Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-19-AC1, 2016

Atmospheric Chemistry and Physics Discussions

Open Access

Interactive comment on "Reynolds-number dependence of turbulence enhancement on collision growth" *by* Ryo Onishi and Axel Seifert

Ryo Onishi and Axel Seifert

onishi.ryo@jamstec.go.jp

Received and published: 9 May 2016

We appreciate your positive and insightful comments. Below we answer all the questions one by one.

(1) The authors do not sufficiently discuss, from a physical perspective, how/why changing the Reynolds number might change the collision behavior. The authors should include a discussion, based on modern results (e.g. see Extreme events in computational turbulence, Proc. Natl. Acad. Sci. USA, 2015, Yeung et al.), of how turbulence changes structurally/statistically when the Reynolds number increases, and how this might affect the collision behaviour.

The physical perspective was discussed in Onishi and Vassilicos (2014) as described in subsection 4.1: Onishi and Vassilicos (2014) clarified that the Reynolds-number dependence of g11 observed for $1/3 < St < 1$ is due to internal intermittency of the three-dimensional turbulence. Onishi and Vassilicos (2014) proposed a plausible mechanism that can explain the Reynolds-number dependence for $1/3 < St < 1$ by defining the local St (St*), via the local flow strain rate, based on K62. As the Reynolds number increases, an increasing part of space is dominated by small St*, which would decrease the clustering effect (g_{11}). As the area of St*$>$1 cannot efficiently increase g_{11}, the extreme local strain rates cannot tip the balance and overcome the reduction in g_{11} caused by the reduced values of local strain rates in most of the space. The proposed mechanism does not need the modern finding for the 'very extreme' events observed in Yeung et al. (2015), it just needs the K62 model for intermittency. As the physical perspective is fully discussed in Onishi and Vassilicos (2014), this manuscript avoids repeating it.

(2) Two papers recently appeared on the arXiv by Ireland et al. (arXiv:1507.07026 and arXiv:1507.07022) that use DNS to consider, from a fundamental perspective, how changing the Reynolds number of the turbulence affects particle collisions in turbulence. The authors of the present article should comment on how their results and conclusions compare with those of Ireland et al. This is particularly important since Ireland et al. suggest that the effects of Reynolds number on the collisions may not be so important.

As described in subsection 4.2.2 in Ireland et al. (arXiv:1507.07026), there is a significant discrepancy in Ireland's conclusions and ours. Our DNS shows a decreasing trend of the clustering effect over the range 81$< Re_\lambda <$527 at St=0.4 and 0.6. However, Ireland et al. did not find such trend and concluded the Reynolds-number dependence of the clustering effect at low St is negligibly small. However, if we carefully look at

Figure 20(a) in Ireland et al. (arXiv:1507.07026), we can see a consistent decrease of the clustering effect at St=0.3, 0.5 and 0.7 when Re_λ increases from 224 to 597. As Ireland et al. (arXiv:1507.07026) shows the RDF in log scale, the decrease looks very small. But in linear scales the decreasing trend can be visible as in Rosa et al. (2013) as well as in our previous DNS studies. This manuscript can settle this dispute as Figure 2 clearly explains that the Reynolds-number dependence is significant when we discuss the large Reynolds numbers as observed in turbulent clouds (although it would not be visible, particularly in log scales, when we discuss the limited range of Re_λ <600).

- Reference: Rosa et al., Kinematic and dynamic collision statistics of cloud droplets from high-resolution simulations., New J. Phys., 15, 045032 (2013)

(3) In the DNS simulations, periodic boundary conditions are used and the particles are subject to gravity. In Ireland et al. (arXiv:1507.07022) it is shown that the simulation box needs to be quite large to avoid errors associated with the settling particles looping through the periodic box during the integral timescale of the turbulence. The results of Ireland et al. seem to show that for simulation domains of the size used in the DNS in the present article (2πL0) such errors could be significant. Can the authors comment on this? How might such errors influence the results and conclusions of the present article?

