Dear Reviewers,

Thank you very much for your constructive and valuable comments, which have helped us improve the quality of the manuscript. Below, we provide our point-by-point responses, with revisions marked in blue.

Sincerely,

Linlu Mei, on behalf of all co-authors

Reviewer 1

This paper presents the implementation of radiative transfer model SCIATRAN on snow, sea ice and ocean system with a great depth of technical details. I think in general the manuscript is well written and worth publication due to the scientific significance of the method and result it presents. Meanwhile, I have the following comments and would the authors to address before its publication.

Response: Thank you for the positive feedback.

Major comments:

1. The paper aims to cover a large number of topics, such as coupled atmospheric-ice-water surface, multi-layer snow and ice system, and polarization. The presentation of these many topics makes it hard for the reader to guess the technical details for specific topic. For example, Appendix A shows the implementation of a vector scheme of radiative transfer solver and all benchmark results in Section 4 are validated against vector radiative transfer models on relatively simple ocean scenes, which strongly suggest that we are seeing a vector scheme of coupled atmospheric-ice-ocean system. However, Section 5 validation is done with un-polarized measurements exclusively and makes me wonder whether it is done with the same vector radiative transfer scheme but under the scalar mode by setting all non-F11 elements of the scattering matrix to zero. I wish the paper can be clearer on that.

Response: The validation results presented in Section 4 compare different radiative transfer models, all of which provide simulations for the vector components. In contrast, the results in Section 5 are based on real measurement data. For these simulations, SCIATRAN was run using the same vector radiative transfer scheme but under a scalar model. This information has now been included in Section 5.

2. For the solving of minimization problem in Section 5, what are the *a priori* information used in each minimization? Also what is the measurement covariance being assumed? Also, what is the minimization solver used for these

results?

Response: We employed an iterative non-linear least squares fitting approach, meaning that no a priori information was used.

Minor comments:

Line 65 reference with ?

Response: We have added the reference in the revised version.

Line 472 refferred -> referred

Response: Corrected

Line 481 show -> snow

Response: Corrected

Please increase the size of the polar plots of Fig. 8-11. They are too small and it is hard to pickup details from them.

Response: We have increased the size of polar plots in Fig. 8 – 11 in the revised version.

Reviewer 2

This paper describes the technical implementation and evaluation of a coupled atmosphere-snow-ice-pond model, SCIATRAN. Overall, this has good potential to be a useful tool for the community, and I commend the authors for advertising it as open-source and publicly available. The model potentially has particular utility for the remote sensing community, as it produces multi-stream intensities and BRDFs for any illumination and viewing geometry. Moreover, the paper is generally sufficiently detailed and thorough in its description, comparison with other models, and comparison with measurements. Below are a few minor comments that I think should be addressed prior to final publication.

Response: Thank you for the positive feedback.

General comments:

It is well-established that the particle extinction efficiency approaches two in the

geometric optics limit (e.g., Bohren and Huffman book, 1988), and indeed the authors formulate this for scattering efficiencies of air bubbles and brine inclusions. Puzzlingly, however, the extinction efficiency for ice particles appears to be taken as 1 (if I understand correctly), as described on the bottom of page 11, neglecting the contribution from diffraction. If true, this seems to be a significant inconsistency in how extinction is formulated for ice particles vs. air bubbles and brine. Please address this issue, along with the implications of this assumption.

Response: We adopted this assumption based on the work of A. Malinka, as, for example, in Malinka, A. V. (2014). Light scattering in porous materials: Geometrical optics and stereological approach. *Journal of Quantitative Spectroscopy and Radiative Transfer*, *141*, 14–23. https://doi.org/10.1016/j.jqsrt.2014.02.022.

As presented in Malinka (2014), "we do not consider the diffraction part, in which case the extinction efficiency would be twice greater. The description of Fraunhofer diffraction by stochastic obstacles is given in Malinka and Zege (2009) and Malinka (2011).

A. Malinka, E. Zege Fraunhofer diffraction by arbitrary-shaped obstacles, J Opt Soc Am A, 26 (8) (2009), pp. 1763-1767

A. Malinka Fraunhofer diffraction by a random screen J Opt Soc Am A, 28 (8) (2011), pp. 1656-1661

We have included the information above in the revised version.

Line 319: A scattering asymmetry parameter of 0.998 is incredibly high. Practically, this would result in almost no scattering, since photons are simply redirected into the same direction they had originally been travelling. Some discussion of the implications of such strong forward scattering should be included.

Response: We used the results presented by A. Malinka in "Analytical expressions for characteristics of light scattering by arbitrarily shaped particles in the WKB approximation". J. Opt. Soc. Am. A, 32:1344–1351, 2015.

A detailed discussion of the potential impacts on simulations accuracy can be found in section 4. COMPARISON TO THE DISCRETE-DIPOLE-APPROXIMATION as presented in Malinka (2015).

We have included the above information in the revised version.

