<u>Reply to reviewers' comments on *"Core and margin in warm convective clouds. Part I: core types and evolution during a cloud's lifetime"*</u>

We would like to thank the reviewers for their insightful and helpful comments that help up improve and clarify the manuscript. Before answering all the reviewers' comments in details, we summarize shortly the main modifications done in the manuscript for addressing the main comments:

1. Focused emphasis is now put on the novel parts of this study: the differences between the three core types, their evolutions in time, and comparison to current understanding of core size and location within a cloud.
2. The goals of the work are stated more clearly, and the importance of bin-microphysics is discussed.
3. The introduction was revised significantly to include a more comprehensive review of relevant previous works and the physical processes associated with differences between the core types.
4. The theoretical section is presented as a summary of previous knowledge, with purpose to gain intuitive understanding of the differences between the core types.
5. One cloud field case study was replaced. The revised version presents a continental shallow cumulus convection case study, based on long term observations taken at the ARM Southern Great Plains site.

Please find below a point-by-point reply to all of the reviewers' comments.

<u>Reply to reviewer #1 – RC1</u>

General Reviewer Comment:

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, here 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 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.

General Answer: We thank the reviewer for the beneficial comments. We revised the paper to be clearer and more complete according to the concerns raised in the review. Please see all the details in the answers below.

Main Comments:

MC1) 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.

MA1) Thank you for this comment. We have revised the manuscript significantly so that clear emphasis is put now on the novel parts of this work: the differences between the three core types, their evolutions in time, and comparison to current understanding of core size and location within a cloud. We are not aware of previous work which tried to perform such a comprehensive analysis and comparison between the different cores. In addition, we describe better in the revised version the previous relevant studies which have dealt with positive vertical velocity and buoyancy in clouds and the effects of entrainment on them.

The new abstract now focuses on the novel aspects of the work:

"The properties of a warm convective cloud are determined by the competition between the growth and dissipation processes occurring within it. One way to observe and follow this competition is by partitioning the cloud to core and margin regions. Here we look at three core definitions: positive vertical velocity (W_{core}), supersaturation (RH_{core}), and positive buoyancy (B_{core}), and follow their evolution throughout the lifetime of warm convective clouds.

Using single cloud and cloud field simulations with bin-microphysics schemes, we show that the different core types tend to be subsets of one another in the following order: $B_{core} \subseteq RH_{core} \subseteq W_{core}$. This property is seen for several different thermodynamic profile initializations, and is generally maintained during the growing and mature stages of a cloud's lifetime. This finding is in line with previous works and theoretical predictions showing that cumulus clouds may be dominated by negative buoyancy at all stages for adiabatic and non-adiabatic clouds.

During its mature growth stage, the cloud and its cores are centered at a similar location. Additionally, at this stage all core types are of similar size and can be used interchangeably. During cloud dissipation the cores show less overlap, typically reduce in size, and migrate from the cloud centroid. In some cases, buoyancy cores can reemerge and often reside at the cloud periphery. Thus, the core-shell model of a positively buoyant center surrounded by negatively buoyant shell only applies to a fraction of the cloud lifetime."

We see merit in including the rather simple theoretical derivations in the text for the sake of completeness and ease of understanding for a reader who is not an expert in this specific field. We have revised the theoretical section so that it better reflects previous works (see the details in the next answer), but please note that none of those works focused on the relative sizes of the different cores.

MC2) 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.

MA2) We thank the reviewer for including such an extensive list of relevant previous literature that was indeed lacking in the original manuscript. We have included most of these references (and others) in the revised manuscript, and now try to connect out findings to previous literature wherever found to be of relevance. A few examples are given here, from the introduction:

"***The common assumption when partitioning a convective cloud to its physical core and margin is that that the cloud core is at its geometrical center and the peripheral regions (i.e. edges) are the margin. Previous observational (Heus et al., 2009a; Rodts et al., 2003; Wang et al., 2009) and numerical (Heus and Jonker, 2008; Jonker et al., 2008; Seigel, 2014) works have studied the gradients of cloud thermodynamic properties from cloud center to edge, and suggest that a cloud is best described by a core-shell model. This model assumes a core with positive vertical velocity and buoyancy, surrounded by a shell with negative vertical velocity and buoyancy. The shell is the region where mixing between cloudy and environmental air parcels occurs, leading to evaporative cooling → decrease in buoyancy → decrease in vertical velocity."***

Another part of the introduction:

"Based on previous findings, here we explore the partition of clouds to core and margin using three different objective core definitions where the cloud core threshold is set to be a positive value (of buoyancy, vertical velocity, or supersaturation). Cloud buoyancy (B) can be approximated by the following formula:

$$\boldsymbol{B = g \cdot \left(\frac{\theta'}{\theta_o} + 0.61q_v' - q_l\right)} \qquad \textbf{\textit{(1),}}$$

Where θ_o represents the reference state potential temperature, q_v is the water vapor mixing ratio, and q_l is the liquid water content. The (') stands for the deviation from the reference state per height (Wang et al., 2009). Buoyancy is a measure for the vertical acceleration and its integral is the convective potential energy. Latent heat release during moist adiabatic ascent fuels positive buoyancy and clouds' growth while evaporation and subsequent cooling drives cloud decay (de Roode, 2008; Betts, 1973). The prevalence of negatively buoyancy parcels at the cloud edges due to mixing and evaporation is a well-known phenomenon (Morrison, 2017). Mixing diagrams have been used to assess this effect (de Roode, 2008; Paluch, 1979; Taylor and Baker, 1991), and are at the root of convective parameterization schemes (Emanuel, 1991; Gregory and Rowntree, 1990; Kain and Fritsch, 1990) and parameterizations of entrainment and detrainment in cumulus clouds (de Rooy and Siebesma, 2008; Derbyshire et al., 2011). "

And theoretical section:

"***Hence, for the adiabatic column case, B_{core} is always a proper subset of W_{core} ($B_{core} \subset W_{core}$). These effects are commonly seen in warm convective cloud fields where permanent vertical layers of negative buoyancy (but with updrafts) within clouds typically exist at the bottom and top regions of the cloudy layer (de Roode and Bretherton, 2003; Betts, 1973; Garstang and Betts, 1974; Grant and Lock, 2004; Heus et al., 2009b; Neggers et al., 2007).***"

MC3) I have some major concerns about the analysis framework.

