The authors would like to thank the reviewer for their comments and feedback. Our responses are given below in blue.

Reviewer 2

This is a welcome short communication that addresses an important issue. The authors proposed to apply a new method for non-local strong contrast expansion, developed by Torquato and Kim (2021) to compute wave propagation in a two-phase media, to the full range of snow density and microstructure found throughout cryspheric regions. The intention is to help to address problems of grain size, shape, and snow density in electromagnetic theory, particularly related to the computation of the microwave scattering coefficient. The paper is well-written and concise. There are several corrections recommended for clarity, and a suggestion for expansion of the discussion. Several other minor corrections are recommended, listed sequentially.

General comments

- Can the authors comment on the general applicability for active microwave modelling? Or is this applicable only to passive microwave solutions?

The SCE is concerned by the propagation of electromagnetic radiation in porous medium (effective permittivity), whatever the source of this radiation. Thereby, as a general response, it applies to both passive and microwave remote sensing equally. However, since the radar and passive microwave radiometer may operate in different ranges of frequency, the benefit is to be evaluated on a case by case basis, for each sensor and each application.

We have added mention of “passive and active microwave” in the conclusion.

- We have often observed enhanced scattering where substantial depth hoar (DH) is present. Does the work of TK12/SCE support/explain this behaviour wrt DH given the distinct microstructure? Given the strong scattering from DH, it would be nice to see some comments on suitability for DH. The authors state in the abstract that this method should be applicable for coarse-grained snow and again in the conclusion but specific reference to DH would be welcome.

As stated in the introduction, it is useful to distinguish three main steps in modeling the propagation of radiation in the snowpack, and this paper is about step 2 (electromagnetic theory) while the reviewer’s question is covered by our step 1 (microstructure). We have a new paper published very recently (Picard et al., 2022) which specifically addresses step 1 in detail, and contributes to the answer to the question of DH scattering efficiency.

G. Picard, H. Löwe, F. Domine, L. Arnaud, F. Larue, V. Favier, E. Le Meur, E. Lefebvre, J. Savarino, A. Royer, The Microwave Snow Grain Size: A New Concept to Predict Satellite Observations Over Snow-Covered Regions, *AGU Advances*, 3, 4, https://doi.org/10.1029/2021AV000630

In a nutshell, yes, depth hoar is an efficient scattering crystal because in general depth hoar crystals are bigger in extent than other grains (a well-known trait) and also because it has a specific structure that we demonstrate to the related to the chord length dispersity which makes it more efficient for a given “size” (this is the main new finding in Picard et al. 2022).

In the present paper, our use of “coarse-grain” is generic and is relative to the frequency. DH can be either small (e.g. at C-band) or big (e.g. Ka band). We have not amended the text because we believe the question of DH scattering is out of the scope of the present paper. However, we will add the reference to Picard et al.

Specific comments

- on Line 22, the authors state that existing theories "prevent consistent modelling of snow". Can the authors clarify what they mean by consistent?

We have checked the definition of this term, which is “a theory that does not suffer from assumptions” and it is conform to what we mean, that is a theory that is valid for “snow, firn and ice in the <u>entire</u> range of density and microstructures found throughout the cryospheric regions” (extract of the actual text). Existing theories are not consistent (at least) because representing snow and dense firn is obtained by media inversion, and the mid-range of densities can not be represented.

- Line 21-24. Related to the above point, perhaps the authors can cite specific studies that

The comment is incomplete but we can say that there are no study to our knowledge that demonstrates experimentally the problem in the mid-range of density. This is a long-standing theoretical problem, but other sources of uncertainty make it difficult to highlight specifically this problem in the measurements.

- In the study by TK21, they do not refer to Strong Contrast Expansion (SCE). While the authors' descriptor is useful, SCE in our community generally refers to snow cover extent which might be confusing for some readers. By way of a suggestion, perhaps the authors could use an alternative descriptor?

Rechtsman and Torquato, 2008, Kim and Torquato 2000 and TK21 use “strong-contrast expansion” in many places. Although we understand Reviewer’s concern, we feel uncomfortable with renaming their theory and we still need an acronym.

- Line 35. In the paper (TK21), they did not specify a snow medium but a 2-phase medium more generally. The authors make it seem as though TK21 had derived it explicitly for snow. Perhaps re-phrase “….which can be used to express the dielectric polarizability….”

