Author Response:

We thank the referees for their time, consideration and helpful suggestions and comments. Below we respond to the comments point-by-point. The referee comments are in *italic*, responses in normal font.

Anonymous Referee #1:

- *My main concern is the limited number of clouds that are taken into account, especially so since this number is so small because of strong restrictions on the identification algorithm.*

The number of clouds analyzed is limited because of the time required for manual tracking, not because there were only 12 clouds that fit the criteria.

- *...is it not possible to use more than only 6 hours of especially RICO?*

The authors only had access to 6 hours of both the RICO and BOMEX simulations, of which the first 2 hours were spinup.

- *show that the typical values (e.g. cloud top height, incloud total,liquid water, temperature, velocity profiles) are similar to what one would get from the conditional sampled field. In this case, I would say that especially the total water and cloud top height are important to show.*

We refer the reviewer to Figure 2 of Xue and Feingold 2006 (reproduced in Fig. 1), showing time series of different quantities for the BOMEX simulations, in particular panels (c) and (d). Panel (c) shows liquid water path and panel (d) shows conditionally sampled mean cloud top height. Below in Figures 2 and 3 are time series of cloud top height and liquid water path for the sampled clouds. The lifetime average values for each cloud are indicated on the ordinate. The mean lifetime average cloud top height of the clean BOMEX clouds sampled is $\overline{z_{CT}} = 1411 \pm 165$ m (the error is one standard deviation), slightly higher but comparable to the domain average cloud top height from the last few hours of Fig. 2d in XF06, the time frame from which the sampled clouds were chosen. The mean lifetime average liquid water path for the clean BOMEX clouds, $\overline{LWP} = 152 \pm 70$ g m^{-2}, is higher than XF06 Fig. 2c, but this is most likely because we intentionally avoided sampling small forced cumuli. The polluted BOMEX clouds have lower mean lifetime average LWP, $\overline{LWP} = 102 \pm 20$ g m^{-2}, which is comparable to the domain average LWP (dashed line, XF06 Fig. 2c). For both cloud top height and LWP, the simulated RICO clouds are similar to those from the BOMEX simulations.

- *For starters, why do the authors normalize rt, but do not do so for the heat content?*

Cloud-average total water r_t shows greater variability over cloud lifetime than θ (10-20% vs ∼1%, respectively), which made the former a more promising clock. Normalization was done to further improve the clock. The poor dynamic range and large variability of the θ time series led to it not being a very promising clock because measurement noise/uncertainty can easily confound the clock. Normalization would not have greatly improved the clock so we present the results unnormalized. We calculated moist static energy (a conserved thermodynamic quantity like θ_e and θ_l) and found that variability over cloud lifetime in such a conserved thermodynamic variable is comparable to the variability of potential temperature (1-3%).

- *Any serious cloud clock needs to be an improvement over the cloud top height metric.*

We refer the reviewer to Fig. 2 below. Cloud top height is multivalued and varies widely betwen clouds in the mature and dissipation stages. Furthermore, the age uncertainty using cloud top height is much greater than for the r_t^* clock. For the clouds sampled, the r_t^* clock is more effective than the cloud top height metric. We added this figure to the manuscript (in Section 3.3) as well as some discussion of the figure.

