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Interactive Comment

Interactive comment on "A flexible importance sampling method for integrating subgrid processes" *by* E. K. Raut and V. E. Larson

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In this response to the reviewer's comments, the reviewer's comments are italicized, and our responses are in roman font.

Anonymous Referee #2

Summary:

This paper discusses methods for improving the calculation of microphysical process rates in the presence of sub-grid variability. It is a very technical paper, and as such quite tough to read, but the authors should be commended on their efforts to detail their methods so rigorously in the published literature. I see no technical errors in the paper, and only have a few minor comments for the authors to address before it is suitable for

publication.

Thank you for your review.

Specific points:

P9154, L26 - It might be useful to state here how the importance sampling level is chosen, since it is quite buried in section 6 and feels like something which should be discussed with the method rather than with the results.

We now state how the importance sampling level is chosen here as well.

Section 4 - is quite long and technical, I wonder if some of it could be put in an appendix, to enable the reader to get to the results quicker. I am keen that none of the information is lost though, as I think it is useful to document the method in such detail.

To improve readability of the manuscript, Eqs. (20)–(22) have been deleted, since they are simply a verification of the math and are not needed to reproduce the method. They will not be lost because they will remain in the discussion paper. Eqs. (28)–(32) have been moved to an appendix.

P9169, L21 - An analytically upscaled version of KK is used as a reference, but as discussed in the introduction, this does not contain information about vertical overlap. Therefore the comparison of the results to this may differ for justifiable reasons, i.e. the results really should be different because the overlap is treated differently. Ideally it would be nice to see SILHS tests against something they would in theory converge to, but if it's not possible to get the vertical overlap assumptions to be the same, then the authors should at least mention this issue in the text.

The revised manuscript states:

"The microphysics scheme used in the simulations is that of Khairoutdinov and Kogan (2000). As a reference solution, an analytically upscaled version of the Khairoutdinov-Kogan microphysics scheme was used, as described in Larson and Griffin (2013).

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Comparison with the analytic solution indicates whether SILHS draws sample points from the correct PDF at each grid level. However, the comparison with the analytic solution does not test whether the PDFs at each level are overlapped accurately. Although the overlap assumptions do not affect these test cases, overlap does influence processes such as radiative transfer. Testing the PDF overlap assumptions is left for future research. Nevertheless, the ability to test convergence at each grid level is an advantage. For instance, early convergence tests revealed several bugs in SILHS. Many microphysics schemes in operational use do not permit analytic solution. For these microphysics schemes, a non-analytic integration method, such as SILHS, is necessary."

P9175, last paragraph - whilst the authors statements about the 8Cat method being much cheaper than the 2Cat-Cld method are true, I feel they should also acknowledge that it will still be much more expensive (presumably 8 times?) than a 'standard' microphysics scheme that is not using Monte Carlo integration.

The expense of the microphysics alone scales with the number of sample points, and SILHS adds additional overhead cost. The revised paper adds:

"An important consideration among Monte Carlo integration methods is their computational cost. The cost of the new nCat method was tested against both the prior SILHS importance sampling method and the cost of CLUBB. Eight SILHS sample points were used in each simulation. Five RICO simulations were performed, and Table 7 shows the means and standard deviations of the five simulations. Each time is a cumulative total of the respective component of the model over the entire simulation. The two nCat methods (2Cat-CldPcp and 8Cat) show no significant increase in computation time as compared to the original SILHS importance sampling method. All SILHS methods are about twice as expensive as CLUBB when eight sample points are used."

I appreciate that this paper is detailing a method and some simple tests, but in the examples given there would really be no need to use Monte Carlo integration, as an-

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alytical upscaling gives perfectly good answers and is much cheaper. I feel this needs to be pointed out to the reader.

The revised manuscript states:

"The microphysics scheme used in the simulations is that of Khairoutdinov and Kogan (2000). As a reference solution, an analytically upscaled version of the Khairoutdinov-Kogan microphysics scheme was used, as described in Larson and Griffin (2013). . . Many microphysics schemes in operational use do not permit analytic solution. For these microphysics schemes, a non-analytic integration method, such as SILHS, is necessary."

As I don't yet feel that any paper has demonstrated the advantages of Monte-Carlo microphysics justify its costs.

The revised manuscript states:

"These costs may be compared with other costs in global climate simulations. To this end, Thayer-Calder et al. (2015) tested the cost of SILHS in the Community Atmosphere Model (R. B. Neale et al., 2012). They show that an adequate cloud climatology can be obtained with as few as 4 sample points (see Figs. 12 and 13 of Thayer-Calder et al. (2015)). The extra cost of computing 4 samples is (1.89-1.69)/1.69 = 16% (see Table 2 of Thayer-Calder et al. (2015))."

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