The model-measurement comparison is helpful, and (encouragingly) indicates that the model is capable of reproducing observed spectral reflectances quite well. The fitting procedure is also effective, but it is important to note that the best-fit model

parameters may or may not agree with the real state of the snow and ice that existed when/where the radiances were measured. In other words, this is not a perfect "closure" evaluation (since states like effective grain size were largely unknown) and it is at least possible that the radiative model agrees with measurements for the wrong reasons. This is a minor critique, but I would like to see this issue acknowledged more clearly in the discussion. (Also, more generally, it seems the fitting technique could be quite useful in the future for remote sensing inversions, once confidence in the forward model is instilled.)

Response: While we appreciate the thoughtful arguments presented, we feel that the discussion on this matter is beyond the intended scope of the current work, which is limited to new implementations of different snow, ice, melting ponds models in SCIATRAN.

Minor comments:

Figure 1 caption: Please include a first name and institution for Dr. Gunnar (attribution of the photo).

Response: Done

Line 65: There is a missing reference, indicated by "?" here.

Response: The reference has been added in the revised version.

Lines 76-88: Does the "Coupled Ocean and Atmosphere Radiative Transfer (COART)" model (https://satcorps.larc.nasa.gov/jin/coart.html) also qualify for discussion here?

Response: The information of COART model is included in the revised version.

Line 103: Briefly, what might this "yellow substance" represent? Mineral dust? Organic carbon?

Response: It can be Chromophoric dissolved organic matter (CDOM).

p.12, 4th last line: For the sake of reproducibility, what value of sigma_y (at lambda_0) is assumed in this parameterization (i.e., for the mass- or volume-normalized absorption coefficient)?

Response: The absorption coefficient of yellow substance was treated as a fitting parameter, and the corresponding fitted values for $\sigma_y(\lambda_0)$ are presented in Table 2 (page 24).

line 296: Assuming this is referring to snowpack (and not atmosphere), "vertical" would be preferable here to "altitude"

Response: This has been corrected in the revised version.

line 313: Is use of the Henyey-Greenstein function a user option in the code? If so, I suggest clarifying this.

Response: H-G functions is also included in the SCIATRAN. We have included the information in the revised version.

line 369: "Stockes" -> "Stokes"

Response: Corrected.

line 453: What are the model options for "relevant BRDF model"? (Perhaps this is explained earlier.)

Response: The BRDF model used in the retrieval process is introduced in for example line 685, where it is applied to snow reflectance.

Line 460: Briefly, please provide more detail about how scattering and absorption by aerosols is accounted for. What types of aerosols are represented? How is the aerosol burden or optical depth determined?

Response: A weakly absorbing aerosol type representative of typical Arctic background conditions, with an aerosol optical thickness (AOT) of 0.04 at 550 nm (see Mei et al., 2019), was used. The optical properties of this aerosol are given by:

1) The operational XBAER aerosol retrieval algorithm, which determines the scattering and absorption characteristics.
2) A parameterization implemented in SCIATRAN version 4.6, where the aerosol particle size distribution and refractive index are expressed as functions of AOT (see Table 2 in Mei et al (2023) for details).

Mei, L. *et al.*, "Retrieval of Aerosol Optical Thickness in the Arctic Snow-Covered Regions Using Passive Remote Sensing: Impact of Aerosol Typing and Surface Reflection Model," in *IEEE Transactions on Geoscience and Remote Sensing*, vol. 58, no. 7, pp. 5117-5131, July 2020, doi: 10.1109/TGRS.2020.2972339.

Mei, L., Rozanov, V., Rozanov, A., and Burrows, J. P.: SCIATRAN software package (V4.6): update and further development of aerosol, clouds, surface reflectance databases and models, Geosci. Model Dev., 16, 1511–1536, https://doi.org/10.5194/gmd-16-1511-2023, 2023.

The above information can be found in Line 611 in the version for reviewing.

Line 561: "... it still can reach values of about 0.7 for clear sky conditions..." - This is

indeed surprisingly high, but it is only in a limited portion of the spectrum. I suggest clarifying here at which wavelengths this high value is achieved.

Response: According to Fig. 6, the wavelengths are for the range between 400 nm – 500 nm. This has been included in the revised version.

line 581: "clody" -> "cloudy"

Response: Corrected

line 749: scattering layer thickness of 0.19-0.23 cm: This is quite thin, and much thinner than the value of 2cm that is often assumed in models for the thickness of the surface scattering layer. I suggest adding a brief discussion on that here.

Response: We would like to clarify a potential misunderstanding: our analysis addresses the optical thickness of the scattering layer—a dimensionless quantity characterizing its attenuating properties. This is fundamentally different from the geometric thickness of the ice.

We have clearly included this information in the revised version.

The references should list last name (surname) first.

Response: Thank you for pointing this out. The reference format will be finalized during proofreading if the manuscript is accepted.

Appendix A: I see that expressions for thermal emission are included here. It's fine to include those in the comprehensive model formulation, but just to be clear: Is thermal emission actually modeled in this study? I assume not because it only deals with solar spectrum processes. Perhaps thermal emission could have a small effect, however, at the longest wavelengths examined in this study. Please clarify.

Response: The thermal emission is not modelled in this study. We have included the information in Appendix A.