

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

Anonymous Referee #1

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General comments

Microphysical process rates (and also radiative effects) depend non-linearly on the associated cloud and precipitation microphysical parameters, such as water content and number concentration. Consequently, if one attempts to determine average microphysical process rates by using microphysical parameters averaged over a large area (e.g., a GCM grid cell), the results may be severely biased. For a non-biased calculation, integration over the subgrid spatial scales is required. The most straightforward and flexible approach is to perform a Monte Carlo sampling over subcolumns, provided that a means of generating the subcolumns is available. However, Monte Carlo integration features random errors, or noise, that could influence the simulation. In a recent pa-

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per, Larson and Schanen (2013) introduced the Subgrid Importance Latin Hypercube Sampler (SILHS), which generates multivariate subcolumns and applies two methods to reduce the sampling noise: Latin hypercube sampling to avoid the clumping of points in the same region of the parameter space, and importance sampling to increase the sampling of the regions that contribute most to the process rates (e.g., increasing the sampling of cloudy parts of the region while decreasing the sampling of the clear-sky part).

The present paper represents a flexible extension of the importance sampling method in SILHS, where the parameter space is split into N_{cat} (here, $N_{\text{cat}} = 8$) categories, and the sampling density (modified / original probability) can be adjusted separately for each category. In particular, it is demonstrated that adding weight to the cloud-free but precipitation containing part of the domain helps to reduce the random errors in evaporation of rain, which were an issue for the earlier version of SILHS (the importance sampling in SILHS actually increased these errors). The utility of this approach for large-scale models depends, in part, on the choice of the coefficients γ_i . Obviously, it is not feasible to optimize them for each GCM grid column, so one would most probably define just one set γ_i , or at most a few sets to be selected based on some simple criteria. Therefore it is critical that the results are not overly sensitive to the case used to optimize γ_i . In this respect, it is promising that the γ_i coefficients optimized for the RICO trade-wind cumulus case also help to reduce the sampling errors for the DYCOMS-II stratocumulus case, with very different cloud fraction.

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.

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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".
2. p. 9152, lines 13–15: Does "extended cloud water" also 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 non-precipitation 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.
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).
5. p. 9167, line 14: To make it absolutely clear, does "any number of sample points" also include the use of less than N_{cat} samples?
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.
7. p. 9169: line 21: "As a reference solution, an analytically upscaled version of the

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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.

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?

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?

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

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.

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

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?

14. Figures 3 and 6: It is interesting and somewhat worrisome that even for time-
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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. 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.

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.

Technical corrections

1. p. 9150, line 7: this should be "a couple of advantages".
2. p. 9152, lines 19–20: "which is related to cloud droplet number" can be deleted. It is explained in the following sentence.
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 ...".
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.
5. Captions of Fig. 3 and Fig. 6: "at the importance sampling level" should be deleted.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9147, 2015.

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