Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., author comment AC2
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Reply on RC2

Francesca M. Lappin et al.

Author comment on "Low-level buoyancy as a tool to understand boundary layer transitions" by Francesca M. Lappin et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-68-AC2, 2021

Author Responses are enclosed in square brackets

[The authors would like to thank the reviewer for their insightful comments and suggestions to improve this manuscript.]

General Comments:

I find the manuscript to be well-written. The authors demonstrate how observations of buoyancy from remotely piloted aircraft systems operating in the ABL can have practical applications in the diagnosis of ABL height, convective initiation and low-level jets. The authors utilize two field studies for evaluation, both of which are appropriate for the kind of (free-convection-driven) environments where buoyancy measurements are likely to be useful. Specifically, the authors demonstrate how the proposed buoyancy-based method for ABL height diagnosis mitigates misdiagnosis from noise in the UAS-measured fields and is a viable method for both UAS and radiosonde-based profiles. One could argue that this article is less about RPAS observations and more about buoyancy as an ABL diagnostic, which would make it better suited for a journal other than AMT. I think the authors' well-written introduction and conclusion sections provide enough context for this manuscript to stand as an RPAS application, however.

I have listed below some specific comments regarding some of the statements made and the figures. I think some non-trivial adjustments are needed in places, but the foundation of the investigation is sound and can be revised into a manuscript ready for publication.

Specific Comments:

L 48: The authors characterize "ABL transitions" on this line through its impact on ABL height and stability and proceed with a strong overview of ABL height estimation methods. I think the description of ABL transitions could be expanded a bit beyond this one line, given the history of this topic itself and its prominence in the manuscript title, to help frame the term for the reader. For example, when is the temporal evolution of an ABL a 'transition' and when is it not? I agree with the authors' general characterization here; my only suggestion would be to include the influences from the 3-D flow on ABL transition that may not be diurnal, e.g., internal boundary layers, mesoscale circulations (such as the authors' example later around L 270?), etc., and transitions in forced convection environments). It's also worth noting that the word transition is used later (L 202) in a different context, seemingly referring to changes in the vertical temperature profile slope, rather than changes in ABL 'regime', which is the more common usage for the 'transition' term, I would argue. My impression after reading the full manuscript is that the authors are using low-level buoyancy from RPAS observations to diagnose boundary layer features and characteristics.

[The authors added L 54-57 were added along with two new citations (Angevine et al. 2020 and Brown et al. 2002) to add context to the term 'transition.' The Angevine paper has an in-depth discussion on ABL transitions and the paraphrasing added to this manuscript conveys to the various impacts on ABL transitions. The L 202 use of 'transition' was changed to 'vertical potential temperature gradient.']

L 128-132: Were the sensor bias corrections applied here identical a particular past campaign, or were new values used? If the latter, what were the new values and how were the corrections applied?

[The bias corrections are identical to the methods described in Segales et al. 2020 (AMT), regardless of the campaign. Each system has a designated scoop and sensor package that has a unique set of offsets gained from calibration upon fabrication. I added a direct point to this citation in lines 129-130 to make that clearer. Also, the scoops are calibrated periodically for sensor drift.]

L 173-174: Minor point, but I would disagree that radiosondes are Lagrangian tracers. The radiosondes here are balloon-launched, which provide lift that enforces upward motion (initially), regardless of the ambient vertical velocity (whether upward or downward). A tracer follows the ambient fluid velocity in three dimensions.

[The authors completely agree. This was brought to our attention after submission. Lines 175-176 were changed to reflect that radiosondes are more quasi-Lagrangian tracers.]

L 193-194: This sentence seems a bit unnecessary and I would suggest that it be omitted. Buoyancy is sensitive to temperature difference between a parcel and its environment, regardless of whether the parcel is in the lower troposphere.

[The sentence was omitted.]

L 195: The sentence reads: " . . . buoyancy becomes convectively well-mixed .. . ." Do the authors mean to say that the ABL becomes convectively well-mixed? Buoyancy being a force, I'm unclear on what is meant by buoyancy being convectively well-mixed. Following the authors' definition in (1), in a well-mixed unsaturated environment, the environmental temperature would follow the adiabatic lapse rate, which a buoyant unsaturated parcel would also follow. It's arguably the components of the expression that are "well mixed" (or constant). Perhaps an alternative expression would be to call it invariant? The expression is also used around L 280 where the authors first introduce "isotropic buoyancy". I think this term is closer to the conditions described, though isotropy does carry a connotation of direction.

["Convectively well-mixed was changed to "roughly constant with height in a CBL." The term "isotropic" was changed to "constant" to not convey that buoyancy is uniform in all directions, per the turbulence connotation. Also, in L 280 the structure was changed to reflect that there are parallels in vertical structure between potential temperature and buoyancy, but they are not both mechanically mixed.]