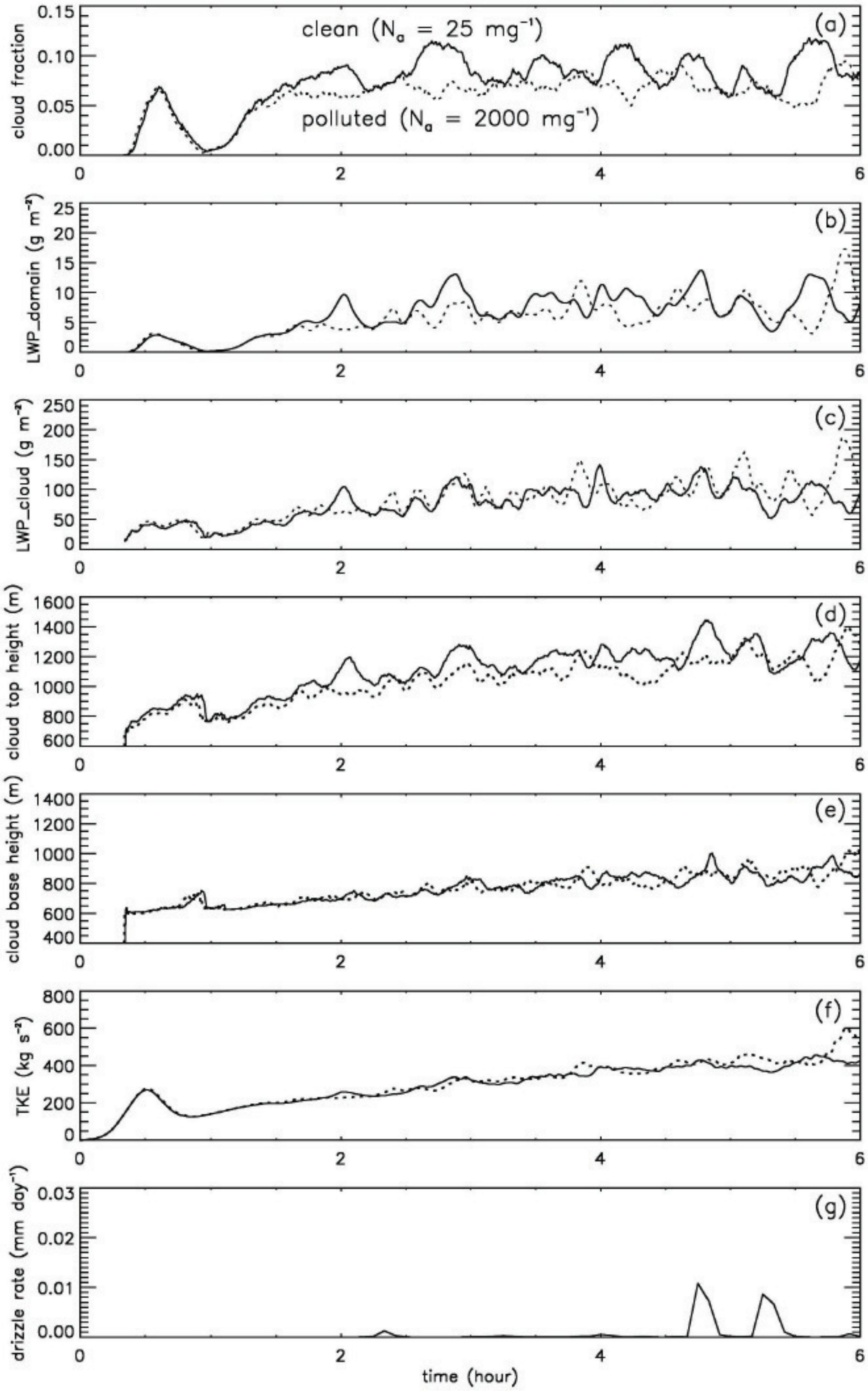

FIG. 2. Time series for the clean (N_a = 25 mg^{-1}) and polluted (N_a = 2000 mg^{-1}) cases for the base simulations: (a) cloud fraction, (b) domain-averaged LWP, (c) cloud-averaged LWP (averaged over cloudy columns), (d) cloud-top height, (e) cloud-base height, (f) vertically integrated TKE, and (g) domain-averaged surface precipitation rate. Surface precipitation rate in the polluted case is negligible so only the precipitation for the clean case is shown. Line types are solid for the clean case and dashed for the highly polluted case, are used consistently in all figures.

Figure 1: A reproduction of Figure 2 from Xue and Feingold 2006.

