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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 #1

General comments

. . . Overall, this paper is a useful contribution to an important and rather complex area of atmospheric modelling. I did not find any major flaws in the paper. Also the writing is generally good, but some points should nevertheless be clarified. Therefore, I recommend publication of this work subject to the minor comments listed below.

Thank you for your review.

Specific comments

1. p. 9148, lines 4–5 and p. 9149 lines 7–8: "An important aspect of atmospheric modeling is integration". There are other issues related to integration in atmospheric models (most prominently, time integration methods), so I recommend to be a bit more specific here: "An important aspect ... is spatial integration over subgrid scales".

The revised manuscript is more specific in these two places.

2. p. 9152, lines 13–15: Does "extended cloud water" also include ice?

Yes. The phrase "extended cloud water" has been replaced by "extended cloud (liquid) water." The manuscript now also includes "The variate χ does not include ice."

3. p. 9153: To make it easier for the reader, please explain the physical meaning of Eq. (4) right after the equation. So far I can tell, it implies that the joint-pdf of the nonprecipitation variables is the same for the precipitation-free and precipitation-containing parts of a grid cell. Now this explanation is delayed until the very end of p. 9156.

The revised manuscript writes: "That is, the marginal distribution of cloud and turbulence within a mixture component, m, is the same both within and outside of the precipitating region. Therefore, integrating over precipitation in Eq. (3) collapses the two terms per component m into one."

4. Much of the paper, in particular section 4, is devoted to the mathematical description of the algorithm. While it needs to be documented, the paper might be easier to read if some of the derivations were placed in appendix(es). For example, Eqs. (20)–(22) and Eqs. (28)–(32).

To increase the readibility of the paper, Eqs. (28)–(32) have been moved to an appendix. Eqs. (20)–(22) have been deleted from the revised paper because they are

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not needed for implementation of the method.

5. p. 9167, line 14: To make it absolutely clear, does "any number of sample points" also include the use of less than Ncat samples?

We now write "This allows any number of sample points to be used at each timestep, including the use of fewer than N_{cat} samples, without causing a biased result."

6. p. 9169, lines 1–12. How are the mixture components 1 and 2 handled in "2Cat-Cld" and "2Cat-CldPcp"? For example, are the cloudy (clear) parts of mixture components 1 and 2 lumped together in the cloudy (clear) category of "2Cat-Cld"? This should be clarified.

The revised manuscript writes, in the description of 2Cat-Cld, "The categories (c,p,1), (c,p,2), (c,np,1), and (c,np,2) are all lumped together into the 'cloud' category, and the other four categories are analogously lumped into the 'clear' category. That is, a point that is in cloud belongs to the cloud category regardless of whether it is in precipitation or which mixture component it is in, and similarly for points in clear air."

In the description of 2Cat-CldPcp, we write "That is, (nc,np,1) and (nc,np,2) are lumped into the no-cloud-or-precipitation category, and the others are lumped into the cloud-or-precipitation category."

7. p. 9169: line 21: "As a reference solution, an analytically upscaled version of the Khairoutdinov-Kogan microphysics scheme was used ...". In my understanding (and based on what is said at the end of p. 9149), the analytic integration scheme cannot handle the vertical overlap (correlations between layers), while SILHS does. So how can it provide reference results for SILHS? Please clarify this.

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."

8. p. 9170, line 11: "The optimal γ_j values are calculated by estimating the right-hand side of Eq. (34) at each timestep." Which process rate h(x) was used for the optimization? Also, assuming that this calculation was done at the importance sampling level, how was this level selected?

We now write "The optimal γ_j values are calculated by estimating the right-hand side of Eq. (21) at each timestep at the importance sampling level. The process used, $h(\boldsymbol{x})$, is the sum of the autoconversion, accretion, and evaporation tendencies from the microphysics scheme. The importance sampling level is chosen at each timestep to be the level with the maximum within-cloud cloud water mass mixing ratio."

9. p. 9171, lines 4–5: What is the reason for considering the sum of autoconversion, accretion and evaporation? Is it because it yields the total rainwater tendency?

Adding these terms allows us to compare the *overall* errors in different sampling methods for the three major non-transport terms. For instance, the percentage error in autoconversion may be large, but the absolute error, as compared to the error in accretion, may be small.