L 202-203: As the authors insinuate, this example shown in Figure 4 is not appropriate for the potential temperature method applied to these specific CopterSonde data due the deep well-mixed layer. Despite that, have the authors experimented with applying some kind of low-pass filter to the CopterSonde measurements to truncate the high-frequency variability? For this example, I have to wonder if, imagining the CopterSonde flew higher or the mixed layer were shallow enough for the CopterSonde to capture it fully, would the method applied to the CopterSonde data in Figure 4 still yield erroneously low ABL heights?

[The authors tried a handful of methods to eliminate the heights being found erroneously low. A low-pass filter was applied, as were linear splines to estimate gradients, and logical statements to not allow heights to be found lower than 200~m. None of these provided desired results, even the last option just moved the height to exactly one data point above the height restriction. It is because within the mixed layer the vertical gradients are miniscule, so the only sizable gradient occurs near the top of the surface layer. From 1432-1700 UTC, we can see in Figure 4 that the mixed layer is within the flight ceiling. As a result, the ABL height grows logically and agrees well with the radiosonde data, which suggests that it does work if the entire mixed layer is captured. Unfortunately, in the US it is difficult to get airspace cleared much higher than 1200 m. While the authors believe that if we could fly higher we would have better success, it is difficult to prove it.]

L 210-212: The authors contend that local winds combined with differences in measurement time yield discrepancies between measurements on the two platforms. I think this hypothesis could use some further analysis, given that winds are much stronger later in this 24 hr period, where there appear to be non-trivial differences between measurement times between the two platforms as well. Is there some kind of forcing at the mesoscale or synoptic scale (or something else?) to explain this condition, where the inter-platform differences early on are much more pronounced than later? To me it seems plausible that this difference could be partly related to the platform heating issue that was shown in Figure 2.

[The difference below 100 m could be a result of the heating issue, but above that height, the sensors are very well aspirated and should not be influenced by the temperature of the shell. Since the winds measured by the radiosonde in that layer are >15 m/s coming from the south and there was a 30-minute difference in sampling time, the radiosonde was likely taking measurements dozens of kilometers to the north, where it was colder at the surface.

I would also disagree that we do not see those effects when the winds are stronger. Where the winds are strongest, we cannot compare the measurements due to the wind constraints on the CopterSonde. The authors input new figures to capture the thermodynamics and kinematics of each case. In the new Figure 3c and 3d, we can see the specific humidity is different beneath the jet. The moisture surge around 0930 UTC is weaker using the radiosonde data.]

L 230: Could the authors please clarify what is meant in this sentence beginning with 'Contrary'? Shapiro et al. (2016) indicate in their paper (pg 3045) that buoyancy is specified to be at maximum a few hours before sunset, also depicted in Figure 7. From references to this elsewhere in the paper, surface buoyancy peaks at three hours prior to sunset. Given a local sunset time of about 0010 UTC, my visual estimate is roughly a four-hour difference between the peak of vertically integrated buoyancy and the time of sunset in the authors' Figure 6. This seems reasonably close to the result in Shapiro et al. (2016), in contrast to the authors' statement on L 230. In addition, the authors are comparing maximum surface buoyancy of Shapiro et al. (2016) to their vertically integrated buoyancy expression. Could the authors please elaborate on why and under what circumstances these terms are directly comparable?

[The authors agree, that this statement is more in agreement than in contrast. Upon further inspection, it appears I had made a mistake in thinking I was referencing an older paper that used the sunset time as buoyancy max. Since the Shapiro et al. 2016 paper is more recent, I have changed the format of the sentence to express that our results agree with the model results (L 241-243). The authors also added a point of difference of the vertical buoyancy evolution from each study. It was not shown in the manuscript but the vertically integrated buoyancy evolution aligns very well with the surface buoyancy since the vertical buoyancy profiles are nearly constant in a CBL. This also seems to agree with Figure 7 from Shapiro et al. 2016.]

L 265: I'm unclear what the authors mean by "process continues throughout the night . . .". The timeline of Figures 9 and 10 begins around 0530 LT (MDT) and continues until around 1100 LT. In the following sentence, are the authors referring to the inversion as shown on Figure 9, which increases after sunrise and then remains fairly constant in time?

[This line is referring to the overall process of drainage flow, not either of the figures. L 282-285 were restructured to make the ordering of the process more explanatory.]

