundary to this data.

L93: As above, consider moving these parameters into a Table and present the equation symbolically.

Thank you, we've moved parameters of equations into Tables 1 and 2. We reference the form of Eqn. (2) for these tables, and removed the redundant Eqn (7).

Figure 2: Consider labelling horizontal [or combined horizontal and vertical distances from each A station to SDP to give a reference for the lateral and total distance traveled by a direct ray.

We've updated this plot with a version that labels station number by horizontal distance, thanks.

L107: There is not any discussion of the difference between H-pol and V-pol data.

The Hpol and Vpol channels were meant to be mentioned as part of a description of the ARA stations, we've rearranged the sections to make this (hopefully) less confusing. For these results, we look at Vpol channels since we are looking at waveforms from a Vpol transmitter.

Figure 3: Is this figure from this manuscript or has it been adapted from another source? If it has been adapted, provide a reference.

We've added a reference to a previous ARA paper that used this figure in the figure caption.

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?

Apologies, we were not clear in this description. We used an asymmetric Gaussian for the timing. We had also tried using a Landau fit in determining the peaks, but found the asymmetric Gaussian performed better at smaller dt(D,R), or shallower depths. We have not tracked down the reason for any shape differences between D and R, although the reviewer's hypothesis is very plausible and we have taken the liberty to include it in the text.

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.".

This figure shows the capture for one VPol channel (0) waveform. The SDP pulser is a vpol transmitter, so only VV is used in these results. Timing values have been added to the caption of the Figure, as well additional text per the reviewer's comment.

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.

We've updated and relabeled figure 5 to indicate a shaded region as a shadow zone for easier readability. We are hesitant to combine Figures 4 and 5 to avoid confusion, since Figure 4 shows actual data and Figure 5 is a simulation of ray paths for input Tx, Rx coordinates.

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).

In our modeling, we start with the Friis Equation, which should, in the absence of internal scattering, prescribe the expected signal strengths for the Direct and also Refracted signals (we note that none of the signals presented in the article are surface-reflected), knowing both the transmitter and receiver antenna signal gain as a function of elevation angle (measured in the KU anechoic chamber), the launch and receive angles (from ray tracing), the expected 1/r loss of signal amplitude with distance, with the path lengths $r_D$ and $r_R$ again determined from ray tracing and the integrated signal attenuation using the calculated attenuation length vs. depth profile at the South Pole. This information has been added to the paper (and, contrary to observation, invariably leads to an R signal strength smaller than D).

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.

We updated this line to use A2 as the example for both. Thanks for the input on consistent vocabulary, we've edited to consistently use horizontal distance and depth. For the shadow zone boundaries, we used radial distance to clarify that this boundary is actually azimuthally symmetric in all directions from the receiver antenna.

L151: Over the 5 km 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.

We agree with the concern of variation over horizontal distances in n(z) for shallow ice, where clear differences in density fluctuations have been observed, e.g., from neutron probe monitor measurements made by Morris and Hawley around Summit (our group has also taken gravimetric data in June, 2026 that similarly shows evidence for this). The lateral coherence length for n(z) at 5 km scales at South Pole, as far as we know, has not been quantified, although if the reviewer is aware of data that measures this, we would be happy to include it. The 2022 and 2023 UTIG radar sounding in the vicinity of South Pole clearly shows internal layer isochrones varying on this length scale, although (as far as we know) this has not yet been translated into refractive index. Note that, for our analysis, rather than independently fit each station's dt(D,R) distribution and therefore extract four different refractive index profiles, we have instead tried to minimize the number of free parameters in our model, by fitting the model parameters to data from only one station (A2), and then showing that those parameters are favored (modulo uncalculated local variations) for the other stations, as well.

Figure 6: What is "SPUNK" depth? This isn't defined in the manuscript.

We've replaced this labeling with "SDP depth" to better match the terminology in the text, thanks.

L166: This needs more discussion - your three-stage model results in an increased value of n(z) at 160 m 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. 1300 m or so), then a key difference between this three-stage model and the others is that it results in an increased overall density.

