Reply to review report by David Lilien submitted on the 9th of January 2023

The IceCube Collaboration

March 7, 2023

> Review of "In-situ estimation of ice crystal properties at the South Pole using LED calibration data from the IceCube Neutrino Observatory" by Abbasi et al.
> This paper presents observations of an anisotropic effect in the calibration data from the IceCube detector near South Pole. Photons are deflected from paths that would be expected from scattering and absorption alone. Previous work has shown that this effect can only be approximated poorly using anisotropic absorption and scattering, and these effects are not on firm physical grounds anyway. In the present work, the authors consider the effect of birefringence. They essentially run simulations of the propagation of light (both ordinary and extraordinary waves) through 1000 crystals, and look at the effect of refraction and reflection as a function of the crystal shape and orientation. From this, they parameterize a relatively simple function of how the birefringence affects propagation, and include that function in the original model of absorption and scattering. They find a much better fit to observations, although this fit is still improved with non-physical anisotropic scattering/absorption effects. In addition to describing this work, there is a lot of history and background of IceCube and of other attempts to model the observations.
>
> I must admit that it has been a long time since I had to deal with derivations directly from Maxwell's equations such as those presented in the paper. While I was fully able to follow the arguments and derivation, I do not think I would have been able to spot an error; hopefully other reviewer(s) have that knowledge. For the portions that I can evaluate, I think the work is nearly publishable (with the exception of the second general comment), though the presentation could use significant improvement to be really digestible. I do not object to long papers, but only when it is justified by the content; here I think the paper needs to be shortened so that the point is not lost in all the other material (see first general comment). I think this will be a very nice contribution when these issues are addressed—the observations are fascinating, and I think the explanation is compelling. While there may not be wide applications, this paper describes very basic information about the properties of ice, and thus deserves to be published.

We would like to sincerely thank the reviewer for the significant time investment in reviewing the manuscript and the constructive and encouraging feedback.

1 General comments:

> The paper is very well written at the sentence level, but the larger structure leaves gaps at some points and provides overly much detail at others. I think a large part of the issue could be alleviated with a more normal paper structure describing a problem and the work to address it rather than the meandering path of the last 10 years (more on this in the next paragraph).

The paper is certainly not intended as a historical review and a number of proposed cuts to address this appearance will be outlined below. Yet given we rarely publish in glaciological journals (the last instance being Ackermann2006) and that a large fraction of the target audience is likely neither familiar with ice optical properties nor the IceCube detector, we find a short summary of all aspects required to perform this kind of measurement is pertinent. Otherwise the reader is left to explore a large body of work in an unfamiliar field before being able to study the novel aspects of this manuscript.

The different gaps in explanations, alternative aspects to be added and areas of excessive details identified by the two reviewers highlight that arriving at a fully concise manuscript is futile given the diametral demands, but we hope to strike a balance here.

> One example of a gap is how this work fits into the context of known birefringent effects in ice (I'm thinking of birefringence at frequencies used for radar); this is addressed in Section 5.3, but that is an odd place for the reader to get the context—it would be much more at home in the introduction.

We struggle to move the entire section, as it does require knowledge about the diffusion mechanism and its relevant parameters, which is only provided in the previous section. We thus propose to leave the section as is and hope that the changes proposed to the introductory section (see two comments from here) are sufficient to give the added context.

> As another example, Section 6 jumps in with no intro—I assume that it is there because it is computationally necessary not to explicitly model the birefringence effects in individual crystals during the simulations, but it would help if that were stated clearly.

Good catch. The reason is in fact as assumed. Using the exact electromagnetics simulation (section 5.2) directly would slow down the overall photon propagation simulation by orders of magnitude, while the evaluation of the parametrized model only adds an insignificant burden. We will add as much.

> In terms of excess details, this is an extremely long manuscript; it can take a really long time to get to the explanation of how the pieces fit together, by which point the reader is already lost. For example, section (3) describing what is essentially an isotropic model of the optical properties of the detector, is not really motivated and so comes as a distraction/reads as history until much later when I understood how the parameterization from the anisotropic model was then used in the isotropic one.

We propose the following cuts:

- Equations 2, 3, 4 and 5 shall be cut along with the accompanying text and replaced with more conceptual descriptions.
- Figures 2 and 16 shall be cut without replacement.
- Lines 234 to 240 shall be cut without replacement.

