Review

The effects of turbulent collision-coalescence on precipitation formation and precipitation-dynamical feedbacks in simulations of stratocumulus and shallow cumulus convection

by C. N. Franklin

General comments:

The manuscripts presents a study of the effect of turbulence on the rain formation in convective clouds. Based on earlier results of the same author an autoconversion scheme which takes into account the effect of turbulence in terms of the Taylor microscale Reynolds number is applied to the simulation of shallow convection and stratocumulus. It is claimed that taking into account the turbulence effect has a significant impact on both shallow cumulus as well as stratocumulus. For the shallow cumulus regime this has previously been shown by other groups and is confirmed here. For stratocumulus the results require clarification.

Major comments:

1. The autoconversion scheme, Eq. (1), requires maybe some clarification. In general the dissipation rate ϵ and the Taylor microscale Reynold number Re_λ are two independent variables. In the Wang-Ayala parameterization the turbulence effects on collision-coalescence depend on both of these variables, but the dependency on ϵ is dominant. Also in terms of scaling theory one would probably first make the collision rate non-dimensional with help of the dissipation rate and the resulting quantity would then be related to the Taylor microscale Reynolds number. As a result, the turbulence effect would again depend on both, dissipation rate and Reynolds number. For the extrapolation or scaling of low-Re_λ DNS it seems crucial to properly separate the dependencies on ϵ and Re_λ and later specify them for atmospheric conditions or parameterize them in terms of other quantities like TKE. Why does Eq. (1) only contain Re_λ and not ϵ? What assumptions have been used to eliminate ϵ? Has Eq. (4) of Franklin (2008) been used for that? Is this equation general enough to achieve proper scaling? To me it looks somewhat empirical and is not even correct in terms of dimensions, i.e., it relates a non-dimensional number, Re_λ, directly to dissipation rate. This leaves some doubts whether this parameterization can properly represent the dependency of the autoconversion on the dissipation rate, which is of course crucial for the different cloud regime like shallow cumulus (high dissipation rate) and stratocumulus (much lower dissipation rate).

2. On page 2289 in section 4 it is argued that the CDNC or size of the cloud droplets affects the evaporation of cloud droplets. This is of course true in nature and has been postulated as an aerosol-cloud effect by Xue et al. (2008). Unfortunately, this effect can not be used to explain the results of the present study, because the bulk microphysics assumes equilibrium between water vapor and liquid water (as the model uses. e.g., liquid water potential temperature as prognostic variable). Therefore this model does not include the

CNDC dependency in the cloud drop evaporation rate. It might be more likely that the non-monotonic behavior in Fig. 9 is a purely statistical artifact.

3. In my opinion, the results presented in Fig. 7 show that the parameterization uncertainties for a bulk scheme in the stratocumulus regime are much larger than the effect of turbulence. Given the uncertainties of the parameterizations one should be very cautious in using this results as proof for the effect of turbulence in this cloud regime.

4. The study uses a rather old version of the UCLA-LES code which is, for example, still based on leapfrog time integration. Since about 2009 the official UCLA-LES code is based on a Runge-Kutta scheme. Why has this very old version been used? The most recent version can be easily downloaded from https://gitorious.org/uclales even without signing up at Gitorious. Using this outdated version is a missed opportunity, because the more recent model versions (since 2010) include the turbulence effect in the SB autoconversion scheme based on the parameterization of Seifert et al. (2010). It would have been easily possible to compare the results from both approaches. This would have made the study a much stronger one, especially for the stratocumulus regime in which the parameterization uncertainties are large. Why not repeat the study with the current version of UCLA-LES and with both autoconversion schemes? Would it be possible to include this in a major revision of the manuscript?

Minor comments:

1. I would recommend to mention the recent study by Kunnen et al. in the introduction as confirms the results of Franklin et al. (2007) and therefore supports the current study.

   R.P.J. Kunnen, C. Siewert, M. Meinke, W. Schröder, K.D. Beheng, Numerically determined geometric collision kernels in spatially evolving isotropic turbulence relevant for droplets in clouds, Atmospheric Research, Volume 127, June 2013, Pages 8-21, doi:10.1016/j.atmosres.2013.02.003

2. page 2280, line 27: typo Smagorinksy

3. page 2281, line 4 and later: The microphysics scheme of Savic-Jovcic and Stevens (2008) assumes a threshold of 25 μm between cloud and rain water. The Franklin (2008) scheme uses 40 μm. Which value is used here?

4. Why does Fig. 7 show KK for comparison, but Fig. 3 shows SB? Why not both, KK and SB, in both regimes?