The n(z) models do all converge to the same value at greater depths (1.774-1.78) by how they are defined, and the reviewer is correct that the implied density at the depths corresponding to the 'top' layer of antennas is increased. Note that we've also changed this section somewhat by including the Looyenga model for comparison as well (which actually gives a poorer fit to our data.)

Figure 7: There is an increased n(z) with one-stage and two-stage models up to 100 m which will increase travel times, whereas there is an increased n(z) > 100 m for three-stage. If you consider a vertical (or angled) direct

ray path and integrated to covert to travel time, how to these effects compare (i.e. does the increased n(z) < 100 m for one-stage and two-stage compensate the increased n(z) for > 100 m for three-stage?

For a typical direct ray path from SPICE to one of the ARA stations, the ray traced travel time does increase for the triple stage model compared to the single or dual stage n(z) models, due to the higher n(z) before reaching asympyotic n=1.78. For the example A2 signal shown in Figure 4, ray tracing indicates an increased direct travel time of approximately 30ns. These absolute travel times however are not recorded in the measured data, since we do not have an absolute reference $t_0$.

What is RICE IoR measured - not defined in text?

We've included a reference to the RICE index of refraction (IoR) paper in the figure description in the text.

Consider extending depth to >170m so that n(z) can be compared at depths relative to the receiver depths described later on.

As far as we understand, the reviewer is requesting density data at depths greater than 170 m. Unfortunately, we are not aware of (or do not have access to) data at those greater depths (nor did we find such a data set in the SUMUp compilation).

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.

Thanks, we've included the $\Delta$ dt(D,R) definition in this line.

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?

We've combined these individual channels to a plot for each station, since channels of the same station cover similar ice and produce similar results. We interepret the variation between channels as a measure of the systematic error inherent in receiver uncertainties (including individual channel surveying errors)

L174: This isn't obvious without a clear definition of dt(D,R) { consider an equation.

Thanks, we've added a (hopefully) clearer definition of dt(D,R) when it is introduced in section 3, as well as for Delta dt(D,R) when it is subsequently introduced

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.
We've edited the overall discussion in this section, including more discussion of the shallow dt(D,R) near the shadow zone for all stations. The 500 m limit, as mentioned above, arises primarily from our software's inability to resolve and distinguish D from R

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.
Thanks, in section 6.3 and the conclusion, we've now placed more emphasis on the triple stage model's better performance at SDP depths for which the single and dual stage models would predict shadowing at the ARA stations

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?
Sorry for the misleading phraseology here - D and R are temporally separated at all depths, except at or near the shadow zone boundary, so interference effects between the two are generally excluded for the impulsive signals produced by the SDP. For continuous wave sources, of course, the reviewer is correct that interference effects could be significant, although those are not considered here.

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.
We've added the dual stage SZB to the SZB plot as well.

L195: By how much? This is shown in figure 12 but not described in the text - how does it vary with radial distance etc.
Thanks, we've edited the text of section 7 to include a better description of the shadow zone behavior. We've added additional information to the SZB plot as well and collapsed to a single plot on the 170m depth receiver typical of an ARA station antenna.

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.
We've added some clarification that these are the energy ranges to which neutrino experiments such as ARA and RNO-G are most sensitive.

L202: I'm not sure this is a 'conclusion' of the manuscript as presented, and instead better suited to discussion of future work.

Thanks, this line has been removed from the conclusion, and the text in the conclusions expanded, overall.

Typing comments:

L71: avoid use of forward-slash in the main text.

Thanks, we've replaced this forward-slash usage with "snow to firn" and "firn to bubbly ice".

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".

Thanks we've updated the phrasing as suggested, stating that the pulse is received by Vpol antennas. We've also clarified in the introduction of the SDP pulser that it is a vertically polarized transmitter to avoid confusion about the selection of vpol channels.

L145: Mix of units - 1-5 km and them 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.

Thanks, since we've added information such as the station distances to section 2.2 at the advice of other comments, these lines have been removed.

L165: Is there a missing space before > symbol?

Added a space here, thanks.

L191: Consider renumbering Section 7 as Section 6.1.

At the suggestion of another reviewer, we introduce shadowed zone in an earlier discussion, and collapse this to one section.