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Comment on acp-2021-334

Anonymous Referee #1

Referee comment on "Revisiting adiabatic fraction estimations in cumulus clouds: high-resolution simulations with a passive tracer" by Eshkol Eytan et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-334-RC1, 2021

What is the adiabatic fraction in cumulus clouds: Eytan et al 2021 ACP

Summary:

In this paper, the authors assess various methods used for estimating adiabatic fraction (AF) in a non-precipitating cumulus cloud. A High-resolution LES model with bin microphysics was used for simulating the cloud field. The AF computed using different methods was compared against the AF calculated using a passive tracer.

I am not convinced about the key results and conclusions that are drawn from this study. Additional details must be provided to understand the results and to assess the significance of this work.

Major comments:

- In section 2.3, the LWCad calculation in Eq. 5, 7 and 8 assume that the parcel under consideration is adiabatic. So, I am wondering how these equations can be used for calculating the adiabatic LWC from a cloud field that is affected by entrainment/mixing. The temperature and water vapor fields from the simulation will be affected by entrainment/mixing. So, using these fields in Eqs. 5, 7 and 8 would violate the assumptions used in deriving these expressions. Thus, the LWCad computed using the technique mentioned in this study would not be correct.
- The AFs computed using the methods in section 2.3 are compared against the AF computed using the passive tracer. Is it a fair comparison to compare AFs calculated using two very different variables? The passive tracer is a conserved variable whereas

LWC is not. Both these variables to some extent can be used for determining the adiabatic core, but once mixing occurs, then a one-to-one comparison may not be fair. Can the authors comment on that? If the authors agree, then what is the significance of the observations and conclusions from the current study?

- In Figure 2(a), AFref>AF_scalar in the upper half of the cloud (the blue-colored region). The discussion related to this (lines 190-210) attributed it to the presence of toroidal vortices and enhanced updraft. The evidence provided to support this conclusion is not very concrete. The issues raised in the previous two points are relevant here. AFref>AF_scalar, this could also be due to a lower estimated LWC_ad. Since the calculations are based on the simulated cloud fields, the adiabatic LWC obtained using Eqs 5, 7 and 8 would be an underestimation compared to the actual LWCad as the cloud field is affected by entrainment. This is evident from the passive tracer field in Fig. 1(d). Thus, AFref> actual AF, and the actual AF would be very close to the AF estimated using the passive tracer.
- The authors say that one of the main objectives is to assess the methods used for computing the AF from the data generated from the field campaigns. Can the authors provide additional references to show which method is used for which field campaign and shed some light on how field data could be used for estimating AF? For e.g., what information is available during a field campaign and what calculations are conducted.
- The standard measurement of entrainment/mixing is done via liquid water potential temp. Can't these conserved variables be used for calculating the AF from field measurements?
- One of the key difficulties in estimating the adiabatic LWC from the field data is related to knowing where the cloud base is located. If the location and the condition at the base are known, then plotting a moist adiabat is sufficient to know the properties of the adiabatic parcel (Rogers and Yau 1996, Bohren and Albrecht 2000). In the current study, there is no mention or discussion about this method. In my opinion, this would be the most fundamental method from the point of view of the field data, provided we know the height of the cloud base and its properties. Can the authors comment on this? There might still be issues related to supersaturation that needs to be investigated.
- Line 355: "condensation that occurs after a mixing event can delete records of earlier evaporation/dilution events" – I do not think this statement is supported by the data or the discussion presented in this work. The LWC a parcel attains at a given height is an integrated effect of past entrainment/mixing/evaporation/condensation events. Without knowledge of this history, the final LWC cannot be computed. So, I do not understand the above-quoted statement from the authors. If the authors do not agree, they need to provide strong evidence to support this statement.
- Finally, the title of the paper is a bit too general. The objective of this study appears to be to assess various techniques used for estimating the adiabatic LWC and does not shed much light on the adiabatic core/mixing processes/adiabatic fraction in cumulus clouds. The authors could come up with a more specific title that reflects the scope of the work.

Minor Comments:

- The abstract should contain the key results/conclusions of this study.
- Line 21: Diffusion efficiency of what?

- Line31: conserved and not “conservative”.
- Lines 36-38: Can the authors give examples of scenarios when radiation and sedimentation effects can be neglected.
- Before Eq 8.: the definition of moist static energy is not correct. $h = L_v q_v + C_p T + gz$. Some additional clarification/steps are required in deriving Eq. 8.