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Comment on acp-2021-844

Anonymous Referee #2

Referee comment on "Intricate relations among particle collision, relative motion and clustering in turbulent clouds: computational observation and theory" by Ewe-Wei Saw and Xiaohui Meng, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-844-RC2, 2021

Review of “Nontrivial Relations Among Particle Collision, Relative Motion and Clustering in Turbulent Clouds: Computational Observation and Theory” by Saw and Meng

This article reports on theoretical modeling of the clustering and relative velocity of inertial particles in homogeneous, isotropic turbulence. The crucial difference between this work and much of the inertial particle work in the literature, is that particles are allowed to collide and coagulate (coalesce). The theoretical model addresses the now-25-year-old suggestion that collision rate can be expressed in a generalized form, being the product of the radial distribution function (which represents the tendency of inertial particles to ‘cluster’ in turbulence) and the the mean radial velocity (which represents the tendency of inertial particles to have an average inward relative velocity). The theoretical effort rests on the output from direct numerical simulation (DNS) of the Navier-Stokes equations for a triply-periodic box of turbulence containing inertial particles with small Stokes numbers. The key, novel findings from the paper are: 1) accounting for collisions leads to an extreme drop off in the radial distribution function at small droplet separation distances, on the order of the droplet diameter (the collision scale); 2) an apparently empirical power-law fit to this drop off in the radial distribution function, together with assumptions about randomness and linearity of droplet relative trajectories, leads to expressions for the mean relative velocity that match the DNS results with reasonable success. Although there have been suggestions of finding 1 in the previous literature, the extension of drift-diffusion theory (Fokker-Planck method used by Chun et al.) to account for collisions provides important new insight into the fundamental link between the two contributions to the generalized collision rate. In short, the notion of independent contributions of radial distribution function and mean relative velocity is invalid. The findings presented here show how they are related to each other, which should lead to simplification of the general collision problem. Furthermore, the finding that a simple model of particle approach angles can successfully describe the behavior of the mean relative velocity is important.

The paper is technically sound, as far as I am able to follow the derivations, and the central findings are of relevance to the cloud droplet collision problem. Recommendations to improve the clarity and interpretation of the paper are given below.

Grammar check: The paper is written in a reasonably clear way, but there are many grammatical errors, especially agreement of case. Even an automated corrector like grammarly would significantly improve this.

Title: I don't find the "Nontrivial Relations" in the title very compelling. It's the authors' call on this, but I would recommend changing it so that the title conveys a clear physical meaning. For example, "Strong coupling of particle collision, relative motion and clustering in turbulent clouds".

Abstract, first sentence: The meaning of "the reverse effect" is unclear.

Abstract, second sentence: This is vague, please specify that DNS for St<1 and St<<1 are conducted (i.e., define "various cases").

Abstract, lines 9-10: It's strange to read that the "theory accurately accounts for the DNS results" but then that a phenomenological model is being developed. Perhaps this can be clarified.

Lines 25-29: It's confusing to discuss RDF and MRV before they are formally defined. I recommend keeping the discussion general here, and not referring specifically to terms that will be confusing.

Line 34: MRV as defined is averaged over all *approaching* particle pairs, correct?

Line 39: It would be more accurate to state that the other theories are similar, rather than equivalent.

Lines 83-85: How is the coagulation growth balanced to achieve steady state? Is there a loss term for the particles that are growing by coagulation?

Table 1: The use of dm is strange. Is there a compelling reason for this? The authors can do what they prefer, but using m or cm would be more typical. Also, in the caption, no units are given for d*.

Line 89: “closely tied to the fluid velocity…”

Lines 105 and 107: I recommend giving equation numbers for q_i^d and q_i^D so they can be referred to later. Specifically, there is no need to squeeze the definitions into lines 145-146, instead they can be cited.

Lines 112-113: I was confused by the Stokes numbers because of the way the sentence is written (it looks like the RDFs are for particles of different Stokes numbers). Reformat to make it clear.

Lines 130-133: It would help to move this sentence to earlier in the section where the figure is first introduced.

Figure 1 caption: Is there a reason for defining d_1 as 0.99d?

Equation 5: This is a repeat of equation 4, so it can be deleted. My sense is that lines 139-144 and the accompanying footnote can be moved to the previous section and the result would be much clearer.

Line 148: The meaning of “nontrivial effects” is unclear.

Line 151: Can a physical interpretation of the two A_{ik} terms be provided?

Lines 155-156: Is there a physical interpretation for why the drift flux is positive for St less than order 0.01?

Line 162: Define Ω.

Line 166: It is stated that the addition of the factor c_{st} is ‘crucial’… please provide an interpretation of what this represents. Is it purely empirical in its motivation?

Equation 8: Formatting is strange.

Line 187: The form of the 'ansatz' suggests that there is statistical independence between g_0 and g_s. Please discuss.

Line 193: Should it be St=0.054?

Figure 1b: The compensated g(r) for r>r_c is flat, so does that imply that c_{10} is 0 in for that part of the piecewise function? If so, this should be stated, e.g., that the form on line 189 only applies in a nontrivial way to the r

Figure 2a: The labels for the x-axis appear to be cut off. Also, the lines are difficult to see, the orange line isn't visible for small r.

Figure 2: Can an interpretation be provided for why there is so little difference for the large and small-r limits of the integral in Equation 8?

Line 205: It is stated that the agreement between DNS and predictions is 'remarkable', but is it really? The theoretical treatment is pretty dense, so it's difficult to come away with a clear understanding of how many adjustable parameters are included. The more adjustable parameters, the less remarkable agreement would appear to be. Some discussion of this point would be helpful in guiding the reader and helping to understand just how remarkable the finding is.

Lines 256-257: The breakdown of the separation paradigm is a significant finding. It implies that the problem is somehow simpler that a product of independent contributions to the clustering and the approach velocity. How reasonable is it to think about this problem now being reduced in complexity?

Section 5: It would be useful to discuss the potential role of micro-hydrodynamic interactions between particles. Those have not been included, but one would assume that they will become significant at nearly the same scales as the currently observed adjustments to the RDF and MRV. One possible implication is that collision-efficiency effects will influence the RDF and MRV in a coupled fashion, rather than independently.

Line 263: The statement "we uncovered the unexplained accuracy of the differential drift-diffusion equation" is vague, so please clarify.

Supplement: The title is incorrect… should be updated to current version.