Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC4
https://doi.org/10.5194/acp-2021-214-RC4, 2021

Review of "Coupled and decoupled stratocumulus-topped boundary layers: turbulence properties" By: Jakub L. Nowak, Holger Siebert, Kai-Erik Szodry, and Szymon P. Malinowski

Ian Brooks (Referee)

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-RC4, 2021

Overview

This paper presents an analysis of two contrasting stratocumulus topped, marine boundary layers over the subtropical North East Atlantic: a well-mixed case and a decoupled case. It presents a fairly detailed comparison of mean thermodynamic and turbulent structures based on measurements from an instrument package deployed on a tether ~170m below a helicopter, along with various measures of mixing/decoupling drawn from the literature.

The results are, for the most part, routine – such boundary layers are well studied (even if our understanding of all the interacting processes is incomplete), and most of the results are in broad agreement with previous studies (as noted in the conclusions). They remain, however, a useful contribution to the field, and do include some unique results – those of very small-scale turbulent properties: profiles of dissipation rate, and isotropy.

There is a limit to how much can be gained from analysis of individual case studies. I would encourage the authors to consider expanding their analysis in future to include all the flights from this campaign (many more than the two used here) to produce a more general synthesis of turbulent behaviour for the coupled and decoupled boundary layers.

Specific comments

The overall structure of the manuscript follows the conventional pattern of background / methods / results / conclusions. This is fine, but I found that the sheer number of different variables being defined resulted in a very long methods sections, where it wasn't always clear what the real utility of a particular parameter was. By the time the reader (or this reader anyway) gets to the relevant results, they've forgotten what all the different symbols and parameters are. It might be worth considering modifying the structure to mix parameter definitions and results – defining/explaining particular quantities immediately prior to presenting the results on them. This is very much a decision to be made on personal preference regarding the readability, I'm sure another reviewer would argue against doing this.

On a related note, there are a LOT of acronyms defined here, not all of them are used very often (eg 'CB' is only used 6 times after being defined…not worth the space saving traded off against having to go back and find out what it means'. I found it easy to confuse many of these because of minor inconsistencies in how the layer names mapped to acronyms– I kept reading 'SCL' as 'sub-cloud layer' instead of 'stratocumulus layer', whereas 'SBL' (sub-cloud layer) I wanted to read as 'stable boundary layer'…which is a common usage, but irrelevant here.

Figure 2 and 4 – it might be useful to indicate cloud base and top on the figures so the reader can immediately see how the flight legs relate to cloud level.

The line style for different sections of the flight track are consistent with those used on the later profile plots - this is clear for fig 4 (flight 14) where the profile plots show 3 distinct profiles; but less so on fig 2 (flight 5) where there are only 2 line types. It appears that in the profile plots the dashed line, which looks like a single deep profile, is actually a composite of several profile sections separated in time, and spanning different altitude ranges. This is fine, but should be made explicit since it has a bearing on variability of the data.

At various points in the discussion of results, specifically the plots of profiles and leg-averaged values, reference is made to a particular flight leg 'LEG2', 'LEG3' etc. I found this unhelpful, since I couldn't immediately identify which leg was which on the plots…what altitude was it? It would be more useful to simply refer to the altitude of the leg.
The legs can be identified by referring back to figures 2 and 4, but (a) that requires the reader to go searching back for the relevant figure, and (b) there is a potential cause for confusion, because the leg numbering (assuming it is chronological…this is never explicitly stated) appears to be inconsistent when referred to the profiles, since for flight 5 the legs start high and word down, and on flight 14 start low and work up (and then down again for final leg). All we really need to know in the discussion is the altitude, the leg number is

a distraction’

Line 169: ‘Negative values suggest instability…’ – for clarity it would be useful to explicity state the variables involved here ‘Negative values of Dq_l suggest…’

Line 171 ‘The parameter of YA00…’ – again, be clear and name the parameter, not (just) the paper where it was first defined…make it easy on the reader.

Line 186: ‘probably there were some clearings…’ – while the effects of such clear air regions will get averaged out by the vertical binning/averaging/smoothing applied to generate the profiles, it ought to be possible to identify if they actually occur from the raw, high rate data, and not have to rely on a vague statement of ‘probably’.

