Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-214

Anonymous Referee #1

Referee comment on "Coupled and decoupled stratocumulus-topped boundary layers: turbulence properties" by Jakub L. Nowak et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-214-RC1, 2021

General comments

This paper presents a description of airborne measurements of turbulence during the ACORES campaign, distinguishing between two cases: 1) a fully coupled cloud-topped marine boundary and 2) a boundary layer that is partially decoupled from surface fluxes. The technical quality of the analysis appears to be excellent and, with a couple of exceptions noted below, is thoroughly explained. As such, I believe this paper makes a significant contribution to our empirical understanding of turbulence in the marine boundary layer. While it is longer than most manuscripts that I review, I'm not sure that it can be substantially shortened without omitting important information.

The focus of this study lies somewhat outside my own areas of greatest experience, so I'm not able to confidently assess the relationship between this contribution and prior work in this same area. That said, the reference list is extensive, and the authors appear to be thorough in drawing connections to earlier work.

Overall, my recommendation is that it be published after considering the suggestions for revisions below.

Specific comments

Lines 43–45: Could a reduction in cloud-top LW cooling due to an overrunning cloud layer at somewhat higher altitude also contribute to decoupling?

Line 106: LEGs are described as being 10 km long, but the time intervals shown on Fig. 2 seem too short at the nominal flight speed of 20 m/sec. I would prefer to see lengths and altitudes of the LEGs included in a table. Among other things, this is relevant to the question of flux sampling error (see comment further down).

The helicopter used weighs somewhere around 2000 kg and imparts substantial downward momentum and turbulent kinetic energy to the environment directly below it. In fact, rotor downwash speeds a short distance below the helicopter are probably around 30 m/sec, and the area of influence expands considerably with distance below the aircraft (albeit with reduced velocities). With that in mind, I would have liked to see more discussion, including any relevant references, in support of the assumption that a 20 m/sec forward speed is sufficient to avoid any influence by the rotorwash on the ACTOS package suspended 150 m below the helicopter, taking into account as well that the package probably trails behind the helicopter by some distance during forward flight.

I believe there should be explicit discussion of sampling error, and its relationship to flight leg length, in connection with the turbulent flux measurements. One newly published paper that seems relevant is Petty, G. W.: Sampling error in aircraft flux measurements based on a high-resolution large eddy simulation of the marine boundary layer, Atmos. Meas. Tech., 14, 1959–1976, https://doi.org/10.5194/amt-14-1959-2021, 2021.

Note for the authors or ACP copy editor:

The quality of the English writing is excellent. The only real indication that the paper was not written by a native speaker is the frequent omission of the articles “a”, “an”, and “the” in sentences where they would normally be expected.