Reply to review by Jason Milbrandt

Dear Jason,

Thanks reviewing our manuscript. Your comments have been very helpful and we hope that you will enjoy reading the revised version. It proved to be quite interesting to include the Murakami and Mizuno approximation in the comparison. Thanks for your suggestions which made us look in more detail into that.

Specific comments:

Reviewer: *The motivation regarding the importance of analytic approximations in bulk schemes may be somewhat overstated. It is summarized in the text on p. 5080 that analytic approximations are ... an essential part of microphysical parameterizations. Using look-up tables is always possible, however cumbersome, so perhaps is is more cor- rect to say that analytic approximations play are an important part. Wording details aside, the authors argue against the use of look-up for operational NWP. This argu- ment seems strange (and incorrect); the parameter testing, tuning, and creation of the look-up tables all gets done off-line, so there is no slow-down for operational NWP with look-up takes. On the contrary, once created the schemes run faster and the collection equations are computed accurately. The authors state that there are technical prob- lems regarding memory access with large multi-dimensional look-up tables. I do not personally have much experience with this, but colleagues at NCAR tell me that this is not real problem on modern supercomputers; once the look-up table is read in at the beginning of the integration, it is held in memory and readily accessible. I agree that good analytic approximations are very important and I would certainly not advocate that we get rid of them and use only numerical solutions and look-up tables, but I believe the argument is overstated in the manuscript and I would recommend reconsidering the arguments made in favor of analytic approximations on p. 5080.*

Reply: Reading the manuscript again, we agree that the wording was maybe not appropriate and that the statements regarding the issues with lookup table were too strong. We re-wrote that part of the introduction and, alhtough we keep most of the arguments, we now say that both, look-up tables and analytic approxiamations, are viable options and it depends on the application and the specific hardware, processor etc. which implementation is most efficient or most appropriate.
Nevertheless, we do think that look-up tables have issues and we know, for example, quite a few cases in which bugs and inconsistencies can be traced back to look-up tables.

Reviewer: *The comparison between the collection equations for the various approaches, sum- marized in Figs. 5-7, is very nice. However, it would be very useful to include here comparisons using the Murakami/Mizuno modification to the Wisner approximation. There approach has weaknesses compared to that of the SB2006 variance formula- tion, as discussed in the text, but it has the redeeming quality that it is very easy to add to existing bulk schemes that currently use the original*

Wisner approximation and with no additional computational cost. Therefore, it would be useful to modelers to see how good or bad the simple ad hoc approximation is compared to the proposed obviously good but not so straightforward analytic approximation. At the least, the authors should discuss in the text how the results compare for the Murakami-Mizuno vs. the SB2006 formulations.

Reply: During this study we were not sure how to handle the Murakami-Mizuno approximation. It just seems not correct to use just one set of parameters in that ansatz for all particles and all possible collision rates. Based on the reviewer's comment we just gave it a try and used the values of Murakami (1990) in combination with our Atlas-type sedimentation velocities (properly averaged as described for the Wisner approximation). This works indeed surprisingly well for most interactions. We therefore replaced the Wisner approximation with the Atlas-type fall speeds in the main text with the corresponding Murakami-Mizuno ansatz. Of course, there would still be the option to re-calibrate the Murakami-Mizuno formula for each collision interaction individually. This is something we did not do in our study, but it would be a viable approach. Besides that, we think that the variance ansatz is in some sense a generalization of the idea of Murakami and Mizuno and we tried to give them credit for that in the paper.

Reviewer: *Similar to the above comment, in the context of self-collection and Fig. 8, it would be useful to include comparisons using the formulation of Verlinde (1990) [and Pasarelli (1978)] for comparison.*

Reply: Yes, this would indeed be useful. We tried to implement the analytic solution of Verlinde et al. (1990) for selfcollection, but unfortunately it did not work out in the short time that we have for the revision. Now we show the numerical solution using power law fall speeds in Fig. 8. This numerical solution should be very close to the analytic solutions of Verlinde et al. (1990) and Pasarelli (1978) and we say that in the paper.

Reviewer: *p. 5079, line 13, An alternative to the Wisner approximation was suggested by Seifert and Beheng (2006). It seems a bit unfair to exclude Murakami (1990) and Mizuno (1990) at this point, who earlier suggested alternatives.*

Reply: Agreed, we now mention the Murakami and Mizuno papers already in the introduction. As mentioned before we feel indebted to those authors, because the formulation of the variance approximation of SB2006 and also the current paper was very much inspired by their ansatz. In fact, the variance approximation was the result of an attempt to derive equations that look similar to the Murakami-Mizuno formula.

Reviewer: *Reference to Brandes et al. (2007) J. Appl. Meteor. Clim. may also be appropriate here.*

Reply: We have included the reference to Brandes et al. (2007), but it is actually pretty hard to say who was the first to suggest or use this type of geometry. Locatelli and Hobbs (1974) give empirical relations that come very close to mass being proportional to D^2, for example, their aggregates have an exponent of 1.9.

Reviewer: *p. 5098, line 10, ... most double-moment schemes . . . apply the analytic solution of Verlinde et al. (1990). What about that of Passarelli (1978) for snow self-collection?*

Reply: Passarelli (1978) is now cited in this paragraph.

Reviewer: *5101, line 11, Given the numerous uncertainties and assumptions in such schemes, . . . error [below 10 %] seems acceptable. The implication here is that the errors in the other schemes, around 20-25 % are not acceptable due some (arbitrary) threshold of acceptability between 10 and 20 %. Given that these uncertainties and assumptions are in fact huge, it would seem that an error of 20-25 % is in fact very acceptable that a reduction to 10 % represents a change that resides in the noise. Perhaps these error metrics are somewhat masking the real gain with the proposed analytic solutions, in the regions where the collector and collectee fall speeds are simi- lar. I think inclusion of the two metrics is fine, but too much emphasis on those numbers might obscure things a bit.*

Reply: We have rewritten this paragraph and put less emphasis on the actual numbers.

Reviewer: *In the legends in Figs. 4-7, it says the straight lines are for numerc . This looks like it may be a typo.*

Reply: We were not able to find this typo in our version, but will pay close attention to this in the final typeseting.