As noted in Woittiez et al. (2009) and discussed in Appendix A in Ireland et al. (arXiv:1507.07022), the periodicity may lead to errors for the settling particles with large St. Ireland et al. (arXiv:1507.07022) defined the critical St, St_{crit}, as $St_{crit} = Fr\,\frac{L}{l}\,\frac{u'}{u_\eta}$, where Fr is the Froude number (=a_η/g, where a_η is the Kolmogorov-scale acceleration), L (=$2\pi L_0$ in this study) is the domain size, l is the integral scale and u_η is the Kolmogorov-scale velocity. For St larger than St_{crit}, the periodicity problem may arise. Figs. 4, 9 and 10 are for settling particles. For those figures, we have calculated Stcrit to check the periodicity problem. (i)For Fig. 4, St_{crit}=3.7, which corresponds to r_{crit}=75um; r_{crit} is the radius of particle with St=St_{crit}. The two plots from DNS, which correspond to r_2=80um and 120um, exceeds r_{crit}. However, since the two plots are more or less similar with the gravitational (Hall) kernel values, the turbulent contribution would be small compared to the gravitational settling contribution. Thus the error due to the periodicity would not significantly affect the results. (ii)For Figs. 9(a) and 10(a), r_{crit} are 50, 65 and 70um for ϵ=100, 400 and 1000 cm^2/s^3, respectively. For Figs. 9(b) and 10(b) r_{crit} are 65, 75, 85 and 90um for Re_λ=66.1, 127, 206 and 333, respectively. The enhancement factor E_{turb}, shown in Figs. 9 and 10, was evaluated by $t_{10\%}$, which is defined as the time required for a cloud to convert 10% of its cloud mass into rain category drops. The threshold between cloud and rain categories was set at r=40um. That is, 10% of particles, in mass and volume, are larger than 40um in radius at $t = t_{10\%}$ by definition. For example, according to the DNS results, 3% of particles are larger than 50um and only 0.9% of particles are larger than 60um at $t = t_{10\%}$. The percentage of particles that are larger than 50um in radius may have some impact on $t_{10\%}$ and consequently Eturb. In this sense, the plot for ϵ=100 cm^2/s^3 in Figs. 9(a) and 10(a), whose r_{crit} is 50um, may contain some error associated with the periodicity problem. However, since E_{turb} for the plot is nearly unity indicating small turbulence enhancement, the periodicity problem does not change the present findings. Overall, the periodicity problem does not seem significant for the present manuscript, but it is worth mentioning. The above discussion has been added as Subsection 4.6: Periodicity influence.

(4) In section 2, I could not see any explanation regarding what particle equations of motion these collision kernels relate to?

Section 2 describes the collision statistics in general, irrespective of the governing equations of particle motions.

(5) Regarding equation 26, the authors make no mention of the validity of such an equation of motion. What about nonlinear drag effects, or finite particle sizes for the larger St particles?

The nonlinear drag effect is included in Eq.(26); f shows the nonlinear drag coefficient. The finite-size effect is, however, ignored. Accordingly, we have added the following sentence in the last part of the corresponding section:
"It should be noted that Eq. (26), which adopts the point-particle assumption, is inaccurate for large St particles whose radii are not small enough compared to the Kolmogorov scale."

(6) Regarding equation 10; presumably this model was derived for the case without gravity. Recently published results show that for St$\geq$O(1), the scaling of the RDF power law exponent with St differs significantly with and without gravity (with gravity it varies vary slowly with increasing St for St$\geq$O(1), and definitely not like St^2. Could the authors comment on this?

Yes, Eq(10) is for the case without gravity. The gravity can alter the clustering effect leading to some errors in our g(R) model. The gravity influence can be significant for large droplets. For the cloud system containing such large droplets, collisions due to the settling velocity difference would be more important than those due to turbulence. This can probably mask the insufficiency of our g(R) model. It actually did for the present work as shown in good agreements between our kernel model and DNS results. For the case without gravity, the results from Ireland et al. (arXiv:1507.07026) supports Eq. (10). It should be noted that Eq. (10) is the model for the RDF at contact, i.e., $g_{11}(x = 2r_1)$, not for the RDF, which is the function of the radial distance x. It should be also noted that the power law exponent is not the only measure of the RDF at contact, and the coefficient C_0 is also the key parameter. Reade & Collins formulation leads to

$$g_{11}(r) = C_0(\frac{\eta}{r})^{C_1} \propto C_0 St^{-C_1/2}.$$

For example, if we look at the data for Re_λ=224 in Figure 22 in Ireland et al. (arXiv:1507.07026) C_0 and C_1 are 6 and 0.45, respectively, for St=2, and 4 and 0.3 for St=3. Substitutions of these values into Eq.(A1) yield g_{11}=5.1 for St=2 and 3.4 for St=3, leading to $\{g_{11}(St=2)-1\}/\{g_{11}(St=3)-1\}$=1.7. This value is not far from the prediction 2.25 from Eq. (10).

(7) Where does equation 21 come from? What are the assumptions behind this?

Onishi et al. (2009) derived Eq. (21). We modified the sentence that includes Eq. (21) into *"Onishi et al. (2009) modeled the enlargement of the relative particle relaxation time by gravity as* ..." The detail derivation is described in Onishi et al. (2009) and thus the manuscript avoids repeating it.

(8) Can the authors include error bars on some of their plots? This would help to show the statistical significance of the argued Reynolds number dependencies of the collision statistics.

Accordingly, we have added the error bars in Figures 2 and 4, and the corresponding explanation in the captions.

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