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Geoscientific Model Development Discussions

Interactive comment on "On the analytic approximation of bulk collision rates of non-spherical hydrometeors" *by* A. Seifert et al.

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GENERAL COMMENTS

This article examines some of the deficiencies in commonly used analytic approximations for collection rates between hydrometeor species in current bulk microphysics schemes and proposes alternative analytic equations which better account for variations in terminal fall speeds with size and variations in hydrometeor aspect ratios (i.e. non-sphericity) with size. The manuscript is very well written and clearly presented, in terms of both the messages presented and the scientific content and relevancy. This paper is makes a useful contribution to the cloud modeling community. I find no fault with any of the analysis or conclusions, other than a slight difference of opinion related to the use of look-up tables (discussed below), and would essentially recommend that

the article be accepted after a few very minor changes. I do have some suggestions for some additional analysis which I believe would highlight more clearly the importance of the proposed analytic approximations, making it a more useful paper. Overall, I congratulate the authors on an excellent and interesting paper.

SPECIFIC COMMENTS

1.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 correct 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 argument 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 problems 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.

2.The comparison between the collection equations for the various approaches, summarized 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 formulation, 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 forumations.

3.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.

2.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.

3.p. 5083, line 20. Reference to Brandes et al. (2007) J. Appl. Meteor. Clim. may also be appropriate here.

4.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?

5.p. 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 similar. I think inclusion of the two metrics is fine, but too much emphasis on those numbers might obscure things a bit.

TECHNICAL COMMENTS

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

Interactive comment on Geosci. Model Dev. Discuss., 6, 5077, 2013.