Figure 3: Qualitatively, the two subplots appear to match-up well except in the first few hours, roughly before 1900 UTC. The CopterSonde wind measurements appear to noticeably underestimate those of the radiosonde at these times between about 400 m and 900 m AGL, but also in the region of roughly 100 – 300 m around 2000 UTC. Corresponding buoyancy values in this spatio-temporal region are also show non-trivial discrepancy in the initial hours at the upper levels. The authors provide some brief analysis later in the text (L 211-213). The authors contend that the discrepancy is due to a difference in location between the two platforms. I noticed that coherence between the two platforms is fairly high later on in the period, when wind speeds are considerably stronger. Is there a known physical reason to expect why the mixed layer temperature should be so different in an adjacent space 30 minutes later given the relatively flat and homogeneous nature of the domain early on? Can these buoyancy differences be explained by the platform solar heating problem (L 180-181)? Also, I would urge the authors to shift their analysis of Figure 3 earlier in the text, to where the Figure is first introduced.

[In regards to the observations before 1900 UTC, I think a large source of this discrepancy is that the radiosonde data is being interpolated across 3 hours of data that do not exist. The large difference in buoyancy aloft is a combination of the difference in cooler surface temperatures influencing the radiosondes 1432 UTC values and the compensation of the interpolation to bring the subsequent profile back to neutral between times of profiles. Looking around 1724 with each platform, the buoyancy profiles are pretty similar, as is the 2000 UTC one. So, those observations between 1432-1724 UTC assume a completely linear transition. This is especially untrue during transition periods, heterogeneity is very high. After 1724, the new Figure 3 shows strong agreement in potential temperature and specific humidity.

As for the wind speeds around 2000 UTC, I agree that there appears to be a non-trivial difference in the wind speed profile. Although, Bell et al. 2019 (https://doi.org/10.5194/amt-2019-453) showed that throughout the campaign the mean difference in wind speed from CopterSonde and the radiosonde data (and doppler lidar) is less than 1 m/s. The apparent difference is likely due to the binning on the colorbar is overexaggerating the difference in wind speeds. For example, the difference could be very small, say 11.9 m/s and 12.1 m/s, but binned in different colors to make it appear the difference is much larger. The authors kept the contour levels a bit larger to help with readability since the intercomparison of measurements is not the paper's goal.]

Technical Corrections:

L 20: decades old -> decades-old

[Fixed]

L 27: this type of data has –OR- these types of data have

[Fixed]

L 82: "Flux-Capacitor" may not be immediately recognized by the reader as the name of a campaign at this point in the manuscript. Suggest: "The Flux-Capacitor campaign" or "Flux-Capacitor (2018)", etc.

[Fixed]

L 83: Suggest: "Southern Plains" or "southern Plains" as the authors see fit – somewhere in here there's a proper noun and the authors are in a better position than the Reviewer to decide what that should be

[Fixed]

L 99: Suggest: San Luis Valley -> San Luis Valley, Colorado, U.S.A. given that AMT is an EGU journal.

[Fixed]

L 99, 111: Note that the same person (Gijs de Boer) is referenced as "de Boer et al." on L 99 and "Boer et al." on L 111. Suggest using the former naming format (and change to 2020a) and renaming the second reference as "de Boer et al. 2020b."

[Fixed]

L 103: Please define CASS here (first defined later on L 121)

[Fixed]

L 116: "the CopterSonde" hasn't yet been introduced at this point in the manuscript. Suggest replacing with " . . . utilized a rotary wing quadcopter RPAS . . ." or perhaps " . . .utilized a CopterSonde RPAS" for clarity.

[Fixed to "CopterSonde RPAS"]

L 124-125: ". . . pressure sensor within the autopilot." Do the authors mean that the pressure sensor is affixed to the autopilot board? Perhaps the authors could clarify here as the word autopilot is often used to refer to the software of the CopterSonde, too.

[The pressure sensor is built into the autopilot board to aid with altitude control. This explanation was added to that line.]

L 170: cross evaluated -> cross-evaluated

[Fixed]

L 176: long established -> long-established

[Fixed]

L 205-206: Doesn't this sentence effectively repeat the statement on L 201-203? Likely can omit.

[Fixed]

L 207-208: "the height of mixed potential temperature" -> "the height of the mixed layer"

[Fixed]

L 210: replace comma with semi-colon;

[Fixed]

L 226: Could the authors please include the equation used to compute vertically integrated buoyancy to verify the units as shown in Figure 6?

[It is not technically integrated, rather summed over the entire profile, which is why the original units are preserved. The authors have changed the phrasing from vertically integrated to vertically summed.]

L 234 -245: The verb tense changes over the course of these two paragraphs, from past to present to future. Please be consistent with verb tense throughout.

[Fixed]

L 260-261: Assuming that the authors mean that the buoyancy foretells of change to local convection strength (?), I would suggest 'portend', 'herald' or 'precede' as a replacement for 'predate.'