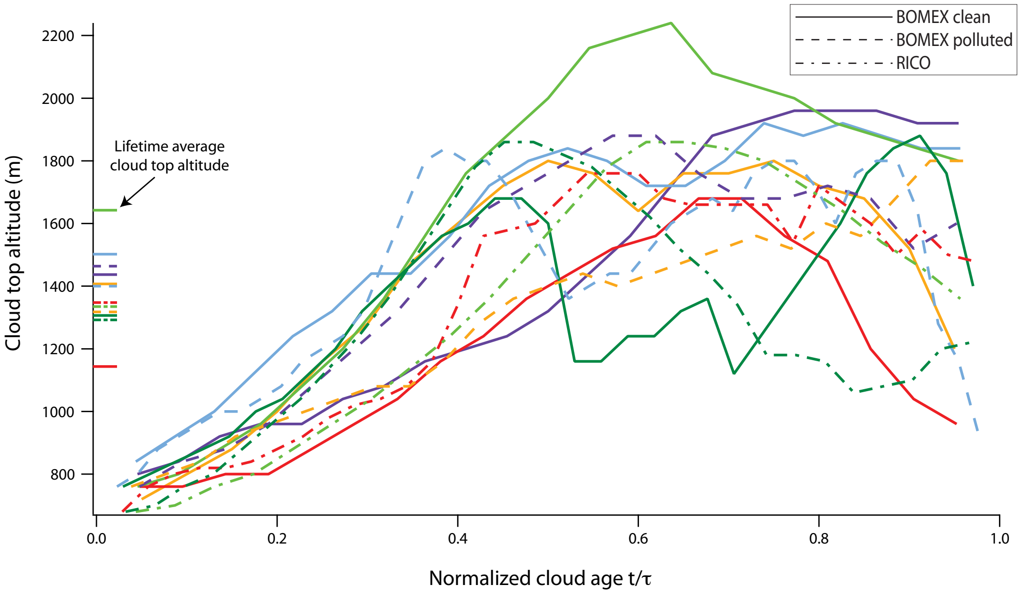

Figure 2: Time series of cloud top height. The colored and patterned ticks on the ordinate are lifetime average cloud top altitude for each cloud.

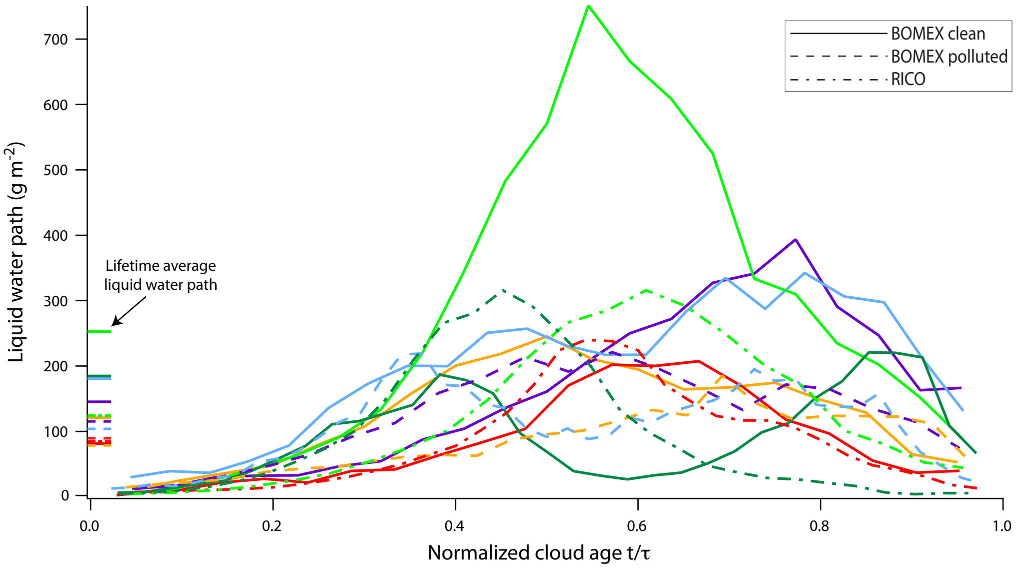

Figure 3: Time series of LWP. The colored and patterned ticks on the ordinate are lifetime average LWP for each cloud.