Line 202-203: ‘Suitable normalisation…’ – Purely my preference, but I’d cut this line. I don’t think it adds anything useful unless you go into detail about the normalisation & averaging referred to.

Line 232: what are the instrument issues that resulted in problems with the lateral wind components? It’s not essential to document this, but, depending on the cause, might be useful for other researchers trying to make similar measurements.

Line 294 and 307: both reference a ‘lateral component’ when the parameter referred to is derived from vertical velocity. Yes, w is ‘lateral’ with respect to the mean wind vector, but it might be clearer here to be explicit and refer to the ‘vertical component’, not least because you have previously noted problems with the ‘lateral’ velocity measurements, where lateral refers to the horizontal cross-wind component, and so is a potential source of confusion.

Line 374-377: The unexpectedly high variances above cloud are presumed to be artefacts resulting from the presence of gravity waves. While I agree that is quite likely, it should be possible to demonstrate it. Coherence/phase/amplitude plots of the correlation between vertical velocity and the other variables should show a clear scale of waves. Power spectra or ogive plots of variances/covariances should also show that most of the variance/covariance results from a narrow range of wavelengths that can be related to gravity waves.

Line 390: The statement regarding T and q as being passive tracers with no significant sources at the transition layer is…arguable. There is no real 'source', but for the SML, the gradient across decoupling transition layer acts as a source/sink term, entrainment mixing brings drier/warmer air down to top of SML (local effective source/sink). There must be some mixing to give high T/Q variances here.

Then…' The TSL features the gradient of qv (c.f. Fig. 6) which might explain increased local fluctuations.' – what other source of increased fluctuations could there be here?

Line 420: You 'speculate that the drivers of convection, i.e. radiative and evaporative cooling, are not efficient in this situation'. What is different about 'this situation' that either of these processes should be different? You can evaluate the evaporative cooling and CTEI parameter…is this weaker than for the other case? Certainly the latent and sensible heat fluxes are much smaller in cloud here than for flight 5.
Radiative cooling is more difficult to assess without direct measurements of the radiative fluxes, but there may be clues available. You mention the availability of the ARM remote sensing data…does that show a higher cloud deck that might reduce the radiative cooling from cloud top? This case does have a slightly thicker cloud and so higher LWC at cloud top…this will slightly modify (sharpen) the LW cooling and SW heating profiles, and maybe shift the relative positions of their peaks in the vertical, changing the balance of heating/cooling.

Line 422: 'moisture delivery from the ocean surface to the cloud might be more difficult in the decoupled STBL' – yes, it ought to be much more difficult.

Line 458: is the departure of measurements from theoretical expectations for homogeneity, isotropy and stationarity here a result of evaluating them from slant profiles? You note the horizontal legs are in much better agreement with theory, suggesting the profile results are not truly representative.

Line 555: 'which suggests important contribution of moisture to buoyancy' – I agree, but this could be evaluated properly. Buoyancy flux (virtual potential temperature flux) can be broken down into the sensible and latent heat contributions and their ratio determined.

Minor issues (grammar, typos, etc)

While overall, the manuscript is clear and well written, there are many minor grammatical issues – notably missing definitive articles: '…in **the** cloud top region…', '…in **the** inertial subrange…' etc. I have noted all those that jumped out at me below, but I'm sure I've missed more.

Line 4: ‘…in cloud top region’ -> ‘…in the cloud top region’

Line 12: ‘in inertial subrange’ -> ‘in the inertial subrange’

Line 22: ‘They occupy…, preferably in the conditions of large-scale subsidence.’ – ‘preferably’ is the wrong word (implies an ideal choice or active preference’, ‘preferentially’ is closer to the meaning required (with greater likelihood)

Line 28: ‘Primary mechanism…’ -> ‘The primary mechanism…’

Line 29: ‘Additional source of turbulence…’ -> ‘ An additional source of turbulence…’

Line 32: ‘…dependent on the level in which SC is coupled…’ – ‘in’ isn’t the right word here, and the meaning intended isn’t entirely clear, either ‘…dependent on the level at which SC is coupled…’ (if the issue of concern is the altitude at which decoupling occurs) or ‘…dependent on the level to which SC is coupled…’ (if the issue is whether, or how strongly decoupled the BL is).

