The authors have made considerable revisions to the manuscript, which have certainly improved it significantly. Nevertheless, I still feel there is a strong need to provide more physical insight into the simulations before the work can be published. I focus on this aspect in my reply, and leave most minor details for a later point.

- The authors state in their reply that the article focuses on a general comparison between the three core types for large statistics of clouds rather than on increasing understanding of core dynamics. I think a too strong focus on the statistics per se rather than the underlying physics and implications is not very fruitful. However, in putting more emphasis on the core-shell model the authors have provided a good starting point for further analysis.

Still, I find it hard to square the results on the core-shell model here with previous studies, which do strongly point to the existence and influence of the shell. Compensating subsidence needs to take place somewhere, likely in the vicinity of the cloud, during the entire cloud life cycle and at all levels. One explanation may be in the fact that the shell is partially located in a non-cloudy area, and therefore not diagnosed in the current framework. For the 3D simulations, the irregular (fractal) shape of the cloud could also play a role.

It could indeed be that the static core-shell model has deficiencies, but is there an alternative model that performs better? For example, previous studies have suggested the warm and buoyant air is part of a vortex-ring, which may not occur at the center of the cloud. Does this hypothesis fit with the results from your simulations? The relative roles of this vortex ring circulation and evaporation in establishing the toroidal circulation that forms which is mentioned in line 424 have not been fully worked out, as far as I know.

- One of the main questions in the new draft is whether the core definitions can be used interchangeably. This is a relevant question, which can help decide how to model clouds (see also W. Hannah, Entrainment versus dilution in tropical deep convection, JAS, 2017, this is a relevant article which I probably should have mentioned explictly before). Since the buoyant cores are only a small part of the cloud, it may be argued they are really different from the other cores. What probably plays a role is that the largest part of the cloud is not relevant at all to mass, moisture and heat transport (though of course it is for radiative properties). The regions outside the buoyancy core carry hardly any net mass-flux and smaller perturbations of temperature and moisture content from the environment. This would be something worth analysing: possibly, even some of the region identified as buoyant core here is only marginally buoyant and not contributing much to the various fluxes. Regions with both net condensation and positive buoyancy would be a further subset to look at.

- The authors have improved the connection to the literature regarding mixing and updraft dynamics. What is still missing is a discussion of previous work on the cloud life cycle and how this fits with the current results. In particular, the studies of Heus et al. (2009b) and Dawe and Austin (2013) already cited are relevant here. The work by Hannah also discusses the life cycle of clouds and the corresponding buoyant cores in detail.

- There is still a large emphasis on adiabatic theory. Although the authors admit it is simplistic, this is an understatement of the extent to which adiabatic theory fails to predict e.g. the vertical velocity and specific humidity in the cloud. It is really important to expand on the description of the role of mixing, dilution and drag. For example, in line 249, the authors write “The growing branch deviates from the adiabat at large masses depending on the degree of sub-adiabaticity of the cloud field.” This sounds a bit tautological and does not refer to the processes at play. Another minor point where mixing comes into play is in line 252, where it is suggested that parcels could take the reverse trajectory in the CvM space. In a situation with realistic mixing, however, this could never occur as the resulting parcels would rapidly become positively buoyant again as they descend.

I find the remarks on the sink term around line 405 confusing as well. Again, the role of mixing is key to the actual liquid water specific humidity.

Similarly, when the vertical velocity is discussed in the conclusions, drag is not even mentioned. For cumulus clouds, drag (and to some extent mixing) are crucial in much the same way drag is a first-order process when describing how a feather falls through air (gravity alone explains the direction of movement and sets an upper limit to the fall velocity, but the actual fall velocity of the feather is extremely poorly explained by gravity alone). The drag force is not analysed, and its potential role in setting the shape of the w-cores is not discussed at all.

- One of the remarks about the time-scales of entrainment/mixing being of the order of seconds (line 339) is confusing in my opinion. Yes, these time-scales represent the mixing on the smallest scales, but this mixing would not occur without the entrainment of air into the cloud by larger-scale/longer time-scale eddies.
- I recognise there is value in the CvM diagrams used here. However, these contain many points and it is unclear which clouds actually contribute significantly to cloud volume or mass transport, as the (mean effective) radius of the cloud is factored out and only mean liquid water path is concerned. Possibly a selection of clouds could be shown, with point size proportional to radius. What is also unclear to me is how core fractions and centroids are determined once there are multiple cores in a single cloud. The shading in figure 4 needs a more precise definition in the text as well. Please make it clear in line 444 that mass refers to mass of the liquid phase.
- The remarks in line 213-216 on thresholds and variance (of what?) are unclear to me.
- As far as I am concerned, the appendices are superfluous, as they demonstrate what previous studies have found already. This is particularly the case for the second appendix, which mostly shows that buoyancy mixes nearly linearly if two parcels are considered.
- It would also be good to go through the draft again and check for e.g. misplaced brackets, spelling/style issues, legend/colorbar placement in figures, the use of LWC rather than q_l and formatting of references.