We are endebted to the Reviewers for their valuable feedback. The resulting revisions to the manuscript are described below.

REVIEWER 2:

This manuscript presents a multi-stream radiative transfer solver capable of handling strong backward scattering surfaces. While there does appear to be some scientific merit to this paper, the authors do not clearly motivate their claims or justify their stated significance with results. They do not present any significant inter-comparison or applications with measurements that is required to meet the scope of AMT. They also do not clearly state their conclusions. There is no clear structure in their methods, results, and conclusions. There is no clearly defined conclusion or results sections. The methods section are broken into discrete section. These sections should be organized into clearly stated methods, results, conclusions. Additionally conclusions drawn should be explicitly demonstrated in the results section with tables, figures, and metrics. Currently there is no quantifiable justification for their abstract statement. I recommend major revision or submission to a different journal with a scope focused on the basic methodological presentation.

Response to comment:

We appreciate your careful and detailed comments. We have re-organized our paper into Introduction, Methods, Results, and Conclusions. We have performed additional accuracy and timing studies which are now discussed in the Results section. After further investigation we became aware that the case of strong backscatter is likely not as common as we initially thought. We have revised the paper to reflect this reality. There are materials including snow and ice that have a significant backscattering component that can be easily fitted to a mesh approximation. These systems are poorly described using current tools, (and the accuracy study in our paper suggests that EigenFlux has an advantage for strongly forward scattering systems as well). We have added additional discussions on snow and ice.

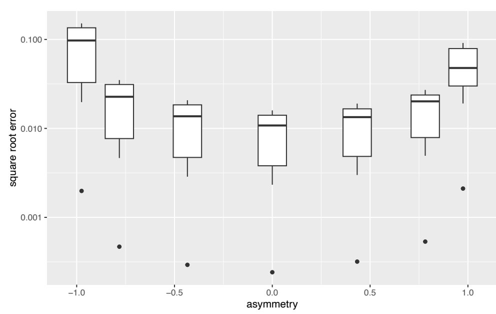
Reviewer comment:

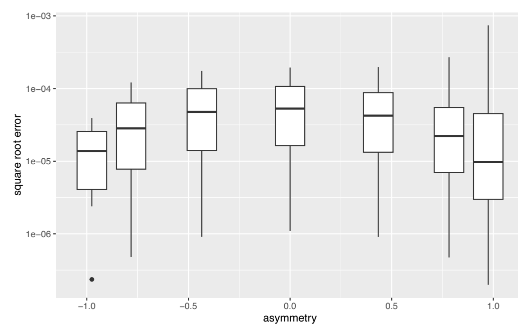
> There seems to be motivation for a non-commercial radiative transfer model (i.e., a novel method) that can handle strongly scattering media with absolute values of asymmetry that are high. While stated in the manuscript, there is no sources or comparisons to clearly

identify this gap they are interested in addressing. Suggest being explicit in what is the problem is, how is it handled before your method, and how is your method faster, better, or simpler using error metrics and accounting for uncertainties.

Response to comment:

Additional text has been added to the Introduction to discuss the gap we are addressing. A new subsection, “Accuracy and Timing Studies” has been added to the new “Results” section which includes an analysis of performance across higher asymmetries. The following (Figures 16 and 14 of the revised paper) show DISORT accuracy in the first graph, EigenFlux accuracy in the second graph.





Reviewer comment:

The method is novel and appears to have some advantages over the current standards, but there appears to be few results demonstrating the improvements. The tool being open source compared to a commercial option is a great idea but the commercial options are not compared nor is the current state of the open source tools. Suggest adding a results and conclusion section to demonstrate some of the improvements over current models or comparing against measured data for a specific application.

Response to comment:

A new subsection and table about comparative tools has been added. A new subsection, “Accuracy and Timing Studies” has been added to the new “Results” section. It includes regression testing across a range of asymmetry values and single stage albedo values and includes error results as a function of higher asymmetry. It includes comparative testing against the DISORT tool.

Reviewer comment:

This seems substantial but it is not clear from their manuscript what aspects of their method are superior to models that can handle snow and ice. The authors do not motivate their work to fit into AMT's scope. There is no significant inter-comparison of methods in this work.

Response to comment:

According to our comparative study, current snow and ice tools use the same algorithm as DISORT- discrete ordinates, with adjustments for the domain, with the same basic advantages and disadvantages. We have added a comparison with DISORT to the paper, discussed in the "'Accuracy and Timing Studies" subsection.

Reviewer comment:

Their method is explained in detail but can be hard to follow. Suggest rearranging and renaming most of the sections into: introduction, methods, results, and conclusions. Also, clearly state where the motivation is coming from using specific sources.