MC3.1) 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.

MA3.1) As expressed in MA1, the original manuscript was faulty in that it gave an impression that it focused on entrainment effects. So we have revised the manuscript to better explain that the theoretical estimations of entrainment effects only serve to give an intuitive understanding of the results found using LES simulations. We use simple dynamical concepts to explain the evolutions of the different core types in cumulus clouds. As written now in the beginning of the theoretical section: "***Here we propose simple physical considerations to evaluate the differences in cloud partition to core and margin using different definitions. The arguments rely on key findings from previous works (see Sect. 1) with aim to gain intuitive understanding of the potential differences between the core types***".

We are well aware that the use of an adiabatic cloud column (as done in the theoretical part) is simplistic, nevertheless, we think it manages to easily convey theoretical ideas and robust cloud field characteristics, such as convective overshooting. For the cloud field analyses (using the CvM phase space), the adiabatic curve is taken as a reference for the growth stage of clouds. We find that it makes it very helpful when trying to extract temporal information from those figures, as was shown in previous publications (Heiblum et al., 2016a, 2016b).

Regarding the importance of drag on vertical velocity, we address this point in the introduction: ***"Usually, the CAPE serves as a theoretical upper limit, and the vertical velocity is smaller due to multiple effects (de Roode et al., 2012), most importantly the perturbation pressure gradient force (which oppose the air motion) and mixing with the environment (entrainment/detrainment) (de Roode et al., 2012; Morrison, 2016a; Peters, 2016). Recent studies have shown that entrainment effects on vertical velocity are of second order, and a rising thermal shows a balance between buoyancy and the perturbation pressure gradient (Hernandez-Deckers and Sherwood, 2016; Romps and***

Charn, 2015), the latter acting as a drag force on the updrafts. Nevertheless, initial updraft magnitude and environmental conditions play a crucial role in determining the magnitude of mixing effects on buoyancy, and thus also the vertical velocity profile in the cloud (Morrison, 2016a, 2016b, 2017).”.

And in the theoretical section:

“***Given an initial vertical velocity of ~ 0.5 m/s, the deceleration due to buoyancy (and reversal to negative vertical velocity) should occur within a typical time range of 1 - 10 minutes. These timescales are much longer than the typical timescales of entrainment (mixing and evaporation that eliminate the*** B_{core}***) which range between 1 – 10 s (Lehmann et al., 2009). Moreover, the fact that a drag force typically balances the buoyancy acceleration (Romps and Charn, 2015) can also contribute to a time lag between effects on buoyancy and subsequent effects on vertical velocity. Therefore, the switching of sign for vertical velocity should occur with substantial delay compared to the reduction of buoyancy, and*** B_{core} ***should be a subset of*** W_{core} ***(i.e.*** $B_{core} \subseteq W_{core}$***) during the growing and mature stages of a cloud's lifetime.”***

Nevertheless, we note that our goal in the paper is not to develop a new parameterization for vertical velocity or gain new insights on the different components of the vertical velocity equation, but rather to get a general understanding of the processes affecting each core type.

MC3.2) 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.

MA3.2) The use of mean liquid water path (LWP) as a measure of cloud mass is an inherent property of the CvM phase space. The center of gravity (COG) which is taken as the vertical coordinate, can be easily linked to the LWP by using the theoretical case of adiabatic cloud column. So there is a good reference case. In addition we note that the mean LWP is taken by dividing the total mass by the mean cross-sectional area, so that even if clouds are slanted the mean LWP will reflect a “slanted” column. Using here the CvM phase space, as was done for cumulus cloud fields in previous works (Heiblum et al., 2016a, 2016b) enables examination of all clouds, at all stages of lifetime for the entire simulation. So we chose to use the CvM phase space due to its suitability to our purposes while we explain its advantages and limitations in the text:

“***In this space, the Center-of-Gravity (COG) height and mass of each cloud in the field at each output time step (taken here to be 1 min) are collected and projected in the CvM phase space. This enables a compact view of all clouds in the simulation during all stages of their lifetimes, with the main disadvantage being the loss of grid-size resolution information on in-cloud dynamical processes***”.

MC3.3) 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.

MA3.3) We agree with the reviewer that the insights into in-cloud core dynamics is limited. However, as explained in depth above, this is not part of the objectives of this work. We choose to perform a general comparison between the three core types for large statistics of clouds rather than increase the understanding of core dynamics.

MC3.4) 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.

MA3.4) The issue of which threshold to choose for the different core types occupied us a great deal. Indeed, choosing a >0 threshold includes passive regions with marginal updrafts increases the variance of the results for the small dissipating clouds. In Fig. RA1 we show the sensitivity to the different threshold for the single cloud case. Increasing the Wcore threshold significantly affects its extent. We find that up to w>0.15, the Wcore remains the largest, but for higher thresholds RHcore tends to be the largest.

