Associate Editor Decision: Publish subject to minor revisions (review by Editor) (14 Mar 2017) by Prof. Daniel Parsons Comments to the Author:
The authors have made a number of modifications based on the comments of the reviewers. This has tightened the paper. There do however remain some areas highlighted by the reviewers that the authors have not yet adequately dealt with in my view (A-D). I judge these to be technical in nature and not too taxing - thus I am satisfied that this does not need to be further-reviewed beyond me as AE. I ask for a further set of changes in response to A-D below and a point-by-point list of changed made to incorporate these changes into the next submission version.

A) CLEAR WATER CONDITIONS INDICATE THE SCALE OF FTURB. (Point 7, Reviewer 2 also) > Can you add a few sentences and beef up the "justification" to satisfy this point more fully please - several of the reviewers raise this point and it is not really dealt with in the revisions. It is a basic assumption that is likely not valid - so you need to address this please.

> We have emphasized this aspect in the following locations:
> P5L4-5
> P5L14-L33

B) TURBULENCE EXTINCTION OCCURRS AT LOW AND HIGH ABSOLUTE CONCENTRATIONS > Can you add a sentence on this in the introduction also - there are a few papers you could cite there in a background review that would make this section easier to manage and introduce.

> We have added this insight as a main result of the paper in the introduction, with references to the supporting DNS and experiments (P1L37-P2L3).

C) Referee 2 Pt 2 - QUADRATIC
his is an important point that is not adequately dealt with yet. Please revisit this.

> The associate editor has urged us to consider this point again. This prompted us to review an authoritive textbook on the subject of turbulent scaling: *"Turbulent Flows."* By Pope [2000]. It turns out that this text does contain a fundamental theoretical consideration for the leading order dependency of w^+ on z^+. However, application of the theory does not lead to the exponential scaling hypothesized by the two referees. Moreover, comparison between the theory and the benchmark data is non-trivial. There is significant misfit between the theory of Pope, and the benchmark data of our Fig. 2. Main reason for this difficulty is that the proposed theoretical scaling (Eq. 7.61 in Pope, 200) depends on only the first term in a Taylor expansion of the spatial field of the Reynolds stresses. Many more terms would need to be incorporated in the theory to approach the benchmark, but this would lead to mathematical considerations of Reynolds-stress fields that are beyond our contribution. We are convinced that the wholly empiric approach we have followed in the assumption of the form of Eq. 7 leads to a correct numeric approximation of the distribution of $w^+(z^+)$, as evidenced by the success of Fig. 2.
> We have added explicit sentences describing the pragmatic empiricism of our Eq. 7 (P4L5-7 & P4L15-17).

D) Referee 2 PT 5 - GRAIN SIZE
I do not agree that this is beyond the scope. Please revise the paper to include points pertaining to this advance - sight of the discussion around this is sufficient rather than additional results - but this is novel and will make the paper stronger.

> We have now included this insight in the main text of the particle size discussion (P7L31-P8L4); and we have acknowledged the anonymous reviewer as a source in the Acknowledgment section (P13L8-10).