Response to comment:

The manuscript has been reorganized into the suggested format and clarifying language added.

Reviewer comment:

The authors do not clearly motivate their claims or justify their stated conclusions with results.

Response to comment:

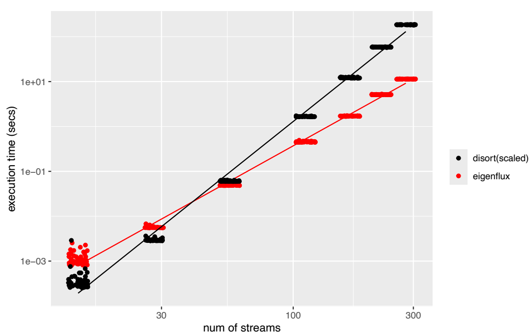
A new subsection, "Accuracy and Timing Studies" has been added to the new "Results" section. It includes regression testing across a range of asymmetry values and single stage albedo values and includes error results as a function of higher asymmetry.

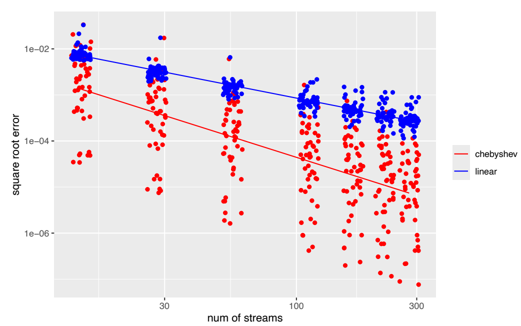
Reviewer comment:

The numerical experiments lack error analysis. Improvements should include analysis showing the sensitivity analysis, reports numerical error, and quantifies convergence time/reliability with mesh refinement. The authors should clearly show the limitation of their model and the improvements over other models.

Response to comment:

Additional work has been performed and a new subsection, "Accuracy and Timing Studies" has been added to the new "Results" section. It includes regression testing across a range of asymmetry values and single stage albedo values and includes error results as a function of higher asymmetry. It includes comparative testing against mesh refinement and improvements over the DISORT tool. The results are shown in Figures 17 and 12 of the revised manuscript, copied here:





Reviewer comment:

The authors do appear to misrepresent some of the related work. For example, why is ``HydroLight, Radiative Transfer Software'' cited when it wasn't designed for this role of atmospheric radiative transfer with highly scattering media. Suggest comparing with models used for ice and snow that might be better comparisons.

Response to comment:

Snow and ice models have been included as part of a new table, Table 2, on comparative tools. Hydrolight was included at the particular request of a previous reviewer.

Reviewer comment:

The abstract claims are concise but not represented well in the manuscript's contents. Suggest clearly demonstrating the claimed results or altering the abstract to align with the manuscript.

Response to comment:

Both the abstract and the contents have been changed substantially as part of the re-organization of the paper. Additional work has been performed and subsection “Accuracy and Timing Studies” has been added to the new section “Results” specifically to address the claimed results. We have examined and modified the claims and the text to ensure there is a clear link.

Reviewer comment:

1) The structure of this manuscript is a bit disorganized and much of the text is hard to understand as a result. Several paragraphs are 1 or 2 sentences. Suggest combining these short paragraphs to make the text more readable and understandable. The science question does not have a clear link to any research gaps.

Response to comment:

We have re-organized the paper into Introduction, Methods, Results, and Conclusions, consolidated paragraphs, and added clarifying language.

Reviewer comment:

2) The language is fluent, but typographical errors and unclear language are present throughout.

Response to comment:

We have corrected the typographical errors and added clarifying language according to the reviewer’s notes within the paper.

Reviewer comment:

3) The formulae, symbols, abbreviations, and units appear to be correct, however no external comparisons are made to verify this. Certain terms like convergence are not defined. Suggest external comparisons and explicitly defining convergence.

Response to comment:

We have now compared and matched our results to DISORT. The use of the term "convergence" was imprecise and alternative language is now used.

Reviewer comment:

4) Several figures have unclear captions and their results are unclear. Tables appear to be labeled as figures. Suggest improving the captions throughout to ensure these tables and figures are understandable on their own, adding more comparison figures, removing or combining figures that do not add to the scientific analysis, and ensuring tables are labeled and reference correctly.

Response to comment:

All captions have been reworked for clarity; tables have been relabeled as tables. Some unnecessary figures have been removed.

Reviewer comment:

5) The manuscript has some quality references. Suggest adding more references for comparisons to other multilayer radiative transfer models like those used for snow and ice.

