We thank the reviewer for offering these comments, which highlight several points (general and specific) that we overlooked. We hope that we have adequately addressed the reviewer's concerns and they feel that the manuscript is improved. Responses to the individual comments are presented below.

Review of: Modeling the refractive index profile n(z) of polar ice for ultra-high energy neutrino experiments

General Comments

Comment 1: At present I feel that it is difficult to get a clear grasp on the geometry/operating parameters of the receivers, the SPD transmit antenna, and calibration antennas. I think it would be useful to have a more detailed description of the entire array, the types of measurements taken (both with calibration antennas and the SPD transmit antenna), description of the SPICE borehole, as well as some of the dimensions upfront in section 2.2 Experimental Layout. As it stands this section is very brief and I think a lot of the pertinent survey geometries details (for instance frequency range, acquisition depths, antennas spacings) seem to be added sporadically throughout the next few subsections. Overall, I think the content of this manuscript will be of interest to the wider glaciological community, but description of the experiment should be a bit more accessible as this is a relatively unique setup.

We apologize for the lack of clarity here and, in retrospect, agree that we have overlooked the intended audience. To hopefully, address this, we have moved information from later sections to 2.2 (Experimental Layout), as suggested, as well as provided additional station layout data in this section. We also included a reference to previous work (Allison et al. 2016) in section 2.2 that includes a more detailed description of station design and measurements taken by the calibration antennas. The South Pole Ice Core Experiment pulsing campaign conducted in December, 2018 is detailed in the extensive (Allison et al 2020); we have more explicitly pointed the interested reader to that reference, but, per the reviewer's comments, have also provided additional details in 2.2

Comment 2: I think the authors could also consider combining Figures 2 and 3 to create one singular survey geometry figure, again, to centralize some of this information. Similarly, I think Figures 4 and 5 could be combined, highlighting the concept of the D and R travel paths.

We prefer to keep Figures 2 and 3 distinct, as they depict incompatible scales, since the overall layout is spread out over distances order km, while individual station components are located order 10 m. For clarity, we've added a top down scale view of station A2 to Figure 3. We similarly feel Figures 4 and 5 should remain separate because Figure 4 shows waveforms of actual SDP data, while Figure 5 shows simulated paths: we would like to keep this distinction clean. We updated Figure 5 simulated ray paths to match the channel and depth shown in Figure 4.

Comment 3: From what I understand the main analysis was performed using the VPol channels. Since the array also contains HPol channels has this data also been used to investigate the refractive index profiles? Any comments on anisotropy? Although it is possible the ice in this area does not have a preferred crystal orientation.

Since the signal-to-noise ratio is so much greater for the VPol channels, we have limited ourselves to VV configurations. VH is, in principle, possible, but, owing to the weakness of the signal, that signal was buried in the noise for the most distant station (A5), which also provides the strongest constraint on c3. As the reviewer suggests, these data were explicitly analyzed several years ago for wavespeed anisotropy dependence on propagation/polarization relative to the (known) local iceflow; a model was constructed that inputs the crystal orientation fabric, as a function of depth, as measured from the SPICE core, and then calculates an expected polarization-dependent propagation time for ARA's Hpol vs. Vpol channels, as observed in stations A1, A4, A2 and A3 (relevant articles are DOI: 10.1103/PhysRevD.105.123012, DOI: 10.1140/epjc/s10052- DOI: 10.1016/j.astropartphys.2022.102737, and DOI: 10.1017/aog.2020.35). As mentioned, in this case, since the transmitter lowered into SPICE for this particular analysis was VPol, the observed received signal strength was typically several times larger for VPol than HPol, so the focus here was on Vpol channels. We investigated the possible effect of anisotropy found in other analyses of this ice, and found the effect on dt(D,R) timing differences to be minor, as expected, since both D and R signals are oriented at the same azimuth relative to the local ice flow direction.

Comment 4: It seems to me that the discussion in section 6 'Shadowed Zone' could be moved earlier in the paper. Especially since it references Figure 5 (and kind of answers one of my specific comments below). In general this section breaks the flow that would otherwise exist between Section 5 and Section 7.

Thank you for this comment. In resonse, we moved the discussion of 'Shadowed Zone' to directly follow section 4 on Simulations, as the shadow zone distances are determined by these simulations for a given n(z) model.

Comment 5: The abstract mentions: "Effective volume simulations for a detector of ARA station antenna depths yield a 14 % increase in neutrino sensitivity over a neutrino energy range of 1018-1021 eV using the three-phase model compared to the single exponential."

But I don't see this discussed explicitly anywhere in the main text of the manuscript? I assume there should be some discussion of this in Section 7?

This is referring to the effective volume comparisons shown in Figure 12, we added to the corresponding section of text in section 7 to explicitly discuss this, thanks.