- *p23462, l24: The entire life time description of the non precipitating clouds is based in dynamic/macrophysics. The microphysics here is not really relevant.*

Agreed. The microphysical description of life cycle has been removed.

- *p23463, l 18: "certainly difficult": I would say: "fundamentally impossible"*

The text has been changed from “certainly difficult” to “effectively impossible.”

- *p23464, l 6: Radar is likely the most effective *observational* tool for studying the life cycle.*

The text has been updated with the suggested change.

- *p23464, l 16: Aircraft*s**

The plural of aircraft is aircraft.

- *p23466, l 15-22: These restrictions are really strict. We can often observe a nascent cloud popping up in several small cells that quickly merge. Likewise, in the late phase of the clouds chunks typically break off and mix away. Does this mean that if any of those processes happen once, the entire cloud is discarded? I would expect that the lack of clouds between your two limits is because of the all-or-nothing condensation scheme and the relatively coarse resolution.*

Several clouds were used that popped up in small cells that quickly merged, as the referee suggests. The constraints on cloud selection (i.e., lifetime *starts* when $n_{cloud} > 5$ cells) allowed relaxation of the “no splitting or merging” requirement with regards to cloud nascence. In the case of dissipation, we ignored vertical splitting when the split happened after the cloud base had lifted from the domain average cloud base. That is to say, we still looked at the cloud as a single entity because the chunks that mixed away were short-lived. The text has been updated to clarify these relaxations of the selection criteria:

Re: nascence:

“Cloud lifetime is defined to be the elapsed time during which the cloud comprises at least 5 contiguous cloudy cells (i.e. clouds may initially form as a small number of nonadjacent cells).”

Re: dissipation:

“Clouds that split horizontally...are also excluded from the study.”

- *p23467, l 11: I always tend to think that there are no holes in these type of clouds. Not on the scale of your LES resolution, anyway.*

Further examination confirms the referee’s conjecture. The sentence on holes was removed.

- *p23468, l 18: What do you mean with variability in the horizontal dimension? Isn’t dissipation always going from outside inward?*

We mean that there is heterogeneity in the horizontal dimension along cloud perimeter, not that strong dissipation is occurring at the cloud core. The sentence on horizontal variability was changed to: “At any altitude, dissipation along the cloud perimeter exhibits similar variability.”

- *p23468, l 25: Those old clouds are not often targeted, but they are actually often hit, either as collateral damage or because of the considerable time it takes for a plane to aim for and fly through a cloud. If anything, the nascent clouds are the ones that are underrepresented.*

We removed the statement on observational bias.

- *p23469, l 4: Why do you leave the neutrally bouyant points out of your figure 3? To me, these may be the most interesting ones, just to see whether the scatter crosses the origin, or sits just below or above. I would expect to see a signature of the overshoot to be visible, so dVol/dt > 0 while B<0. The strongest (negatively) buoyant points are also the most trivial.*

The neutrally buoyant points are plotted in Figure 3 and Figure 4 but are not included in the analysis described in the text. The text was updated to clarify that neutrally buoyant points are excluded from the analysis but not the figure: "Figure 4 illustrates the relationship. Analyzing only points with $|\Delta\theta_v| \geq 0.05$, 93 % of the points..." Points with $|\Delta\theta_v| < 0.05$ account for <15% of the data set.

- *p23469, l16/table 2: Why do you only normalize for rt, and not for theta? And why do you propose theta (which is roughly equal inside or outside the cloud) and not theta_ e or theta_ l?*

See response above to *For starters...*

- *p23469, l 12: Don't you mean fig 7 instead of fig 9?*

Yes. The text has been appropriately updated.