- Section 4.3 (Early empirical modeling) shall be replaced, removing most details regarding the mathematical modeling. The suggested new paragraph can be found in response to a detailed comment regarding this section.

In addition, we propose to expand lines 67-74 (describing the structure of the manuscripts) as follows:

> This manuscript has the following structure: Section 2 introduces the IceCube Neutrino Observatory (Sections 2.1 and 2.2) and how it employs ice as a detection medium (Section 2.3). Section 3 describes the properties of the LED calibration data used in this study (Section 3.1), explains the photon propagation software used to generate simulated data (Section 3.2) and details the likelihood analysis comparing simulated to experimental data in order to infer ice properties (Section 3.3). Section 3.4 briefly reviews the state of the isotropic, layered model used to describe the ice optical properties prior to this work. The experimental signature of the ice optical anisotropy (Section 4.1) as well as early modeling attempts (Section 4.3) are summarized in section 4. The newly developed model to account for the ice optical anisotropy based on the ice-intrinsic birefringence is described starting with Section 5. Sections 5.1 and 5.2 explain the electromagnetic theory governing the birefringence in polycrystals, while Section 5.3 introduces a software package to simulate the resulting diffusion patterns. Section 5.4 compares the experimental signatures and conceptual understanding of the underlying optics to birefringence observations in radar sounding, a field most readers are probably more familiar with. Section 6 explains how the diffusion patterns are applied in the IceCube photon propagation simulation (Sections 6.1 and 6.2) and how crystal properties have been inferred (Section 6.3). Section 7 describes the resulting ice optical model. Section 8 discusses shortcomings of the model as well as future measurements in upcoming IceCube extensions and through drill-hole logging.

While this adds some additional text, we hope that this additional context allows the reader to judge which parts of the manuscript are relevant for their particular interests/expertise.

> Some of the figures, which appear to be reproduced from elsewhere and are not particularly necessary for the present work, could be cut to streamline things; in my view, Figures 2, 3, 4, and 16 should all be cut.

We are happy to cut Figures 2 and 16. Figure 4, we think necessary to explain the nature of the used data (arrival time distributions of individual photons) and the matching simulation. Figure 3, while not vital, gives a sense of scale and completeness/complexity of the available data which we think is helpful to the unfamiliar reader. With the detector fully embedded in the ice and with each of the 60'000+ LEDs being observed by hundreds of sensors each volume of ice is tested repeatedly and from a multitude of illumination directions.

> At a number of places, avenues that were pursued but proved fruitless are described in great detail—I would suggest cutting these down for readability.

As detailed during the specific comments we propose to significantly shorten section 4.3 (Early empirical modeling).

> The paper would benefit greatly from a more clear description of the observations that imply anisotropy early in the paper (like we get along with figure 8), and a description of what the goal of the paper is. The introduction perhaps attempts to do this, but it reads more like a history of IceCube optical modeling than a problem statement–perhaps this was the goal, but it appears that both the other reviewer and I found this to be challenging to read as-is. Much of the second half of the intro in some way gives what the structure of the paper will be (e.g. describing the isotropic model fitting, etc.), but it was phrased in such a way that it was unclear that these would be expanded upon and would be critical components of the present work. In my view, the abstract does an excellent job of presenting what the paper is about, but the structure of the rest of the paper then does not follow the outline laid out in the abstract. For example, it would be very helpful for the reader to know that you are going to develop a grain-resolving anisotropic optical model, from that parameterize a diffusion function, and then input that into the previous ice model.

This is a valuable comment and we suggest the following modifications to the Introduction section:

- A reference to Figure 6 shall be added to line 42 and a reference to Figure 8 shall be added to line 46.
- We propose to change the structure of the paragraph starting on line 47 as follows:

    First attempts to attribute the observed effect not to Mie scattering but to the ice intrinsic birefringence have been made by Chirkin and Rongen (2020). Here the optical anisotropy results from the cumulative diffusion that a beam of light experiences as it is refracted or reflected on many grain boundary crossings in a birefringent polycrystal with a preferential c-axis distribution. The wavelength of $\sim 400\,\mathrm{nm}$ employed in the IceCube calibration studies is significantly smaller than the average grain size, which is expected to be on the millimeter scale. Thus, grain boundary spacings and orientations must be accounted for in addition to the fabric, making the effect challenging to derive from first principles.