Nevertheless, we think the >0 threshold is the only one which is purely physical. All other thresholds are case dependent and can change considerably from case to case. For a study aiming to analyze specific cloud dynamical features it might make sense to apply a strict threshold and limit the variance. But for a comparison we find that the current threshold is the most general. This point is now discussed in the revised text: "***We note that setting the core thresholds to positive values (>0) may increase the amount of non-convective pixels which are classified as part of a physical core, especially for the W_{core}. Indeed, taking higher thresholds for the updrafts decreases the W_{core} extent and reduces the variance. Nevertheless, any threshold taken is subjective in nature, while the positive vertical velocity definition is process based and objective.***"

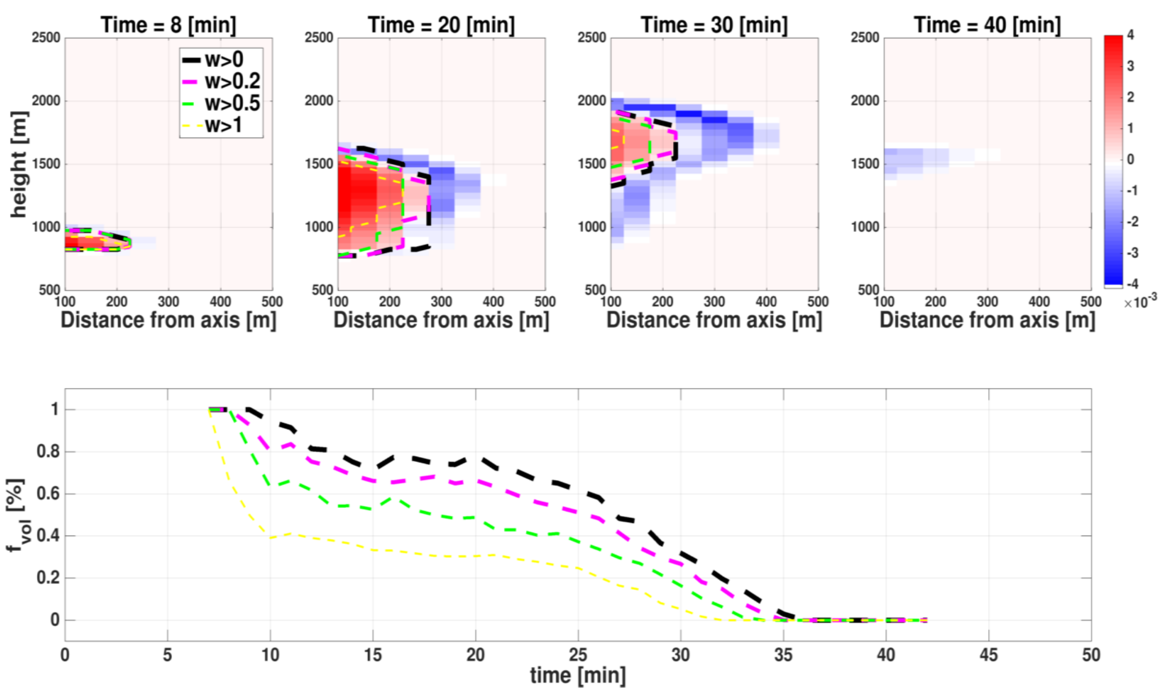

Fig. RA1. Top: Four vertical cross-sections (at t=8, 20, 30, 40 minutes) during the single cloud simulation with aerosol concentration of 500 CCN. Y-axis represents height [m] and

X-axis represents the distance from the axis [m]. The black, magenta, green and yellow dashed lines represent different vertical velocity core thresholds (see legend for values). The background represents the condensation (red) and evaporation rate (blue) [g kg^{-1} s^{-1}]. Bottom: Temporal evolution of vertical velocity core volume fractions (using different thresholds) from the total cloud volume (f_{vol}).

MC3.5) 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 organization of convection.

MA3.5) Thank you for this comment. After reexamination of the Amazon simulations we found that indeed cold pools play a dominant role in the organization of the cloud field. Therefore we decided to change the manuscript and show a newer and more documented case study (CASS - http://portal.nersc.gov/project/capt/CASS/).
The revised section about Cloud field model (section 2.2): ***"To check the robustness of the cloud field results, two additional case studies are simulated: (1) The same Hawaiian profile used to initiate the single cloud model, and (2) a continental shallow cumulus convection cases study (named CASS), based on long term observations taken at the ARM Southern Great Plains (SGP) site (Zhang et al., 2017)."***
This continental case study produced similar results to the oceanic case studies in the paper as can be seen in the revised fig. 7 below.

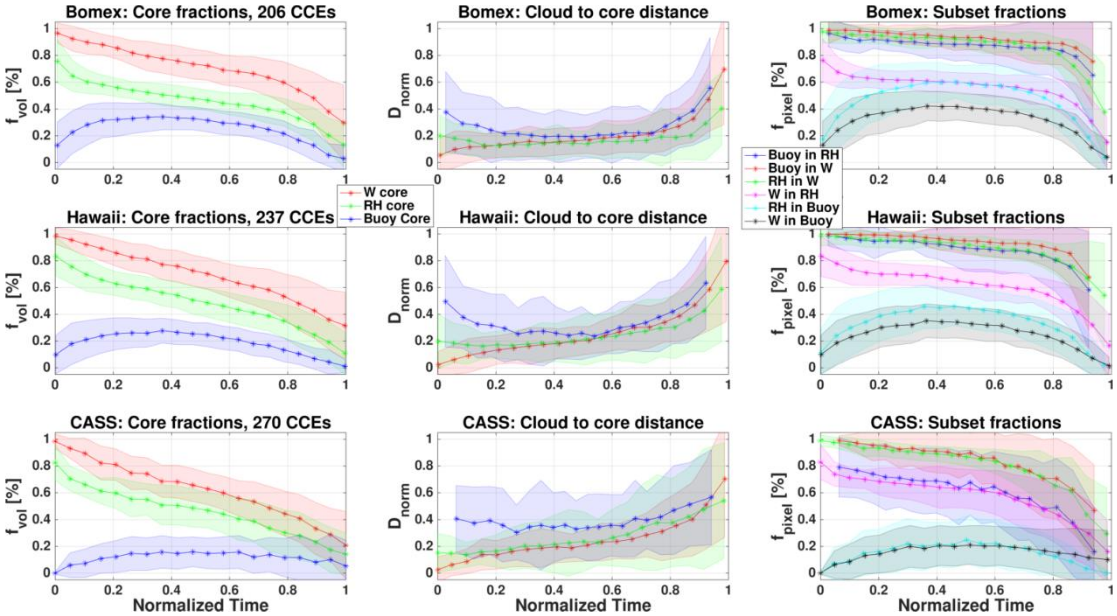

Figure 7 (from the revised manuscript). Normalized time series of CCE averaged core fractions for the BOMEX (upper row), Hawaii (middle row), and CASS (bottom row) simulations. Both core volume fractions (f_{vol}, left column), normalized distances between cloud and core centroid locations (D_{norm}, middle column), and pixel fractions of one core within another (f_{pixel}, right column) are considered. Line colors indicated different core types (see legends), while corresponding shaded color regions indicate the standard deviation. Normalized time enables to average together CCEs with different lifetimes, from formation to dissipation. The number of CCEs averaged together for each simulation is included in the left column panel titles.