- *p23473: It is a nice result that a dynamically driven clock works best on what looks like the dynamical entity of the cloud. However, if those pulses are moving from bottom to top, shouldn't we be able to see the clock being slightly ahead at the bottom and slightly late at the top? I don't think I can see this in figure 9, although the print in this figure is too small to read on my hardcopy.*

The reference point $z^* = 1$ is variable over cloud lifetime, such that when the cloud top dissipates, the real altitude can precipitously drop over a few (2-4) time steps. This is the case for the cloud in Figure 9. As the second pulse is occurring, the cloud top is rapidly dissipating. Hence the real altitude range covered by $0 < z^* < 1$ is compressed, preventing a "signal" from propagating upward through the cloud due to both replenishment from the new pulse and the fact that "near cloud top" r_t^* is artificially increasing due to no cloud existing at higher real altitude.

- *p 23471, l 27: Does that mean that if you rescale with (rt_ ml - rt_ env(z)) that all these lines would go from 1 to saturation? Or, even more interesting, Go from 1 to 0 if you let the life cycle of the cloud end not just at the moment when the cloud is evaporated, but when the cloud is dissipated (presumably much later, especially in terms of rt).*

First, scaling by $r_{t,ml} - r_{t,env}(z)$ forces the clock to an arbitrarily high value at $t/\tau = 0$ because $r_t^*(0) \approx \frac{r_{t,ml}}{r_{t,ml} - r_{t,ml}} = \frac{r_{t,ml}}{0}$. Secondly, the r_t ending point of such a clock is highly dependent upon the altitude of the last remnant bit of a cloud, which is variable even for the small population examined here. Lastly, $r_{t,ml} - r_{t,env}(z)$ gives information about how close a cloudy parcel is to being evaporated as opposed to how close a parcel is to its initial state (essentially a pulse age). Since we are framing cloud age as independent pulse age, total water relative to saturation does not make the clock more useful or easier to interpret.

- *p 23475 l 20: Dawe and Austin had a very different definition of isolated clouds, since they did include many break ups and collisions. Given the amount of clouds they were able to detect in a similar simulation size, I doubt that 75+% of the clouds are born and die in isolation according to your definition. This is especially true for later stages of the RICO simulations, with slightly higher cloud cover and more interactions between clouds than in BOMEX.*

We agree that it is unlikely our definition would result in 75% of all simulated clouds forming and decaying in isolation. However, the purpose of referencing Dawe and Austin's result is to reinforce the point that the clouds sampled are probably more spatially isolated than the typical cloud.

- *I don't think I can see this in figure 9, although the print in this figure is too small to read on my hardcopy.*

Figure 9 has been edited for legibility.

Anonymous Referee #2:

- *1. The title could be more informative of the content.*

While a longer title may be more informative, we believe the title accurately represents the content and we would like to keep it as is.

- *2. It would help to include a sequence of cross sections of liquid water, total water, buoyancy, and vertical velocity for one cloud to show its structure and evolution.*

See Figure 2, panels c-d from Xue and Feingold 2006 to get a sense of what happens to the BOMEX cloud field in the domain average. Dawe and Austin 2011 give an excellent overview of the evolution of cumulus in LES. Specifically, figures 4 and 5 show cross sections of cloud area, vertical velocity, total water, $\Delta\theta_l$, LWC and buoyancy. A reference to Dawe and Austin 2011 was added in section 2 on Methodology.

- *3. 66:12 Which models? explain.*

We updated the text to refer the reader to Table 1, which contains information on the models: "The models and relevant model parameters are detailed in Table 1..."

- *4. 66:18 What fraction of all the cells were cloudy?*

The fraction of cloudy cells in the sampled volume is arbitrary as the sample volume size varied over time - often it was artificially large during the dissipation phase to accelerate volume selection since the selection was performed manually. The fraction of cloudy cells in the sampled volume is therefore not a systematically constrained value.

The 2-d cloud fraction for the entire domain for BOMEX can be found in Figure 2a of Xue and Feingold 2006, and for RICO in Figure 1e of Jiang et al. 2009.

- *5. 66:19 How were contiguous cells defined? (e.g., adjoining faces only?)*

The manuscript has been updated: "Cloud lifetime is defined to be the elapsed time during which the cloud comprises at least 5 contiguous cloudy cells (i.e. clouds may initially form as a small number of nonadjacent cells). We define "contiguous" as sharing an adjoining face."