[Fixed]

L 269: Add 'UTC' after '1530'

[Fixed]

L 281: "below the ABL" -> below the top of the ABL (or similar)

[Fixed]

L 323: Note that AMT asks that authors include the DOI in the references where available (https://www.atmospheric-measurement-techniques.net/submission.html#references)

Figure 1: The caption indicates that the red lines are from the CopterSonde and the blue lines are from the parcel calculation, but the figure shows the opposite color assignment.

[Fixed]

Figure 1: Could the authors please enlarge the text of the legend in subplots (a) and (c) slightly to improve legibility.

[Nearly every plot was altered to increase readability and increase font size.]

Figure 1: Would suggest indicating the UTC times of the examples in the caption to help the reader distinguish the top and bottom rows of the figure.

[The figure titles were simplified and the time is in a more readable format]

Figure 2: In the caption, add a space between 2018 and Purple.

[Fixed]

Figure 2: The time of sunset (sunrise) in Washington OK on 05 October 2018 was approximately 1910 CDT (0727 CDT), or 0010 UTC (1227 UTC) (e.g.,: https://sunrise-sunset.org/us/washington-ok/2018/10). The sunset (sunrise) graph on the figure indicates approximately 2020 UTC (0121 UTC), however. Could the authors please revise the figure? Also, the abscissa tick mark labels do not appear to cover the time range indicated by the caption (1500 UTC 05 October 2018 to 1430 UTC 06 October 2018).

[Fixed]

Figure 2: The CopterSonde temperature graph (black) is drawn as a continuous line over this period (indicated to be approximately 24 hours according to the caption), but Table 1 indicates that these observations were taken during 46 flights averaging 30 minutes each. Why the black-shaded graph in this figure drawn this way? Could the authors please revise the figure to indicate where CopterSonde observations are present versus absent?

[The line was replaced with black points to reflect the non-contiguous measurements.]

Figure 3: In the caption, some text is missing before the word filled: e.g. …"and wind speed (m s-1) is shown in filled contours"

[Fixed]

Figure 4: Could the authors please enlarge the text in the legend in the upper right corners of both subplots to improve legibility? Same for Figure 5 and other figures where this point size is used.

[Fixed]

Figure 4: Please indicate in the caption the date and location of these measurements as done in Figure 3.

[Fixed]

Figure 4: The graphs indicating time of sunset appears a bit off with respect to the abscissa tick mark labels in subplot (b). The time of sunset should be around 0010 UTC by my findings for 05 October 2018 in Washington, OK.

[Fixed]

Figure 5: In the caption: buoyancy determined -> buoyancy-determined

[Fixed]

Figure 7: Could the authors please add a ruler showing distance to these subplots? The authors make frequent reference in the adjacent text to distances from the stations and it would be helpful to have that reference on the subplots. Also, it is somewhat difficult to discern where the mountains are located with the radar reflectivity present and the presence of multiple colors shading the surface. Perhaps the authors could add a subplot showing relief? This is mostly in response to the statements on L 243-244 about issues with radar coverage and interference from mountains. I'm not entirely sure I follow this

statement – are the authors referring to convective cell base height? I would anticipate convective cells within the valley would be recognized by radar if there were no obstructions between the cell and radar, no?

[The ruler was not included because it crowded the figure too much, as would a relief subplot. I have made reference to a paper which shows the aerial of the valley better in the methods (L 93-94). L 243-244 were rephrased stating that there is a lag in when the storms become visible to the radar because they must grow higher than the surrounding mountains. Since in some areas the mountains are 2 km higher than the valley floor, the towering cumulus stage immediately following convective initiation is obstructed by the mountains. It is not until the cell has entered its mature stage that it will grow taller than the surrounding mountains. Since this study was focusing on convective initiation, we relied in part on the ASOS station and personal accounts rather than the KPUX radar.]

Figure 7: Please add the label 'UTC' to the times labelled on the subplots, for clarity.

["UTC" was added to the subplots as suggested]

Figure 8: Suggest some rewording for clarity: "Unfilled contours are buoyancy (m s^-2) where solid (dashed) are positive (negative) and filled contours are specific humidity (g kg^-1) from CopterSondes . . . "

[Since the figure was changed, the caption was altered and I believe it is a bit more clear]

Figure 8: Date format in the subplot titles differs from the format used elsewhere in the manuscript (DD MMM YYYY).

[Fixed]

Figure 9 & 10: I would suggest merging these two figures as subplots of a single figure, as the authors did in Figure 8. The authors use both Figures 9 and 10 to describe the sequence of physical processes in the final paragraph of Section 5. I think it would be useful to have the figures side-by-side to facilitate comparison.

[The figures were combined and the references throughout changed as well.]

Please also note the supplement to this comment:
https://amt.copernicus.org/preprints/amt-2021-68/amt-2021-68-AC2-supplement.pdf