Specific comments:

SC1) Equation 2 seems to be based on a parcel that is not mixing with its environment (see my main concern 1 above).

SA1) Equation 2 is added here to provide a non-expert with a basic understanding of how supersaturation and vertical velocity are thermodynamically linked. For the sake of accuracy, we have clarified that equation 2 (eq. 3 in new manuscript) refers to the adiabatic case which neglects mixing: "***Neglecting mixing with the environment, S and w can be linked as follows:***

$$\frac{dS}{dt} = Q_1 w - Q_2 \frac{dq_l}{dt} \qquad (3),$$

where Q_1,Q_2 are thermodynamic factors (Rogers and Yau, 1989)."

SC2) 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).

SA2) Thank you for this comment. All of the figures were redone so that the texts are larger, features are clearer, and cases where lines overlap can be distinguished. Figure 2 is added here for example:

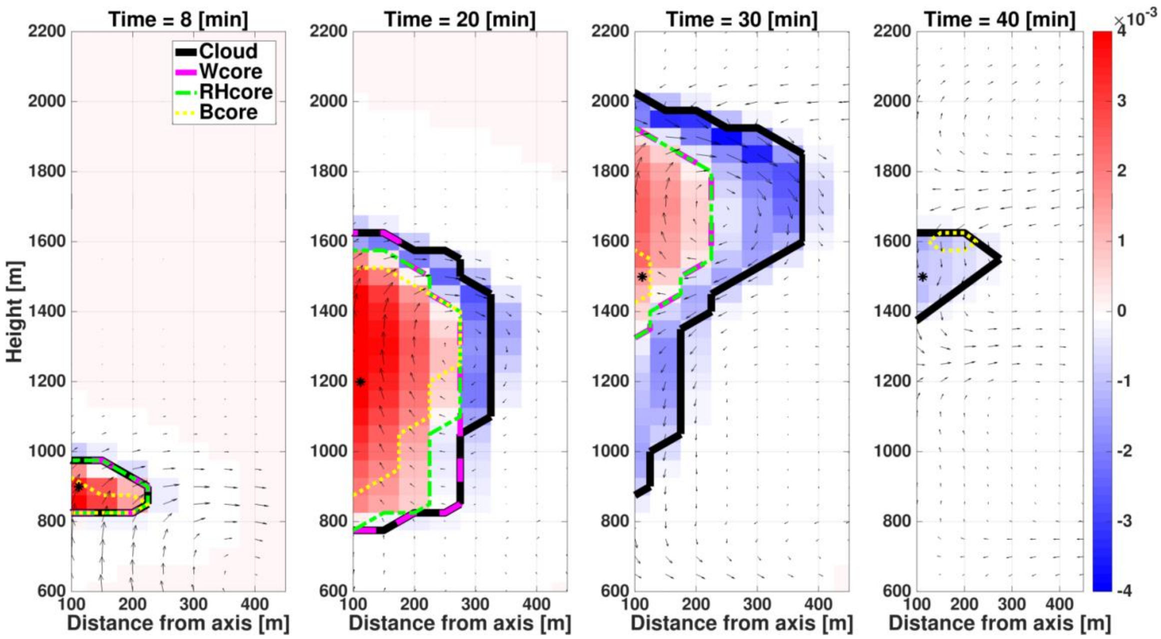

Figure 2 (from manuscript). Four vertical cross-sections (at t=8, 20, 30, 40 minutes) during the single cloud simulation. Y-axis represents height [m] and X-axis represents the distance from the axis [m]. The black, magenta, green and yellow lines represent the cloud, W_{core}, RH_{core} and B_{core}, respectively. The black arrows represent the wind, the background represents the condensation (red) and evaporation rate (blue) [g kg^{-1} s^{-1}], and the black asterisks indicate the vertical location of the cloud centroid. Note that in some cases the lines indicating core boundaries overlap (mainly seen for RH and W cores).

SC3) Equations should avoid the use of acronyms, such as LWC (in any case, is this specific humidity or mixing ratio?)

SA3) Thank you for this comment, we have replaced LWC with q_l – liquid water mixing ratio in eq. 3 (see SA1 above), and eq. 1, as follows:

$$B = g \cdot \left(\frac{\theta'}{\theta_o} + 0.61 q_v' - q_l\right) \quad (1).$$

SC4) 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.

SA4) Thank you for noticing this. We have added to the revised text exact definition of cloud growth in the introduction: "…***partitioned to two main regions: i) a core region, where mainly cloud growth processes occur (i.e. condensation – accumulation of cloud mass), and…***", the single cloud results: "***During cloud growth (i.e. (increase in mass and size)…***", and cloud field results: "…***fractions decrease with cloud growth (increase in mass and COG height) while…***".

In addition, we point out in the text the importance of bin-microphysics that enables cases of sub-saturated cloudy pixels: "***It should be noted that the bin-microphysical schemes used here calculate saturation explicitly, by solving the diffusion growth equation, enabling super- and sub- saturation values in cloudy pixels. This is in contrary to many other works that used bulk-microphysical schemes which rely on saturation adjustment to 100% within the cloud (Khain et al., 2015). This difference may produce significant differences on the evolution of clouds and their cores*** ".

