Reply to Referee #1

We appreciate your additional comments. Below we answer all the questions one by one.

(1)Concerning Figure 2.
(1-1)First, I realized that the text in the paper does not actually refer to this figure at any point (that I could see). I wonder whether the line below equation 42 is supposed to be referring to figure 2 and not figure 3?

>>Yes. It has been modified in the revised manuscript.

(1-2)Second, the results in Fig 2 suggest that in going from Re_lambda=O(100) to Re_lambda=O(10000), the RDF at contact can vary significantly if St is large enough. Yet the data for Re_lambda>O(1000) is based upon a model that attempts to capture the effects of intermittency, and not DNS data. Being a model, the results are open to question. On the other hand, in Ireland's recent JFM paper (Journal of Fluid Mechanics, Volume 796, June 2016, pp617 - 658, which is the published version of the arXiv paper I referred to in my first review), in Fig23, their DNS results show that the effect of Re_lambda on the RDF saturates as Re_lambda is increased. This is in contrast to the assertions of the present paper that claims in Fig 2 (using a model) that increasing Re_lambda continues to decrease the RDF up to Re_lambda=O(10000). The authors need to discuss this difference in detail. Since the Re_lambda dependence of the collision statistics is in fact the main topic of the present paper, the authors need to seriously consider how to explain the discrepancy between their results and those of Ireland, e.g. whether the effect of Re_lambda saturates or not etc. The authors should consider how the different numerical schemes employed in the two studies may be responsible for the discrepany etc.

>>We've included the data of Figure 23 of Ireland's paper (JFM, 2016, vol. 796, pp.617-658) in Figure 2 of the revised manuscript and revised the discussion for the figure at the end of Subsection 4.1. The updated figure clearly shows that both of ours and Ireland's results show a decreasing trend for St=0.6 for Reλ>200. The two DNS data are consistent (this should be emphasized), but the interpretation is different between our paper and Ireland's. We need further data for high Reynolds numbers to conclude which interpretation is actually correct. One thing to be noted is that we have proposed the plausible mechanism that the flow intermittency can cause the Re dependency (Onishi and Vassilicos, 2014 J. Fluid Mech.) and Figure 2 actually shows good agreement between the prediction and the DNS results.
Even the saturation of g11 for St=1 at Reλ=500 is fully consistent with our DNS data and the Onishi model of g11. The Onishi model does predict a

decrease in g11(St=1) for Reλ larger than 1000, though. Making predictions for the behavior outside of the range of available DNS data is, in our opinion, valuable, because it allows for a falsification (or validation) of the model when DNS at even higher Reλ becomes available. Obviously such predictions for high Reλ are also necessary to apply the results to atmospheric flows like clouds.

(2)Regarding section 2, the authors stated in response to my first review that section 2 is general, and is not specific to the equation of motion for the particles. But this is not correct, for example, as I pointed out in my initial review, equation 10 is only valid for monodisperse inertial particles, subject to Stokes drag only without gravity. So how then can section 2 be general, and not specific to the equation of motion for the particles?
>>We meant Subsection 2.1 is general. Sorry for our misunderstanding of your comment. Yes, Eq. (10) is developed based on the DNS for non-settling monodisperse particles. We have modified the sentence preceding to Eq. (10) as "*Onishi et al. (2015) proposed an original model for the clustering effect in monodisperse systems of* <u>*non-sedimenting particles with Stokes' linear drag*</u>."

Reply to Referee #2

Thank you again for your insightful comments.

(1) Eq. (21), in the limit of $s_v \rightarrow 1$, $\theta_{i,sed} \rightarrow s_v \theta_i / \sqrt{3}$. Would the factor $\sqrt{3}$ present a problem?

>> The factor $\sqrt{3}$ comes from the underlying assumption that the particle velocity fluctuation is isotropic (i.e., the fluctuations in the vertical and horizontal directions are the same). This assumption is invalid for large $s_v>4$ (Onishi et al., 2009 *Phys. Fluids*). But, it does not cause a problem for this study since the particle velocity fluctuations become negligible anyway and the turbulence effect on collisions become insignificant consequently.

(2) Bottom of page 8, the statement "This simple form is exact ...." is certainly not true. Eq. (22) is one way to combine two mechanisms, and they are many possibilities.

>> We have simply removed the sentence to avoid ambiguity.