- To clarify on the goal of the paper the following sentence (inspired by the wording of the reviewer) shall be added in line 58.:

    Due to computational limitations, a grain-resolving anisotropic optical model is parametrized using diffusion functions. These function in turn are applied as an extension to the existing, homogenious ice optical simulation. The new simulations, assuming different ice crystal realizations, are then compared to LED flasher data, which allows to partially constrain the crystal fabric, size and elongation.

- As detailed earlier we also propose to expand the second half of the intro, so that the reader is clear about the interplay of the sections and which sections may be most relevant to their interests.

> 2. Overall, I think the consideration of possible fabrics is a bit simplistic — this is fine considering the computational expense, but as of now some statements are simply incorrect. For example in Appendix A, the authors write "Woodcock (1977) realized that all possible fabric states can be visualized in a 2D plot," which is not at all true—only a rotated form of the second order fabric can be visualized in this way. In addition to correcting mistakes like this, we need some consideration of the implications. The question that I am particularly interested in is whether the higher-order moments of the fabric have any effect on the process of interest here; in radar, only the second-order moments control the effects, but I am not sure that the arguments carry over. For example, do circumpolar hoop fabrics and other complicated fabrics which are observed in glacial ice (see, e.g. the Faria paper cited in the text for examples), behave identically to single maxima in terms of this birefringence, or do they produce some other effect? If the latter or if it cannot be determined, then the text should make clear that what was considered was only a subset of the fabrics that have a give pair of Woodcock parameters.

Aside from computational limitations, the simplifications were primarily chosen to coincide with the fabrics to be expected at our study location. The most simple starting point and ice realization at shallow depths is a uniform fabric, below ∼1200 m ice at the geographic South Pole features a very strong and clean girdle (https://www.usap-dc.org/view/dataset/601057) and in the deepest ice one would expect this to turn unimodal. These three scenarios can nicely be interpolated between using the plane spanned by the two Woodcock parameters.

We will make sure to include the reasoning above in the introduction and section 5.2. Plain errors such as in the appendix will of course also be fixed (as detailed in the specific comments below).

Regarding more complex fabrics, such as circumpolar hoops, we do share the reviewers suspicion that they will affect the diffusion patterns in yet unexpected/unexplored ways. But we have so-far not simulated such cases. (Partially because that would require a new sampling scheme similar to the one presented in appendix A.) It is also worth pointing out that we found the fabric to be a subdominant contribution to the diffusion pattern, with the elongation being of primary importance. There is of course a physical link between fabric and crystal shape, but without a quantitative model linking them, we are currently left to treating them as independent parameters.

> 3. We need a better description of how the layering fits into the modeling. I think my confusion stems from a difference in how I think of layers (generally packets of ice deposited at the same time or radar reflections depending on context) and how this paper uses layers (as best I can tell, these are packets at specific depths, but I am unclear how they vary spatially and what the "IceCube coordinate system is"). This ambiguity clouds the results to a certain extent—for example, is a girdle in this coordinate system truly a vertical girdle, or is it tilted by the layer slope? While I list this as a major comment, it is only because I think it is important to address, not because it requires a lot of work—just a clear description of layers at line 148 when then first come up would satisfy me (section 3.4.1 comes late). In addition, this paragraph should make clear the extent to which the tilt is included in the model compared to being a source of error.

In this work "layers" also refers to "packets of ice deposited at the same time" and exhibiting equal optical properties. It's just that due to the resolution limitations we only consider ice properties averaged over ten meters, instead of studying for example annual layers. This is similar to the depth resolution in radar measurements. These layers are not assumed to be at the same absolute depth everywhere in the detector. Instead, their absolute depth is given at a reference location, while the absolute depth at any other location requires knowledge about the tilt / layer undulations as detailed in section 3.4.1.

To avoid confusion we propose to change the description at line 148 as follows:

> The detailed stratigraphy associated with the yearly layering cannot be constrained through IceCube data, nor is it needed in order to accurately describe the photon propagation over large distances exceeding tens of meters. Instead, average properties in 10 m depth increments, here called "ice layers", are being considered. The absolute depths of these layers as for example shown in Figure 5 are referenced to a location in the center of the lateral footprint of the detector. At any other location in the detector the same layer are found at slightly different depths following the layer undulations as will be described in section 3.4.1.