Finally, as suggested by the reviewer the buoyancy is taken with respect to the surrounding environment. Citing from the text: "B_{core}***: buoyancy (see definition in Eq. (1)) above zero.***

The buoyancy is determined in each time step by comparing each cloudy pixel with the mean thermodynamic conditions for all non-cloudy pixels per vertical height“.

SC5) Parentheses should only be used around the year when an author is cited and the author name is part of the sentence.
SA5) Thank you, this issue has been addressed.

SC6) 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.

SA6) This reference is no longer relevant and has been removed from the text.

<u>Reply to reviewer #2 –RC2</u>

General Reviewer Comment

The work herein seeks to examine and compare three methods of defining convective cores through analysis of buoyancy (B), relative humidity (RH), and vertical velocity (W). The authors do a thorough job of comparing and contrasting the evolution of the various core definitions and highlight the overlap or lack thereof among the 3 defining core characteristics. They have performed their analysis via multiple methods including a theoretical model, single column type model, and a couple of models at the LES scale with bin microphysics and without saturation adjustment assumptions which can be limiting. The results appear quite robust among all methods of representing convective clouds and their cores and among various thermodynamic environments represented by different initial soundings. The manuscript is well-written, clear and concise, but a few questions and concerns, given below, should be addressed.

General Answer: We thank the reviewer for the beneficial comments and we were happy to read that the reviewer found our results robust and the paper well written and clear. The manuscript was revised according to all the comments.

Main Comments:

MC1) The motivation of the paper seems to lack its proper placement with respect to previous published work regarding convective cores and entrainment. While the focus of this work is specific to examining the relative differences between core definitions and their evolution over time, the work should be more appropriately placed in context and should emphasize what is novel in this work.

MA1) Thank you for this comment. In the revised manuscript clear emphasis is put on the novel parts of this work: the differences between the three core types, their evolutions in time, and comparison to previous understanding of core size and location within a cloud.

In addition, the introduction was changed significantly to include a broader review of works and ideas from the past that are relevant to this work.

We have added a few sentences to the introduction that clarify the objectives of the work: "***Specifically, we aim to answer questions such as:***

- ***Which core type is largest? Which is smallest?***
- ***How do the cores change during the lifetime of a cloud?***
- ***Can different core types be used interchangeably without much effect on analysis results?***
- ***Are the cores centered at the cloud' geometrical center, as expected from the core-shell model?"***

MC2) Some aspects of this work regarding entrainment, dilution, and their impacts on buoyancy are not new. However, the framework of comparing cores, core subsets, and their evolution in multiple model frameworks is perhaps more unique. It may help to better frame the paper in such a light.

MA2) Thank you for this comment. The abstract, introduction, and summary in the revised manuscript now put more emphasis on the novelties of the work while referring better to previous works when relevant. Although previous works have dealt with positive vertical velocity and buoyancy in clouds and the effects of entrainment on them, we do not know of a work which tries to perform a comprehensive comparison between the different cores and tracks these cores throughout their lifetime. The new abstract now focuses on these aspects of the work:

"The properties of a warm convective cloud are determined by the competition between the growth and dissipation processes occurring within it. One way to observe and follow this competition is by partitioning the cloud to core and margin regions. Here we look at three core definitions: positive vertical velocity (W_{core}), supersaturation (RH_{core}), and positive buoyancy (B_{core}), and follow their evolution throughout the lifetime of warm convective clouds.

Using single cloud and cloud field simulations with bin-microphysics schemes, we show that the different core types tend to be subsets of one another in the following order: $B_{core} \subseteq RH_{core} \subseteq W_{core}$. ***This property is seen for several different thermodynamic profile initializations, and is generally maintained during the growing and mature stages of a cloud's lifetime. This finding is in line with previous works and theoretical predictions showing that cumulus clouds may be dominated by negative buoyancy at all stages for adiabatic and non-adiabatic clouds.***

During its mature growth stage, the cloud and its cores are centered at a similar location. Additionally, at this stage all core types are of similar size and can be used interchangeably. During cloud dissipation the cores show less overlap, typically reduce in size, and migrate from the cloud centroid. In some cases, buoyancy cores can reemerge and often reside at the cloud periphery. Thus, the core-shell model of a positively buoyant center surrounded by negatively buoyant shell only applies to a fraction of the cloud lifetime."

Specific Comments:

SC1) Line 40: Here you mention that negatively buoyant cloud may exist due to W>0 and S>1. You might specifically mention the other components of the W equation that keep W>0 and S>1 and their relative contributions during stages of B>0 and B<0. Perhaps this could also be addressed in the main text in greater detail. Once B<0, the other components of the W equation will begin to weaken since the "fuel" is missing. What tends to weaken faster, and what implications does this have for the W core?

SA1) This part was removed from the revised abstract. The existence of negatively buoyant clouds (as referred to in the previous version) can be attributed to inertia or "leftover fuel from sub-cloudy layer buoyancy. The other components of the vertical velocity equation (de Roode et al., 2012; Romps and Charn, 2015) can only decelerate the buoyant updrafts and not actually create a cloud. An exception is large scale advection and quasi-geostrophic ascent, which are irrelevant to the scope of this paper.

