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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On clocks and clouds" *by* M. K. Witte et al.

Anonymous Referee #1

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This paper uses cumulus clouds generated by LES to develop a simple metric for the age of a cloud. Simple, as in: Can be determined based on instantaneous and local observations that are typically available in an air plane or from remote sensing. I find the paper relevant in that it discusses a topic that I believe needs to be discussed in the literature. As the authors, I am not sure that this study will be the definitive study solving the problem once and for all; for that, the sample of clouds is too small, and it lacks sufficient understanding why the normalized humidity is the best metric. However, I very much appreciate the honesty about these issues from the authors, and I can therefore recommend it to be accepted for publication in ACP, after some questions have been addressed.

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. This may mean that the clouds are not typical for the entire ensemble (as the authors acknowledge). I understand that using a different algorithm (like, the Dawe and Austin one) is no option here, but is it not possible to use more than only 6 hours of especially RICO? The intercomparison studied 24 hours in total. One way to reassure the reader that this limited sample says something about the cloud field is to 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.

My second main point would be that I'm not sure that the paper really proves that r_t* is the best possible quantity and has much predictive skill. For starters, why do the authors normalize rt, but do not do so for the heat content? More importantly, the authors leave out the most naive metric of cloud age: Cloud top height. One glance at the figures of Zhao & Austin, H09 or Dawe and Austin tells us that cloud top height is a flawed metric in the later stages of the cloud life time (it stays more or less constant, or decreases again). Maximum cloud top height also tends to vary a lot from cloud to cloud, although this may be less so in your subset because of the sampling method. Any serious cloud clock needs to be an improvement over the cloud top height metric. Much of Fig 8 can possibly be explained by the idea that in-cloud water content must be somewhere between cloud base and cloud top values. The slope of the curves are in rough agreement with my back of the envelope guesses of the cloud top height evolution. It would be good if the authors can comment on this.

Other points: p23462, l24: The entire life time description of the non precipitating clouds is based in dynamic/macrophysics. The microphysics here is not really relevant. p23463, l 18: "certainly difficult": I would say: "fundamentally impossible" p23464, l 6: Radar is likely the most effective *observational* tool for studying the life cycle. l 16: Aircraft*s* p 23466, 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. p 23467, l 11: I always tend to think that there are no hoes in these type of clouds. Not on the scale of your LES resolution, anyway. p23468, l 18: What do you mean with variability in the horizontal dimension? Isn't dissipation always going from outside inward? 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. 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$\sim$<0. The strongest (negatively) buoyant points are also the most trivial. 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? l 12: Don't you mean fig 7 instead of fig 9? p 23473: 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. 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). p 23475 l 20: Dawwe 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.

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