We acknowledge that the use of a technical/ slang term such as "IceCube coordinate system" is unfavorable in a paper. The term appears twice, once when converting absolute depth readings in SpiceCore to the IceCube stratigraphy and once when denoting the ice flow / anisotropy direction and primarily serves as internal reference. We propose to leave the appearance in line 302 as is, as an explanation is already present there, and reword the appearance in line 460 to:

> It reached a final depth of 1751m (Winski et al., 2019)), which corresponds to a depth of 1820m in the IceCube ice model (see Figure 5) accounting for the layer undulation between the two reference points.

Regarding the orientation of the girdle, it is worth noting that the diffusion patterns are simulated separate of the photon propagation framework and only their parametrizations are applied during photon propagation. This allows to orient the girdle arbitrarily and independently of the layer undulations. Here the parametrizations are evaluated such that the girdle normal vector is assumed to be perfectly horizontal, with no assumed correlation to the layer undulations.

2 Specific comments and technical corrections:

> 20-21: The sentence ending on line 20 and the one beginning there are both incorrect. While ice is indeed mechanically anisotropic, and bulk anisotropy results from anisotropy of the grains, the development of fabric is not related to this anisotropy in such a simple way. Sometimes fabrics orient favorably to the strain direction, but for some of the most common fabrics observed in ice sheets the opposite is true; beneath divides, where the stress state is uniaxial compression in the vertical, vertical single maxima form, but compression is thought to be harder parallel to the c axis than perpendicular to it. This statement perhaps belies a misunderstanding of the multiple processes contributing to fabric development, among which on migration recrystallization is thought to have the effect described here—and migration recrystallization does not dominate fabric development everywhere (or even most places). Moreover, it is unclear what "c-axes orthogonal to the strain" means, given that strain can act in multiple directions—in the case of compression, as already mentioned, the c-axes tend to orient parallel to the direction of maximum compression.

The short introductory summary given in these two sentences was indeed very much constructed to fit the girdle fabric scenario as encountered in the ice instrumented by IceCube. To avoid generally incorrect statements we propose the following alternative introduction and would be grateful for feedback:

> As a hexagonal crystal it will most readily deform as shear is applied orthogonal to the c-axis (crystal symmetry axis, normal to the hexagonal basal planes), leading to slip of the individual basal planes (McConnel, 1891). In polycrystalline ice the crystals effectively re-organize themselves to minimize the stored strain energy, resulting in non-isotropic / preferential

c-axes distributions and a bulk anisotropic viscosity (Faria et al., 2014b). The effects of recrystallization are experimentally most commonly observed as a crystal orientation fabric through the use of polarized light microscopy on thin sections of ice core samples (Alley, 1988; Wilson et al., 2003). In this work we only consider scenarios where c-axes are distributed isotropically (uniform fabric), are aligned in a single direction (unimodal fabric) or lie in a plane (girdle fabric). The later is of primary importance for the studied ice.

> 48: The wavelength belongs in the previous paragraph describing the observation

We would like to keep the quantitative number here (to contrast to the average grain size), but appreciate the point that the wavelength is a vital characteristic of the observation and will also add it in line 41.

> 82-82: "in addition to photons...mostly photons" is very confusing

We don't understand this comment. "Mostly photons" does not appear in this paragraph. Maybe *protons* was misread as *photons*?

> 147: "Ice layers" asks for confusion considering that the ice physically has layers that can exist on similar spatial scales. The terminology should distinguish between these and annual or radar layers. Slices? Packets?

Please see the response to general comment 3.

> 167: This section seems mis-titled, or at a minimum the title is not helpful. As I see it, this section just describes inference of isotropic properties of ice as an optical medium—so why not say that?

The section is meant to introduce the available data (*3.1 LED calibration data*) and means to analyze it (*3.2 Photon propagation simulation* and *3.3 Likelihood analysis*) which are common to the isotropic ice properties summarized in *3.4 The South Pole Ice Model (SPICE)* as well as the inference of anisotropic properties later in the manuscript. "IceCube as a laboratory for glaciology" may still be too sensational of a title and we propose to change it to "Deriving ice optical properties from LED calibration data".

