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Interactive comment on "Core and margin in warm convective clouds. Part I: core types and evolution during a cloud's lifetime" *by* Reuven H. Heiblum et al.

Anonymous Referee #1

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This article provides an analysis of the structure of cumulus clouds and the connection between different regions in cumulus clouds. It defines three different regions in the cloud: 1) a Relative Humidity core, where the air is fully saturated, 2) A buoyancy core, where the air is positively buoyant and 3) a vertical velocity core, defined by upwards motion. The authors argue that typically the buoyancy core is a subset of the relative humidity core, which again is a subset of, or at least smaller than, the vertical velocity core. They also consider the effect of mixing on buoyancy and the existence of overshoots as an explanation for this result.

I have three key concerns about the draft as it stands, which I think would need to be

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addressed before publication. More detail on each of these points can be found further below.

1) The main conclusions are not sufficiently novel.

2) There has been significant work on mixing in cumulus clouds in general and on the role of processes at the cloud edge in particular that the current study does not refer to.

3) I have some major concerns about the analysis framework.

Despite the fact that I am critical of the theory and analysis as it stands, I think there is value in analysing the simulations presented here in detail, particularly because many previous studies that have worked on this topic have used an approach to condensation based on immediate saturation adjustment in each grid cell. The use of a spectral bin-microphysics model in the context of this a study is therefore valuable, although the difference between the two approaches is not made clear to a sufficient degree. I also realise that there is a follow-up article which may include novel results that might be partially supported by the present work.

I would encourage the authors to either fully revise the study, or to incorporate the parts of the study that are needed to support part II into that article, but still make major changes to these sections to address the concerns below. If the material is not incorporated into part II, it would require a more significant overhaul of the draft than would usually be considered a major revision. However, many previous studies have looked into mixing in cumulus in further detail using SAM, so the authors should be well-placed to improve their analysis. The draft is also generally well-written, so some of the material could likely be reused.

1) The conclusions are not sufficiently novel.

The two main conclusions are not really new results.

- The role of mixing in rapidly reducing cloud buoyancy has been established in previous theoretical work: e.g. Morrisson (2017) looks into this in detail. This role of mixing in rapidly reducing buoyancy is incorporated in at least three existing parametrisations of cumulus convection (see Kain and Fritsch, 1990; De Rooy and Siebesma, 2008 and Derbyshire et al., 2011). Moreover, the framework of critical mixing fraction used in the studies of Kain and Fritsch and De Rooy and Siebesma already provides a framework for understanding how mixing impacts on buoyancy, and the result that mixing generally reduces buoyancy is well established. There are also a number of existing diagrams that have been used to understand the thermodynamic properties of cumulus clouds, which have also shown mixing generally leads to a region of negative buoyancy (Paluch, 1979; De Roode, 2007). The fact that mixing between core and margin parcels leads to intermediate buoyancy in a near-linear way can also be deduced from previous work (e.g. Pauluis and Schumacher, 2010).

- Similarly, literature on the existence of convective overshoots dates back as far as at least Betts (1973).

In order for additional theory to be valuable, it should therefore include predictions that can be directly and quantitatively compared against Large-Eddy Simulation.

2) Lack of connection to the existing literature. I am citing a few key studies below but this is by no means a comprehensive list.

- Over the past two decades there has been a large number of studies on the role of negative buoyancy at the cloud edge (e.g., Zhao and Austin 2005; Jonker et al. 2008; Heus and Jonker, 2008), as well as on the implications of the cloud edge for determining effective mixing between clouds and their environment (e.g. Dawe and Austin 2011).

- Similarly, several studies have looked into the role of convective cores in simulations with cloud tracking (e.g. Dawe and Austin 2012, Heus and Seifert 2013), and used this to study the role of these cores throughout the life cycle of the cloud.

3) I have some major concerns about the analysis framework.

- One of my main concerns is that the theoretical arguments lean heavily on analysis of adiabatic parcels, with mixing as an afterthought. Previous studies suggest that adiabatic parcels do not occur in shallow and congestus cumulus (e.g. Romps and Kuang, 2010), and this seems to be the case in the current study as well. Note that liquid water path is plotted on a logarithmic scale, i.e. even the clouds that contain most liquid water contain several times less liquid water than they would in the adiabatic case. An adiabatic parcel model therefore offers only very limited insight into the dynamics of cumulus convection. Several approaches exist that better represent the effects of mixing throughout the cloud life cycle, e.g. continuous lateral entrainment (Lin and Arakawa 1997, Morrison 2017) or episodic mixing. In order for a theoretical framework to provide quantitative predictions that can be tested against LES, one would likely need such an approach. Moreover, it is clear from a number of recent publications (De Roode et al. 2012 is cited already, but see also Romps and Charn, 2015; Morrison, 2016) that theoretical models for the vertical velocity should incorporate the role of drag.

- The use of the liquid water path as a measure of cloud mass also seems more appropriate for stratocumulus than for cumulus convection, where clouds may be slanted and mixing might lead to lateral growth. Referring to the mean liquid water path as the cloud mass is confusing. Instead of liquid water path, a tracer concentration could be used (e.g. Romps and Kuang, 2010) to provide a more robust measure of dilution.

- The analysis in this study mostly considers convective elements as a whole, or fractions of pixels within an element (figure 2 is an exception here). This provides limited insight into the dynamics of the different cores. Previous studies have provided a more detailed analysis into the circulation around rising cumulus clouds (see, e.g. Blyth et al, 2005; Peters, 2016). Considering the different regions identified in these previous studies would be another way to obtain novel results that go beyond the current conclusions.

- The thresholds used in the analysis should be discussed further. One of the risks of only considering zero thresholds is that passive regions of the cloud with marginal updraught velocities or regions where gravity waves lead to upwards motion are included in the analysis. Some previous studies have addressed this issue by looking into streamlines (e.g. Romp and Charn, 2015), however, this might not be very straightforward to implement. Alternative approaches would be to determine characteristic updraught values of buoyancy and vertical velocity, or considers multiple thresholds.

- The domain used may be too small for the Amazon simulations. In order to check this, one would need to check that the cloud top is sufficiently far removed from the domain top, and that convective cold pools are not dominating the spatial organisation of convection.

Besides these three major concerns, I have some smaller points of criticism, some of which are cosmetic, which would also need to be addressed:

- Equation 2 seems to be based on a parcel that is not mixing with its environment (see my main concern 1 above).

- The presentation style of figures needs improvement. For example, in figure 2, some of the contours overlap, which makes the current presentation confusing to the reader. Some figures are also too small in my opinion (figure 2 is an example here).

- Equations should avoid the use of acronyms, such as LWC (in any case, is this specific humidity or mixing ratio?)

- Some of the terminology is unclear: are cloud growth/cloud suppression regions simply regions of net increase/decrease of supersaturation, or is something beyond this meant? It is important to point out that some of the regions that are subsaturated using a bin microphysics scheme would be diagnosed as saturated in an approach to condensation which performs immediate adjustment when defining the RH-core. Similarly, buoyancy is best defined with respect to the surrounding environment, rather than with respect to a reference profile.

- Parentheses should only be used around the year when an author is cited and the author name is part of the sentence.

- I could not find the reference to Dias et al. (2012). I have not done a comprehensive check for other missing references at this point.

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