Review of "Impact of high and low vorticity turbulence on cloud environment mixing and cloud microphysics processes" by Kumar et al. (acp-2021-101)

The authors have addressed most of my comments with care. While I have some very minor comments below, the manuscript is generally ready for publication. I do not need to see it again.

Line numbers refer to the tracked-changes version of the manuscript.

Minor Comments

Ll. 91 – 94, 294, 331: Since the mono-disperse case is not discussed in the revised version, I suggest removing all remaining references. (Alternatively, one should state clearly in the conclusions section that the mono-disperse case is not discussed in the paper.)

Ll. 143 – 145: I fully agree that the volume fraction of 50 % is unrealistic. However, assessing the volume fraction of high vorticity regions has never been an objective of Squires and Eaton (1991) and Shaw et al. (1998). Therefore, I suggest writing "assumed high volume fraction" instead of "finding of high volume fraction".

L. 171: Writing about a (singular) instability feels odd. I suggest using the plural ("instabilities").

Ll. 218 – 220: By showing the relative dispersion in Fig. 6, one could strengthen the argument here: Broadening is always stronger in the high vorticity regions.

Ll. 263 – 269: Technically speaking, the number density should be constant in the homogeneous mixing regime. Accordingly, the statement "a turn toward the homogeneous mixing regime where both the number density and the mean volume radius decrease rapidly" is slightly misleading.

Technical Corrections

L. 6: Replace "DNS" with "direct numerical simulation".

L. 137: "the required cuboid" or "a cuboid"

Caption of Fig. 3: "KE", not "K.E."

L. 264: "till" is informal.

Ll. 263 – 264: Please rewrite this sentence. (Maybe: "In the LV case, mixing is inactive for the first 1.2 s, and for 1.4 s in the HV case.)