> 170: I do not see why this should be true; e.g. for absorption there is no requirement to measure exactly the e-folding distance, rather than calculating it from the absorption over a shorter distance

While this is generally true, the distance scales involved here do not make this a viable measurement. Consider for example a full 1 m ice core segment with a representative absorption length of 200 m. Over this distance only $1 - \exp(-1/200) = 0.5\%$ of the light is absorbed, which is very challenging to resolve, taking into account experimental uncertainties (primarily coupling of the light source and sensor to the ice core). To our knowledge no such measurement has ever (successfully) been performed.

> Figure 2: is this relevant? For this paper, simply saying that you have a photomultplier, LEDs, and associated electronics seems sufficient

Agreed. This Figure will be removed.

> 184: Ice realization could use more explanation. There is more detail below, but here perhaps just “hypothesized optical properties and ice-crystal orientations”

We will adopt this wording.

> 234: I find the idea of an “error rate” to be confusing here. Does this mean there is one incident photon not related to the LED every 2 ms?

The ”noise rate” refers to electrical signals at the PMT output which are indistinguishable from the signal of a single photon hitting the PMT although the sensor is in total darkness. 500 Hz, so indeed one on average every 2 ms, may sound devastating, but since we are only considering a signal window of 1.5 us, the probability of a single photon like noise signal occurring within the signal window of each LED flash is smaller than 1/1000.
Since the sentence seems to be causing confusion and the noise modeling isn’t actually important to know about here, we propose to just drop the sentence.

> 270: Layers here are still poorly defined. I suggest describing them as annual layers and noting that radar reflections result from contrasts in dielectric properties, which are generally assumed to be isochronous (although not necessarily annual).

We propose to change the sentence from
One relevant complication is the layer undulation over the footprint of the array.
to
One relevant complication are the undulations of layers of equal optical properties over the footprint of the array.

> 281: This feels incomplete: I am left unclear as to whether any effect of this tilt is included in the modeling. We need a description of whether this fit into anything above and a preview of where it will become relevant below.

The tilt is included in the modeling/simulation used for this work, as it is required to achieve a reasonable data-simulation agreement even before considering the anisotropy.
Tilt as implemented accounts for the depth shifts of isochrons at different lateral locations in the detector, but does not actually account for the layers being sloped. But we did check that this can not induce an anisotropy like effect.

The tilt is here primarily introduced as it is a topographically related effect (the ice flows down the valley, while the layers curve up the hill slopes) and makes referencing directions easier. To avoid confusion we will add a mention that in this work tilt is included as described in Aartsen2013.

There is one technical complication which is mentioned but not elaborated upon. Fitting the depth-dependent crystal size is, for primarily technical reasons, done in tilt-corrected depth instead of absolute depth. This likely has only a small effect and it is (at least to us) not clear which of the two depths dictates the crystal properties to start with. We will add a sentence on this in the outlook section.

> 286: Rather than naively, which is vague, just state the assumption (i.e. that the transmission medium has only isotropic dielectric properties).

Yes this should be specified. Since in the optical regime pure ice was thus far considered optically perfect the dielectric properties were not foremost on our mind. We propose the following wording

instead:
Given the optical modelling discussed so far, the amount of light...

> 291: repetitious

Indeed. We propose to simplify the sentence from
This ice optical anisotropy was first seen in 2013 (Chirkin, 2013d), and is here called the ice optical anisotropy.
to
This ice optical anisotropy was first discussed in 2013 (Chirkin, 2013d).

> 310: The elevation angle is not difficult to obtain from thin sections on an ice core; the full 3d orientation of each individual c-axis is automatically analyzed. Perhaps this should say that the tilt axis is difficult to measure in an ice core, due to the uncertain orientation of the core.

Agreed. This is an important distinction. We accordingly propose to change the sentence from
As this is a parameter which is hard to accurately obtain from ice core data, it may be further investigated in the future.
to
As this is a parameter that is difficult to obtain from ice cores as their in-situ orientation is often not retained, it may be further investigated in the future.