Line 35: ‘…structure features adiabatic lapse rate (dry below cloud, moist inside), strong capping inversion at the top, near-constant concentration of moist-conserved variables…’ -> ‘…structure features an adiabatic lapse rate (dry below cloud, moist inside), a strong capping inversion at the top, and near-constant concentration of moist-conserved variables…’

Line 40: ‘ Stable or…’ -> ‘A stable or…’

Line 62: ‘…in conventional rationale…’ -> ‘…in the conventional rationale…’

Line 105: ‘…depended on local cloud…’ -> ‘…depended on the local cloud…’

Line 106: ‘usual strategy involved:…’ -> ‘the usual strategy involved…’

Line 109: ‘…and flight pattern…’ -> ‘…and a flight pattern…’

Line 140: ‘Brunt-Vaisala frequency…’ -> ‘The Brunt-Väisälä frequency…’

Line 143: ‘…quantifies vertical gradient…’ -> ‘…quantifies the vertical gradient…’

Line 166: ‘…as BL mean.’ -> ‘…as the BL mean.’

Line 183: ‘…where it features the increase of…’ -> ‘…where it features an increase of…’

And ‘…analogously, with the decrease of…’ -> ‘…analogously, with a decrease of…’

Line 184-185: ‘…capped by the layer of…’ -> ‘…capped by a layer of…’

Line 239: ‘Described modification…’ -> ‘The modification described…’

Line 243: ‘…with simple…’ -> ‘…with a simple…’

Line 244: ‘…from original signal.’ -> ‘…from the original signal.’

Line 249: ‘…taking average along LEG…’ -> ‘…taking the average along the leg…’

Line 252: ‘Worth to remember…’ -> ‘It is worth remembering…’

Line 260: ‘…such approach…’ -> ‘…such an approach…’

Line 266: ‘Range of scales…’ -> ‘The range of scales…’

Line 266: '…limited by the smaller among spatial resolutions of two multiplied signals…' the word smaller here might be read as implying the smaller scale (ie, higher resolution), suggest -> '…limited by the lowest spatial resolution of the two multiplied signals…'

Line 267-268: following the previous statement, you note the scales of individual measurements, but it would be helpful to be explicit and state the resulting scale for the final fluxes.

Line 317: 'In case of LEGs…' –> 'In the case of LEGs…'

Line 338: 'Similar approach…' -> 'A similar approach…'

Line 340: 'Such value of…' -> 'Such a value of…'

Line 345: 'Integral lengthscale…' -> 'The integral length scale…'

Line 347: '…integral of autocorrelation function…' -> '…integral of the autocorrelation function…'

'…in formal definition…' -> '…in the formal definition…'

Line 353: 'At Taylor microscale…' -> 'At the Taylor microscale…'

Line 365-366: The statement 'Depending on flight segment type, they are illustrated with continuous profiles (PROF) and/or dots with errorbars (LEG).' Is redundant, delete.

Line 372: '…reaches minimum value…' -> '…reaches a minimum value…'

Line 387: '…resemble typical mixed layer…' -> '…resemble a typical mixed layer…'

Line 390: '…exhibit maximum…' -> '…exhibit a maximum…'

Line 398-399: The statement ‘while the shear production at the bottom and at the top of the boundary layer’ is incomplete…needs some statement about the shear production.

Lines 395, 402, 406: statements about results are phrased as ‘seems to’, ‘appears to be’ etc. Unless there is real doubt, be definitive…is it as stated or not?

Line 495: ‘…immensely stable…’ -> ‘…strongly stable…’ (immensely might be overstating things a bit).

Line 561: ‘…vanishes at the level…’ -> ‘…vanishes at a level…’

Line 568-569: ‘Vertical velocity variance suggests the profile somewhat different than the convective similarity scaling’ -> ‘The vertical velocity variance suggests a profile somewhat different than the convective similarity scaling’

Line 659: ‘Main processes…’ -> ‘The main processes…’

Line 669: ‘imortant’ -. ‘important’

Line 670: ‘relevant systematical…’ – ‘…relevant systematic…’