10. p. 9172, lines 25–26: What explains the smaller sampling error for 8Cat in autoconversion and accretion for the DYCOMS-II RF02 case? This is not intuitive because

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for a nearly overcast case, almost all sampling points reside in the cloud also for the other methods.

We now include a table that compares the sample point allocations between the four methods for DYCOMS-II RF02, analogous to Table 4. The revised manuscript includes this table and the following paragraph:

"The reason for the improvement using the 8Cat method can be inferred from Table 5. The table shows the percentage of sample points allocated to each category at the importance sampling level, averaged over the entire simulation. The 2Cat-CldPcp, 2Cat-Cld, and LH-only allocations are all similar, but the 8Cat allocation places more points in (c,p,1) than (c,p,2). That is, unlike the other three methods, the 8Cat method is able to preferentially sample from mixture component 1. Component 1, in turn, contains larger cloud (liquid) water mixing ratios. The other three methods necessarily place more points in component 2 than component 1, because component 2 occupies more of the (original) PDF. However, it was shown in Table 3 that optimally, component 1 has a much higher *per-probability* sampling density than does component 2. This increased sampling of component 1 is the source of the improvement of the 8Cat method."

11. p. 9173 (or elsewhere): It should be reported how the new sampling method influences the computation time, compared to the earlier version of SILHS, for a given number of subcolumns. This is relevant especially as it is known (e.g. Thayer-Calder et al. 2015) that both CLUBB and SILHS entail significant computational costs.

The revised paper adds a comparison of run time between the old and new sampling methods and CLUBB. The following paragraph is added:

"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

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total of the respective component of the model. 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."

12. p. 9183, caption of Table 3: Should Eq. (33) be Eq. (34)?

Yes; thank you. This has been fixed in the revised manuscript.

13. Figures 1, 2, 4 and 5: Are these figures based on the ensemble of 12 simulations, or did you just pick one simulation for each approach?

For these figures, only one simulation is used. An ensemble of 12 simulations is used only for the profile plots, to reduce noise and make the main result clearer. The revised manuscript states “We note that, in this paper, only the profile plots display an ensemble average. The time series plots display a single simulation so that individual sample values can be seen. The plots displaying RMSE vs. the number of sample points are not strongly influenced by the choice of random seed."

14. Figures 3 and 6: It is interesting and somewhat worrisome that even for time averaged values over hundreds of timesteps and with a fairly large number of sample points (32), the differences between different realizations are still so clearly visible for RICO.

The revised manuscript states:

“It is interesting that this noise remains even after time- and ensemble-averaging. This highlights the large degree of variability in cumulus clouds and the need for careful parameterization of this variability."

However, these plots (especially Fig. 3) are rather confusing visually, and it is very difficult to compare the four sampling methods with each other. I think it would be more informative to show (e.g.) the profiles of ensemble mean value and std. dev. among the 12 simulations, for each sampling method separately.

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To make it easy to compare noise among the different sampling methods in the profiles, we now show two figures for each case, each with four panels. One shows the mean of the simulations over all timesteps and all ensemble members, and the other shows the RMSE over all timesteps and ensemble members. This style of plot — unlike, for instance, Fig. 3 of Ackerman et al. (2009) — allows the reader to compare the variances of all methods on a single plot.

15. Captions of Figs. 3 and 6: The number of timesteps should (1) also be given for the RICO case in Fig. 3, and (2) be given correctly for the DYCOMS-II RF02 case in Fig. 6. According to p. 9170 (lines 13–15) these numbers are 864 for RICO and 360 for DYCOMS-II.

This has been corrected in the revised manuscript.

Technical corrections

1. p. 9150, line 7: this should be "a couple of advantages".

Fixed as suggested.

2. p. 9152, lines 19–20: "which is related to cloud droplet number" can be deleted. It is explained in the following sentence.

Fixed as suggested.

3. p. 9156, line 7: I think it would be more precise to say: "Each category has $p_j \geq 0$, but naturally, categories with $p_j = 0$ need not be included ...".

Fixed as suggested.

4. p. 9172, lines 9–10: This should be "an ensemble of 12 simulations". This also applies to p. 1973, lines 15–16; p. 9188; and captions of Figs. 3 and 6.

Fixed as suggested.

5. Captions of Fig. 3 and Fig. 6: "at the importance sampling level" should be deleted.

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Fixed as suggested.

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