> Section 4.3: This is almost all repetition of published work, and the level of detail is unecessary—it should be sufficient to show the curves in Figure 8, point out that a factor of 11 is unreasonable for the absorption anisotropy, and say that the fit was mediocre when only modifying the directional scattering and absorption.

Agreed. This is a good place to shorten the manuscript, without loosing context. Please find a suggested update to section 4.3 below (getting rid of all math details and already saving a full page):

Following the paradigm that ice optical properties are driven by Mie scattering on impurities, early attempts tried to model the anisotropy through directional modifications of absorption and scattering. In the original parameterization presented by Chirkin(2013d), it was argued that due to time and space reversal symmetries the absorption length and geometric scattering length cannot be direction dependent. Therefore the anisotropy was implemented as a modification to the scattering function, the only remaining Mie scattering parameter. This effectively results in a change of the effective scattering coefficient as a function of the propagation direction. Photons propagating along the flow axis experience less scattering than photons propagating along the tilt axis or inclined from the horizontal.
While not derived from first-principle Mie calculations, the parametrization was justified to be a plausible result of elongated impurities becoming preferentially aligned by the flow and thus introducing a direction dependence to the scattering function. While several glaciological studies (Potenza et al., 2016; Simonsen et al., 2018; Gebhart, 1991) explore the shapes of impurities, elongations for different impurities are not well established, nor is there to our knowledge any evidence for elongated impurities becoming oriented with the flow.
An evaluation of the data-simulation agreement is shown in Figure 8. It shows summed photon arrival time distributions for all nearest emitter-receiver pairs, roughly aligned along and perpendicular to the ice flow for a variety of anisotropy models and the employed flasher data. The scattering-based anisotropy model results in more intensity being observed along the flow axis. However, there remains substantial disagreement between the model and the observed

> data. As scattering is reduced in the flow direction light arrives earlier on average. The resulting change in the rising edge position is strongly penalized in the fit and limits the amount of intensity that can be recovered. To reduce the shift of the rising edge, a directional modification to Mie absorption was considered as an alternative by Rongen (2019). A factor 11 modulation of the absorption coefficient was required to fit the data, which seems unphysical. As evident from Figure 8, this model results in a delayed rising edge for propagation along the flow direction as desired, and did result in an improved data description compared to the scattering based model described earlier, but is also unable to fully match the intensity difference to data.
> To conclude, while resulting in partially successful effective descriptions, directional modifications to Mie scattering or absorption cannot reproduce observations nor are such modifications well motivated on first principles.

> 318: unclear if this applies only to Chirkin 2013d or the present work as well-defined

This modification of the scattering function is unique to the model developed in Chirkin 2013d and not part of the birefringence modelling proposed here. As the new section 4.3 (see above) does not go into this detail, we do not see any further action necessary.

> 385: Add a reference to Petrenko and Whitworth here

Will do.

> 454: Usually the glaciological literature refers to a single Woodcock parameter, $\log(S_1/S_2)/\log(S_2/S_3)$; I assume that these parameters are the numerator and denominator of that fraction, but it would be helpful to state that explicitly

Agreed. We propose to change the wording from *based on Woodcock parameters* to *based on the Woodcock parameters* $\log(S_1/S_2)$ *and* $\log(S_2/S_3)$.

> 463–475: I am unclear as to how this incorporated in the results, given the lack of data. It becomes clear much later, but we need a preview

Would it be sufficient to state after line 475 that *Both fabric and grain shape are not directly taken from ice core data, but left as free parameters in the fit (section 6.4)?*

> Figure 11: I am unclear on whether perpendicular to the picture is the same as perpendicular to the picture. i.e., it would be helpful to say that photons get "emitted" initially into/out of the page. I also think the description of the axes is unclear—as best as I can tell, this is a histogram of the normalized components of the direction vector? Why not use something other than n, considering that it has other meaning elsewhere?

We propose the following updated caption:
Example diffusion patterns after photon propagation through 1000 crystals (roughly equivalent to 1 m) with a perfect girdle distribution of c-axis orientations. The initially emitted photon direction is perpendicularly out of the picture, with an opening angle to the flow as indicated. The figures histogram the final direction vectors of many photons. The change in diffusion (width of the distributions) as well as the subtle effect of photon scattering towards the ice flow (towards the right) can be seen.

n is indeed ambiguous. Given the new caption, axis labels do not seem necessary.