- *6. 66: 26 Define "virtual potential temperature" as used in your analysis.*

Virtual potential temperature is a standard meteorological quantity and, as such, we feel it does not require an explicit definition in this paper.

- *7. 66: 26-28 Presumably "turbulent kinetic energy" refers to sub-grid scale TKE. If so, it will depend on the grid size, even when computing cloud averages. Make this clear to the reader.*

TKE was estimated using resolved-scale vertical velocity variance. All references to TKE have been changed to "vertical velocity variance" to more accurately reflect the analyses.

- *8. 67:4-5 Positive buoyancy does NOT imply rising motion, nor does negative buoyancy imply subsiding motion. Instead, buoyancy contributes to the vertical acceleration.*

Agreed. We updated the text to clarify that we do not assume positive buoyancy necessarily implies upward motion: "Positive values of cloud-average $\Delta\theta_v$ indicate positive buoyancy while negative values indicate the opposite, which on average are associated with rising and subsiding motion, respectively."

- *9. 71:24 Is the "cloud top" instantaneous? or maximum over cloud lifetime?* and *10. 72:5 Does "mid-cloud" refer to mid-cloud at that instant? or relative to the maximum cloud top over the cloud lifetime?*

Cloud-relative altitude z^* uses the instantaneous cloud base and cloud top as bounds, so the absolute altitude z referred to by z^* may change every time step. The manuscript has been updated to clarify this point.

- *11. 72:19 Define ai*

The manuscript has been updated: "The single-pulse age model predicts an age $t/\tau \approx 0.5 \pm 0.1$, while the cloud exhibits $r_t^* \approx 0.90$ at 3 times ($a_1 \sim 0.3$, $a_2 \sim 0.5$, $a_3 \sim 0.6$, where a_i are the times at which hypothetical measurements were taken over cloud lifetime).

- *12. 73:21 Explain what "recorded data points" refer to.*

The term "recorded data points" was changed to "sampled cloud time steps" and the following references to "points" were changed to "time steps."

- *13. 74:9-12 Why not add a category that encompasses the multiple pulse cases? (This would require two variables, presumably.)*

In the manuscript, we propose that the cloud-average buoyancy perturbation $\Delta\theta_v$ is the best candidate for a second variable to diagnose multiple pulse clouds. See Section 3.4.2, the paragraph starting at pg 23472 l 16 for a discussion.

- *14. 75: 2-3 Change "Precipitation reduces the potential for evaporative cooling" to "Precipitation reduces the potential for evaporative cooling due to cloud droplet evaporation"*

Agreed. The text has been updated appropriately.

- *15. 75: 25 This statment needs to be revised: "there is disagreement in the treatment of entrainment and detrainment mixing among different cloud-resolving models (de Rooy et al., 2013)" because (1) the intended meaning of "treatment" is not clear: It could be "analysis" or "representation" and (2) the models considered by de Rooy et al. are not cloud-resolving models, but large-eddy simulation (LES) models. LES models should resolve the large entraining eddies and therefore be more realistic than cloud resolving models that entrain primarily via SGS fluxes.*

The word "treatment" has been changed to "representation." The term "cloud-resolving models" was used in error, as de Rooy et al. (2013) specifically discuss entrainment and detrainment representation in LES models. The text has been updated to make it clear that we are discussing LES.

- *Technical Corrections 1-2,4-5,7*

The above technical corrections have been made.

- *Technical Correction 3*

The term “cell” is a commonly enough used term in referring to simulated grid volume that we find it to be more useful and less awkward to use than using “grid volume” throughout, which we feel overcomes the disadvantage of any inaccuracy in usage.

- *Technical Correction 6*

The figure has been updated using b_1 and b_2 to refer to “single pulse ages” and a brief description has been added to the text: “Assuming the two pulses are independent, we can determine a single pulse age associated with each pulse. The times a_1 and a_3 can be expressed as single pulse ages b_1 and b_2, respectively. In Fig. 9b...”