Many previous works have dealt with the relative importance and feedbacks of the W equation components (de Roode et al., 2012; Morrison, 2016a, 2016b; Romps and Charn, 2015) and we think it is a subject that requires a study on its own. However, we revised the paper to better explain the W equation in the introduction, as follows:

"***Neglecting cases of air flow near obstacles or air mass fronts, buoyancy is the main source for vertical momentum in the cloud. In its simplest form, the vertical velocity (w) in the cloud can be approximated by the convective available potential energy (CAPE) of the vertical column up to that height (Rennó and Ingersoll, 1996; Williams and Stanfill, 2002; Yano et al., 2005):***

$$0.5w^2(h) = \int_{h_0}^{h} B(z)\,dz = CAPE(h) \qquad (2).$$

Here we define CAPE to be the vertical integral of buoyancy from the lowest level of positive buoyancy (h_0, initiation of vertical velocity) to an arbitrary top height (h). Usually, the CAPE serves as a theoretical upper limit, and the vertical velocity is smaller due to multiple effects (de Roode et al., 2012), most importantly the perturbation pressure gradient force (which oppose the air motion) and mixing with the environment (entrainment/detrainment) (de Roode et al., 2012; Morrison, 2016a; Peters, 2016). Recent studies have shown that entrainment effects on vertical velocity are of second order, and a rising thermal shows a balance between buoyancy and the perturbation pressure gradient (Hernandez-Deckers and Sherwood, 2016; Romps and Charn, 2015), the latter acting as a drag force on the updrafts."

SC2) Lines 177: The potential initial temperature perturbation of 1C is rather large for this type of shallow convection setup. Could such a large perturbation shock the initial field and generate a sizeable convective pulse and gravity waves that impacts the rather small domain size?

SA2) Thank you for this comment. There was a mistake in the text. The SAM model was initialized with random ±0.1°C (instead of ±0.1-1°C) perturbations throughout the domain. It is corrected in the revised text.

SC3) Line 182: The cloud pixel threshold here of 0.01g/kg seems rather small. What could be deemed a visible cloud would likely be closer to 0.1g/kg. Including values closer to 0.01g/kg would likely include very diffuse clouds at cloud edges that are generated in models. Choosing a different threshold could seemingly have a great impact on the definition of the cloud volume. Have you examined the impact of this threshold choice? I am aware that many papers have used the 0.01 g/kg threshold; but the choice here seems more critical given the examination of cloud volume and such.

SA3) The question of cloud pixel liquid water content (LWC) threshold is something we have examined as part of this work. We started by taking an even lower threshold of 0.005 g/kg (Cohen and Craig, 2006) but eventually raised the threshold to 0.01 g/kg based on other works (Jiang et al., 2009; Xue and Feingold, 2006). The impact of threshold choice is shown in Fig. RB1 below. The 0.01 and 0.005 g/kg thresholds yield similar results with regards to cloud volume, while higher thresholds (0.05 and 0.1 g/kg) reduce cloud volume significantly. By taking areas of condensation and evaporation as indicators of cloudy regions, it can be seen that the higher values thresholds “miss” pixels with high evaporation rate (vapor diffusion), in both growing and dissipating stages of cloud lifetime. Hence, we find that the 0.01 g/kg threshold best reflects a cloudy volume, without the risk of including insignificant cloud debris as can be seen in some cases for the lower 0.005 g/kg threshold.

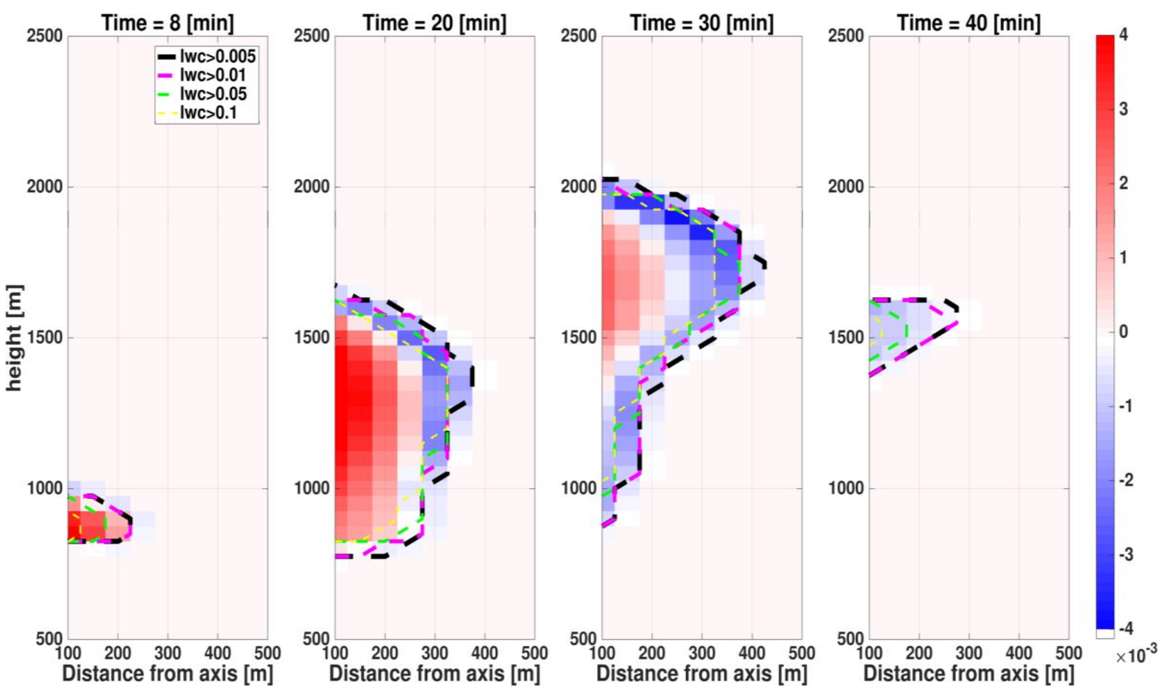

Fig. RB1. Four vertical cross-sections (at t=8, 20, 30, 40 minutes) during the single cloud simulation with aerosol concentration of 500 CCN. Y-axis represents height [m] and X-axis represents the distance from the axis [m]. The black, magenta, green and yellow dashed lines represent different LWC thresholds for a cloudy pixel (see legend for values). The background represents the condensation (red) and evaporation rate (blue) [g kg^{-1} s^{-1}].

SC4) Line 184-186: Here you state that buoyancy is determined relative to the mean thermodynamic conditions for non-cloud pixels. How is buoyancy computed and applied in the dynamic core of the model? Are these the same or different, and what are the implications if these are different?

