

Interactive comment on “Top-down and Bottom-up aerosol-cloud-closure: towards understanding sources of uncertainty in deriving cloud radiative flux” by Kevin J. Sanchez et al.

Anonymous Referee #4

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This paper presents results from a variety of measurements during an intensive field campaign at Mace Head in Ireland. It is perhaps unique in comparing estimates of cloud drop number concentration and radiative fluxes at cloud top based on several significantly different methods for a handful of cases during the campaign. Given the disparity among the cases (i.e. cumulus/stratocumulus; coupled/decoupled; adiabatic/sub-adiabatic), as well as the presentation of the results, it is a little unclear how to generalize the results of the study. The most substantive result seems to be the successful application of method for adjusting a parcel model calculation of the cloud-top radiative flux to account for dilution of the cloud by entrainment that results in a flux estimate that agrees better with in-situ measurements of cloud extinction. The paper

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is appropriate for publication in ACP after addressing some minor revision.

In a couple of places some fairly arbitrary adjustments were made with inconclusive results. For example, in lines 319-322 the authors describe a test where the aerosol concentration imposed on the parcel model is arbitrarily reduced by 50% based on the notion that the aerosol concentration in the cloud layer of a decoupled boundary layer is likely to be less than what was measured at the surface. Yet the the change resulted in little change in the cloud-top radiative flux. How do the authors reconcile the small change in radiative flux for such a larger perturbation of the imposed aerosol concentration with their ultimate conclusion that the main source of error in their bottom-up radiative closure for the decoupled boundary layer cases is the lack of measurements to constrain the CCN concentration in the decoupled cloud layer?

In the conclusion it is stated that cloud-top entrainment is only observed on 2 out of 13 flight days, and a decoupled boundary layer on only 4 of 13 flight days. It might be valuable to include this in the abstract. While reading the paper, I was struggling to understanding the broader implications. Is there sufficient data to draw a tentative conclusion about the overall sign and/or magnitude of errors in bottom-up forcing calculations based on the surface station data at this location? If this can be addressed in any manner by the authors, then the paper will have substantially greater importance.

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