> Figure 12: Ice flow direction should be indicated. Perhaps this should come before Figure 11, to give the reader some intuition?

The ice flow direction will be added. We are happy to swap the figure order.

> 488: missing punctuation and article/noun mismatch

Thanks. This will be fixed from
The same quantitative behavior as described above is reproduced, however this approach does not allow for a flexible configuration and is slow to simulate a reasonable photon statistics
to
The same quantitative behavior as described above is reproduced. This approach however does not allow for a flexible configuration and is slow to simulate reasonable photon statistics.

> Section 5.3: Except for the directionality of the effect, the rest of this section could come much earlier (even in the introduction) and give the reader useful context.

We struggle to move the entire section, as it does require knowledge about the diffusion mechanism and its relevant parameters, which is only provided in the previous section. We thus propose to leave the section as is and hope that the changes proposed to the introductory sections are sufficient to give the added context.

> 529: I count 8 parameters—or should Eq 24 be two equations, one for x and one for y

Equation 24 is indeed a short-notation for two independent sets of parameters, one for the x and one for the y diffusion. We will make this clear by giving two independent σ_x and σ_y equations.

> 533: Language is odd—update sounds computational but the rest of the sentence sounds physical

Since this refers to the simulation, it is indeed a computational aspect/limitation. We propose to reword this from *During normal photon propagation ...* to *During photon propagation simulation ...*

> Figure 14: Top y-axis label is mission. I guess the panels on the diagonal are actually different than the rest? It is pretty incomprehensible as-is—perhaps some separation between those panels and the others, and something clear in the legend, would help.

The diagonal panels show marginalized LLH contours for the parameter in a given column. Since the LLH value is already encoded in the color of the points for which an axis is given and since there is no clean way to show axis labels for the other diagonal panels but the top one, we opted against a label here. Since no quantitative results are actually derived from this exemplary plot, we suggest to simply remove the diagonal panels resulting in an overall less messy plot.

> 573: we need a definition of pre-fits

Yes. We propose to add the following sentence prior to line 573:

This is done through pre-fits, which either vary all parameters for a single exemplary layer or fit the depth dependence of a given parameter while keeping all other parameters fixed.

> Section 7: These results are really nice, and so I was surprised that this section was so short. As mentioned above, I think more of a traditional paper structure (where there is less history and these are the results) would be beneficial.

To us the primary result of this work / message of the paper is the newly gained conceptual understanding about light propagation in the peculiar glacial ice micro-structure, rather then the resulting ice model. Yet as also requested by the over reviewer we propose to expand to the discussion of the depth dependence to:

The overall grain size of ∼1mm and the increase in size at larger depths, where ice crystals are generally larger, are as generally expected and measured in glaciology (Laurent et al., 2004; Alley et. al., 2021). In addition an anti-correlation between crystal size and impurity concentrations, as mapped by optical properties can be observed. This follows the expectation that impurity related processes such as impurity drag hinder grain growth (Durand, 2006). [...] Averaged over all instrumented depths light diffusion in the birefringent ice polycrystal amounts to an effective scattering coefficient of $2.47 \cdot 10^{-2}\,\mathrm{m}^{-1}$, accounting for on average $\sim 8.5\%$ of the total scattering present in the ice. The comparatively strong isotropizing effect of Mie scattering also explains why the intensity on the tilt axis is never fully depleted.

> 686: The use of such tensors dates at least to Love, 1944 "A Treatise on the Mathematical Theory of Elasticity, 4th ed." and presumably earlier (it has a long history in elastics and fibers). In glaciology, it dates at least to Castelnau et al., 1996, "Viscoplastic modeling of texture development in polycrystalline ice with a self-consistent approach: Comparison with bound estimates."

The Scheidegger(1965) paper is 31 years earlier than Castelnau(1996). As he is the usual reference in glaciological literature, we propose to change the sentence to the following:
This ensemble of vectors can be represented via the matrix [Scheidegger(1965)].....

> 698: This is not true, as implied below by the acknowledgment that you cannot obtain a true c-axis distribution from these two parameters

Yes "all" is too strong a statement here. We suggest to change to the following:
Woodcock(1977) realized that many commonly encountered fabric states can be visualized...