SA4) The buoyancy in the dynamical core of the axisymmetric model is calculated in a similar way to the buoyancy calculations as described in the paper, with the sole difference being the dynamical core buoyancy is calculated with respect to the mean initial thermodynamic conditions while we take the mean instantaneous non-cloudy thermodynamic conditions. Since the domain is sufficiently large and unaffected during the simulation, the differences between the two buoyancy calculations is negligible, as can be seen in Fig. RB2.

In the cloud field model (SAM) the dynamical core buoyancy is calculated with respect to the mean horizontal thermodynamic conditions (cloudy and non-cloudy), which gives almost identical results to our calculation in this work (i.e. with respect to only non-cloudy). Since each of the models' dynamical core calculates buoyancy a bit differently, we chose one calculation that applies to both.

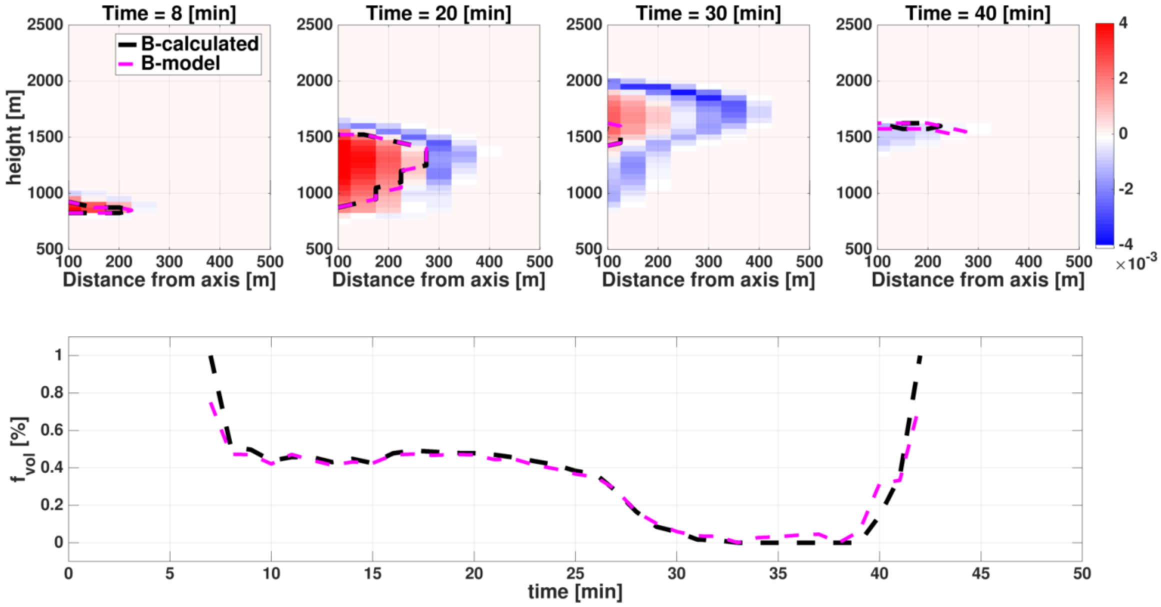

Fig. RB2. Comparison of dynamical core buoyancy (magenta lines) with calculated buoyancy (black lines). The top panels are similar to Fig. RB1, but with lines representing core extent. The bottom panel shows the temporal evolution of buoyancy core volume fraction from the total cloud volume (f_{vol}).

SC5) Line 257: Here you state that the cloud top downdraft promotes adiabatic heating that leads to the decay phase positive buoyancy. Is this definitive or supposition here? Is this seen in other clouds? Is this adiabatic heating greater than any local evaporative cooling?

SA5) A significant part of Part II of this work was devoted to the explanation of why pockets of positive buoyancy appear is non-convective regions of dissipating clouds. We show that if the evaporative cooling is weak enough (or no evaporation occurs), the adiabatic heating is sufficient to create positive buoyancy in weak downdrafts. The reader is referred to Part II within the text for the single cloud:

"***Further analysis (see Part II) shows that the entire dissipating cloud is colder and more humid than the environment but downdrafts from the cloud top (see arrows in Fig. 2) promote adiabatic heating, and by that increase the buoyancy in dissipating cloudy pixels, sometimes reaching positive values. These buoyant pockets will be discussed further in Part II.*** ".

and cloud field:

"***The prevalence of cloud edge B_{core} pixels during dissipation can be explained by adiabatic heating due to weak downdrafts (see Sect. 4.2, Part II) which are expected at the cloud periphery***.".

SC6) Line 268-270: Are the changes in cloud volume fraction susceptible to the choice of cloud mass concentration used to define a cloud grid cell (0.01 g/kg)? How would choosing a different threshold impact your analysis?

SA6) We have tested this question as part of this work and found that the main conclusions would not have changed regardless of the LWC threshold chosen. This fact is demonstrated in Fig. RB3 for the single cloud case, where it can be seen that the subset properties of the three cores and their relative sizes are similar. The main difference that arises is the positive buoyancy core that appears during dissipation only for lower cloud LWC thresholds. However, we find this effect to be substantial in the cloud field simulation and for other aerosol concentrations, and thus should not be considered an outlier only seen for vert low LWC pixels. An additional figure for a low aerosol concentration of 25 CCN is also shown below (Fig. RB4), where an increase in buoyancy core during dissipation is seen for all thresholds.