Response to comment:

Language on snow and ice has been added, both to the Introduction and as part of a new table on comparative tools. "Snow and ice are particularly important elements in determining the albedo of the Earth, and microwave sensing has become an important tool for characterizing current snow and ice levels. Current algorithms such as the Discrete

Ordinates method are being provided as part of RTE solvers which also include custom models for the highly forward scattering and low absorbency features of snow and ice."

Additional reviewer comments

Would snow and the aforementioned SMRT be able to handle this (+- 0.99 asymmetry)? Is so, it would be important to state that and make comparisons/contrasts.

Response to comment:

This is an excellent suggestion. Yes, SMRT should be capable of treating highly forward-scattering cases ($g \gtrsim 0.99$), provided that the underlying snow or ice microstructure produces such asymmetry factors. Because SMRT derives the phase function from physical microstructure models and uses the DORT discrete-ordinate solver, it faces many of the same numerical considerations as DISORT for strongly forward-peaked scattering. SMRT models snowpack and it is specialized for that application. It has electromagnetic and snow structure models and derives a phase function from that theory, so asymmetry is derived. The question is whether the snowpack or icepack strutures that it can model have a symmetry of greater than 0.9. Stamnes[1] reports g = 0.85, 0.89, 0.997 for air bubbles, snow grains, and brine pockets, but SMRT has limited capability for modeling brine pockets[2] . The underlying solver- DORT- is an implementation of Discrete Ordinates, so should have the same general capabilities as DISORT. DISORT requires some version of the "delta-N" method which models some portion of the forward scatter as pure transmission to handle asymmetry greater than 0.9, which also results in loss of precision. It seems reasonable that DORT has the same issues. The comparison to DISORT is considered in more detail in a new section we have added on "Accuracy and Timing Studies".

Additional reviewer comments

Table 7, this caption needs more detail. Number and distribution should be explained. This should stand on its own without the text.

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[1] "Modeling of radiation transport in coupled atmosphere-snow-ice-ocean systems" , K. Stamnes, B. Hamre, J.J. Stamnes, G. Ryzhikov, M. Biryulina, R. Mahoney, B. Hauss, A. Sei, Journal of Quantitative Spectroscopy & Radiative Transfer 112 (2011) 714–726

[2] Picard, G., Sandells, M., and Löwe, H.: SMRT: an active–passive microwave radiative transfer model for snow with multiple microstructure and scattering formulations (v1.0), Geosci. Model Dev., 11, 2763–2788, https://doi.org/10.5194/gmd-11-2763-2018, 2018

Response to comment:

Caption has been expanded. “Test case parameters. v_i and z_i were fixed for all runs, so there were 33*29 test cases. These test cases span the range of asymmetry and single stage albedo, with increased density at the extremes of strong scattering and low absorbency, in order to cover the analytic features of the RTM. For v and z Chebyshev point of the second kind were used, remapped to the interval [0,1] (and [-1,0] for v.) Then g is distributed as the remapped Chebyshev points of the first kind. The strong sensitivity to low absorbency dictated that ω be the squares of the Chebyshev points of the first kind.}”

Additional reviewer comments

"Figure 11, convergences of…" What is convergence? Do you mean rate of convergence?

Response to comment:

Caption has been reworded to say “As depth y increases, the diffuse internal intensity dominates the direct source intensity (the peak at the right) and the normalized intensity converges to the diffusion limit.”

Additional reviewer comments

"Inherent and Apparent Optical Properties" This is is interesting because you do this analysis for a two stream model, however there are multistream models that exist. Your's is also a multistream model. It would be better to show how this problem impacts the models that you stated cannot handle the highly backscattering media with large asymmetry.

Response to comment:

This section is about the two-stream model. Comparision to other multi-stream models is made in a new “Accuracy and Timing Studies” section.

Additional reviewer comments

"reference DISORT results" This comparison should be shown in the paper to larger extent. Figure 18 appears to be a table with some time values that don't appear to be in favor of the current paper's methods.

Response to comment:

The figure has been removed in favor of a new, complete section of comparative results.

Additional reviewer comments

"Table 2" So it is faster in some cases but slower in others? Only 6 comparisons? What about comparing the actual values of TOA radiance for example? Can they reproduce your results? You claim they cannot get to asymmetry as high as your model, but never compare results in bet-case or worst-case scenarios.

Response to comment:

The new section on timing studies shows that this anomaly is due to DISORT being faster for smaller number of streams, while EigenFlux is faster for larger numbers of streams, with a crossover at n = 56. See the new section on test results.

Additional reviewer comments

"Incorporating Azimuth-dependence" and "Model Approximations" -- are these methods?

Response to comment:

Yes. "Incorporating Azimuth-dependence" has been removed and the material incorporated into the Methods section as part of the paper reorganization. Model Approximations" is now part of the Methods section.

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