TK21 is generic while this paper is a specific application to snow. We have reformulated this section to make it clear.

- Equation 1, and Line 47 – As is not defined. It is sort of defined on Lines 51-52 but still not very clear. Please can the authors define this term as it is in equation 1.

We have added a forward reference to the paragraph dedicated to these terms.

- Line 41 - Is this the same as the scaled SCE in Figure 1? If so, be sure to use consistent nomenclature. If not, then can the authors explain the difference more clearly?

The scaled SCE is defined in TK21 and we use the scaled SymSCE in Figure 1 by applying symmetrization to the scaled SCE following TK21 approach described in their Appendix. We are sure that the nomenclature is correct and consistent.
However, we have shifted the quote which may have been a source of confusion: "scaled" SCE in TK21 → "scaled SCE" in TK21

- Line 46 – it is not clear what the italicized symbol is in the in-text definition of Ke. This should be defined.

This is the Latex symbol for the imaginary part. We have added the definition.

- Line 57 - what, specifically, would be needed in terms of 'increasingly more detailed information of microstructure?' What does this mean? The authors should provide the reader with more clarity since field experimentalists will be interested.

We propose to add "(e.g. larger and more resolved 3D images of the snow microstructure)". Although this statement clarifies the text for the "experimentalists", it suffers from subjectivity, mainly because it is not yet clear how the size and resolution of microstructure images translate into accuracy of the n-point correlation functions and then in the An terms.

- Lines 56 – 59 – What is the benefit of n > 2 should it be a feasible computation? Or, in other words, what are we missing out on by using n = 2? What are the practical implications?

As in the previous comment, we do not know how different the model would be with n=3. It depends on the amplitude of A3 and of the 3-point correlation function. Given the uncertainties in fitting the 2-point correlation functions models to experimental snow data, we believe the priority in 2022 is on consolidating the case n=2 (see Sandells et al. 2021) before addressing n=3 in the future. In both cases anyway, this represents very significant work for our community as detailed in our response to Reviewer 1's comments.

- Figure 1 and lines 70 – 71, Line 80 (and discussion throughout paper) – RT08 does not appear in Figure 1. Do the authors mean SCE R08? Similarly, TK21 in Figure 1. Do you mean Scaled SymSCE T21? The authors should check that the text matches the figure descriptors, here, and throughout the paper. Otherwise, it is confusing.

We have corrected the typo on RT08 and TK21 in Figure 1.

- Line 78 – what is the FDTD method ? Please expand this acronym.

Done

- Line 80 – The authors imply that there is some spread between Scaled SymSCE T21 and Mie DMRT and IBA for koa>~1. What are the implications, or is this unimportant?

The equivalence of the theories below <1.5 is a good sign and the differences between the theories is suspicious because at most one can be correct. We conclude that it is safer to keep the size < this limit. We have reformulated the text: "These results also highlight the equivalence between SCE, IBA and DMRT Mie up to about k_0 a ~ 1.5. Our main practical recommendation is limiting the size in the range where these three theories agree."

- Line 122 – the authors write "Let choose a linear combination…" This should be "Let us choose…"

Done

- Line 125 – you introduce SymSCE. Is this the same as Scaled SymSCE T21 shown in Figure 1? This gets a little confusing.

We have corrected by adding scaled: " This provides a new approximation of the scaled SCE that we call scaled SymSCE."

- Line 149 – 151 - Is there work going forward on this? There must be some existing suite of in situ measurements that could satisfy the requirements, no?

Not to our knowledge. A relatively homogeneous snowpack around 450-500kg/m3 is not frequent, and assessing these theories over thick, and thus heterogeneous, snowpacks (as on the ice-sheet) is subject to many other uncertainties.

- Line 155 – the authors call for more precise in-situ microwave and snow measurements than what is available at present. What do you mean by more precise? Please be more specific here – what measurements, exactly, are needed? How does 'SCE' relate to physical field measurements? What field measurements are needed?

We have added the following information and refer to Picard et al.

“will require more precise in-situ active or passive microwave (backscatter or brightness temperature) and snow measurements (density, grain size, temperature) from snowpack with intermediate densities (450 – 500 kg m^{-3}) than available at present.”