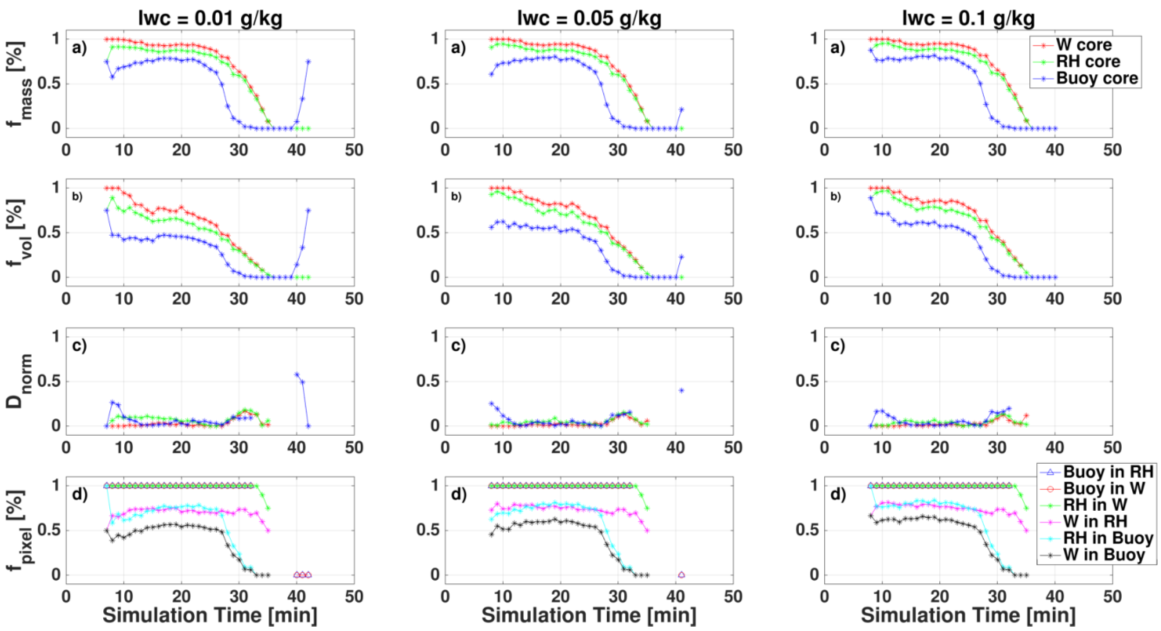

Fig. RB3. Same as figure 3 in the manuscript, but for three different cloudy pixel LWC thresholds [g/kg]: 0.01 (left column), 0.05 (middle column), 0.1 (right column). Aerosol concentration is 500 CCN.

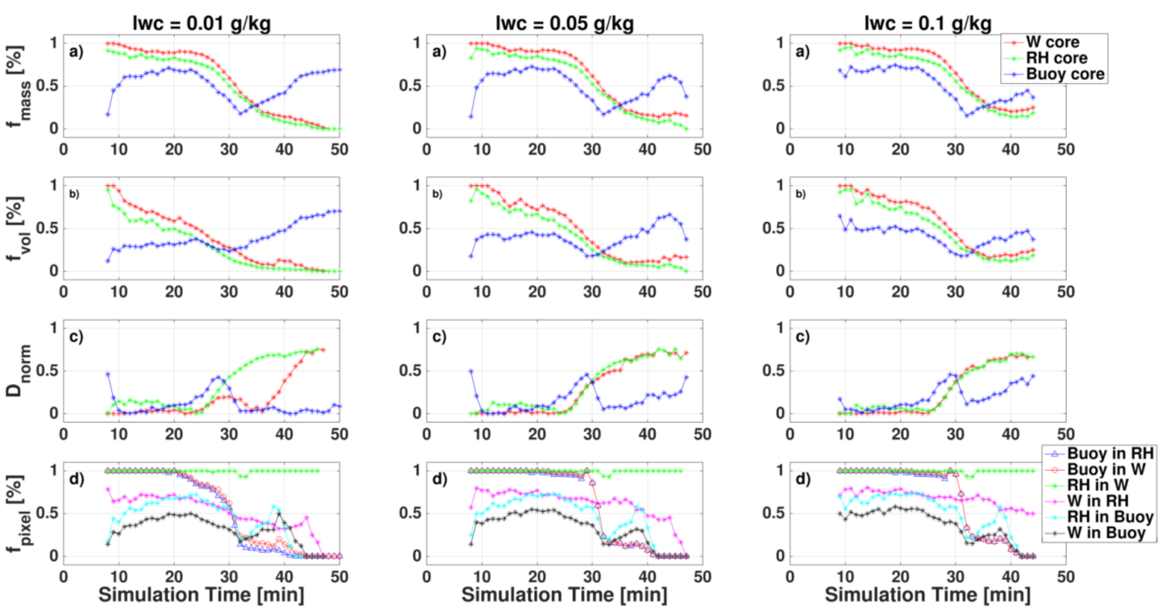

Fig. RB4 Same as figure RB3, but for an aerosol concentration of 25 CCN.

SC7) Line 722: How valid is this non-changing temperature assumption to your analysis? This seems like a rather unrealistic and constricting assumption. The local dT could be large which could greatly impact dB and mixing.

SA7) The non-changing temperature assumption only applies to the reference environmental temperature. This assumption is based on the fact that the environment is sufficiently large and its mean temperature is not affected by local evaporation. We find this assumption to be standard practice for almost all models calculating buoyancy (Khairoutdinov and Randall, 2003; Seigel, 2014), which take the horizontal mean temperature as reference (which changes very slowly during the course of a simulation, if at all), rather than a local temperature in the vicinity of a cloud.

SC8) Line 267: “expect” should be “except”.
SA8) Thank you, the change was carried out.

SC9) Line 371: “overweighs” should be “outweighs”.
SA9) Thank you, the change was carried out.

SC10) Line 591: “cloud’s” should be “clouds”.
SA10) Thank you, the typo was fixed

SC11) Line 619: “from precipitation” should be “by precipitation”.
SA11) Thank you, the change was carried out.

SC12) Line 634: This should read: “In cases where the: : :.”
SA12) Thank you, we added “where” to the sentence.

SC13) Figures: My main comment about the figures is that most of the them need to be larger, especially the fonts, so that they are easily readable. The time series plots need to be much large in order to see overlap where it exists.
SC13) Thank you for this comment. All of the figures were redone so that the texts are larger and cases where lines overlap can be distinguished.

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