Specific Comments

Lines 28-32: I think it is worth explicitly stating that the dielectric constant of ice typically increases with depth, resulting in a decreasing radio wave velocity profile which is specifically the cause of the shadow zones for shallow receivers you mention.

Thanks, we added a clarifying statement that the shadowed zone specifically results from dielectric permittivity increasing with depth.

Lines 42-44: This statement probably does not need to be a parenthetical.

We removed the parenthesis around this statement.

Lines 46-48: The parameter b0 is not defined on its own here (it is just wrapped up in the definition of the surface snow density). Even if this is just an empirical constant it should be mentioned. Also, even though it is obvious, z is also not defined after this equation.

Thanks, we've edited this to explicitly define b0 and z

Line 108-109: "Nearby (30–50 m away) englacial pulsers are used to calibrate channel positions." This seems a bit breezed over and it isn't clear to me that the authors go into much detail later on about how these calibration antennas are used other than to say "nearby calibration pulsers only test n(z) over the limited depth range of the deployed ARA antennas" in Lines 116-117. Is the calibration process critical for the analysis? If so, maybe describe a bit more in detail.

Following the reviewer's comment, we've added a reference to a study that discusses these calibration pulsers here. To more directly address the reviewer's comment, in terms of this analysis, the local calibration pulsers provide more limited constraints on n(z) since: 1) the relative timing from the cal pulser transmitter to the receiver stations depends not only on the antenna positions and n(z), but also the channel-to-channel cable delays (dt(D,R) is independent of cable delays), and 2) the local cal pulsers provide only one source point, whereas the SDP sample a 1-km chord of source depths. The slope of the channel-by-channel dt(D,R) distribution with depth (flat, negative or positive) also provides information on antenna surveying errors, with refractive index uncertainties entering in second order. As an example, the dt(D,R) distributions were used internally to identify a 2-meter miscalibration of one of the ARA strings in stations A4 and A2; this was undetected using only local cal pulser data. Those comments are now made explicit in the main text.

Figure 5 caption: "Shadow zone refers to the area above and to the right of the dashed blue line where pulses cannot be detected." I'm having a little trouble understanding this part. I get that the deep pulser is at -1300 m, but I don't quite understand the shadow zone area. For instance if there were a receiver at (-200, 2000) you would still see a path from the transmitter at -1300 m. I guess being more explicit about what this dashed line indicating the shadow zone represents would be helpful. Do you mean that if the transmitter is above this line there would be no R path?

We've rewritten this caption to clarify that the boundary is where transmitter locations do not have a ray path to the receiver location. In simulation, there exists neither D or R paths from transmitters above this line to the receiver due to ray bending in the ice. (In practice, there exists signal past the shadow zone boundary at a lower amplitude, although this is forbidden for a smooth, monotonically varying n(z) profile). Put another way, if one imagines a stationary receiver at depth $z_0$ and imagine a receiver at the same depth $z_0$ at a separation distance of d=1 meter, as one moves the transmitter laterally away from the receiver, dt(D,R) decreases to zero at a lateral distance of $d_{SZB}$. Lateral distances $d > d_{SZB}$ are shadowed, with no classically-allowed ray path from transmitter to receiver.

Figure 6: the x-axis says 'SPUNK' depth. Should this be SPD? But maybe I'm missing something.

The SPUNK (our local naming convention) depth does refer to the SDP depth, thank you for this correction.

Line 169-170: would be nice to explicitly state upfront that $\Delta$dt(D,R) is the difference between the measured and simulated values of dt(D,R)

We've added this definition to the line introducing the term $\Delta$ dt(D,R), thanks.

Line 193-195: Is it possible the references to Figures 11 and 12 aren't quite right? For instance, "Figure 12 compares effective volume simulations using the two ice models for neutrino detector stations with 170 m and 100 m deep antenna deployments" but don't these plots in Figure 12 only consider the 170 m depth case? Also when discussing Figure 11 in the earlier lines, it seems only the left panel is mentioned (ie, the 100 m case)? Considering this part of the analysis seems to be one of the main motivations for characterizing the refractive index profile, I think more time can be spent making sure the discussion of the figures is clearer.

Thank you for identifying this, there were some previous edits that resulted in some incorrect referencing here. To provide some context for these numbers, 170 m is the approximate maximum depth of the firn at the South Pole (location of the Askaryan Radio Array), while 100 m is the approximate maximum depth of the firn at Summit Station, Greenland (location of the Radio Neutrino Observatory-Gree these are the two extant pre-eminent radio neutrino detectors. We've corrected and cleaned this up, as well as added text hopefully elucidating the 14% increase seen for energies above $10^{18}$ eV in Figure 12.