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Interactive comment on "Ship- and island-based atmospheric soundings from the 2020 EUREC^{4}A field campaign" *by* Claudia Christine Stephan et al.

Anonymous Referee #2

Received and published: 17 September 2020

The manuscript by Stephan et al. describes the radiosonde data set obtained during the EUREC4A field campaign. Weather balloons were launched from 4 ships and one island station at Barbados. All data were obtained using the Vaisala radiosonde system. The paper describes the setup, launch operations, data collection, and processing. The three levels of data processing are publicly available.

Some analysis of the data set demonstrates its huge potential and usefulness for atmospheric science.

The raw data are complete, the level 1 and level 2 data are ncdf files following the CF convention and appear to be properly formatted.

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The paper is overall well written and the data set overall well documented. However, there are a number of smaller issues, which should be addressed. I would recommend publication after some corrections.

Detailed comments:

The data set of the Meteor contains a set of corrected raw data files. Can you elaborate why a correction was needed and how it was applied?

Lines 80: Please describe here down to what altitudes descent data were typically recorded. This is given later in the manuscript, but should be moved to here.

Lines 81: add "... nearly match fall speeds in the middle and lower troposphere to balloon ascent speeds".

Line 82: "somewhere above each platform": Could you please make a statement about the drift of the soundings over the vertical region of interest. Something like the average horizontal distance between ascent and descent measurement at a relevant altitude.

Line 83: Which software version of the MW41 system was used at each station?

Since the RS41 SGP sondes were used, I assume the reported pressures are the measured pressures. Since the Modem sondes used GPS height for that purpose, it might be useful to highlight that difference.

Line 95: add "... which sometimes delayed soundings ..."

Throughout the description of the balloon filling at the different platforms, it was not very clear, how the amount of fill gas was gauged. Do I understand correctly, that the volume was estimated based on for example a marker (e.g. R/V Meteor) inside the container? Or was there some attempt to measure the amount of gas by monitoring the gas pressure, explicitly measuring the volume or measuring lift? Please clarify.

What balloon size/sizes were used at the different stations? On soundings with parachutes, were balloons with internal parachutes used, or were parachutes added

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externally? If this was different on the Meriam, it could possibly explain their larger number of faster falling sondes.

Lines 118: The nighttime soundings during leg 1 used less helium to increase the vertical resolution. This was changed after that. Was there no value in doing so? Was anything useful learned? The authors could briefly explain this change.

Line 231: Figure 11 is referenced before Figure 10, which needs to be corrected.

Line 106f, 122ff, 133 ff, 155ff and lines 207ff: The influence of a ship on observations near the surface is well documented and understood. It is not very clear how this was handled here. I understand that the operators tried to minimize that effect by launching from a location on a ship that minimizes this effect to the extent possible. Was any additional data screening done to evaluate and filter the effect of a ship in the lowest 50 m or so? The Vaisala system has a setting that filters out data showing a superadiabatic lapse rate. If that setting was used, then some of the ships influence may be filtered out by the Vaisala system in their level 1 data.

The setup of sounding systems on ships can be tricky, since launch site, receiving antenna, reference pressure sensor, and wind measurements may be several 10s of m separated vertically. Have you verified that the altitudes in the lowest 50 m are all reasonable and consistent? I noticed that there may be unreasonable jumps of more than 20 m in the first second after launch.

I checked the altitude of the BCO launch site, which is set at 25 m in the files, but may only be 13 m in reality. It's not clear at what level above msl the balloons were launched from the ships.

Maybe the authors could comment how important the lowest 50 m of profile are in their studies. I guess they were not very important, but offsets like 10 may shift the entire profile and may be significant in high resolution studies.

Lines 230ff: I assume that the difference in atmospheric conditions downwind and an

hour later would increase the scatter between ascent and descent measurements, but should not cause any systematic bias. A different sensor response (including GPS) between ascent and descent is more likely to cause systematic biases.

Line 255: Please move the definition of the hydrolapse from the legend of Figure 7 to here.

Lines 270ff: The patterns Sugar, Gravel, Flower and Fish are not obvious and scientifically accepted patterns. Please describe these here.

Line 351: Change to "... together with the NRT SST maps produced by ...". Can you add, which satellite(s) is used for these maps?

Line 384: Was there a particular reason to use the Magnus-Tetens formula for the M10 sonde humidity calculations? This could have been handled the same as the Vaisala sondes. However, I do not expect that the differences are significant over the region of interest.

Figure 5: The spread of the rise rate appears a little large, in particular in the stratosphere, where the balloon rise rate becomes a lot more uniform. I don't believe that ascent rates are calculated on 500 m bins, rather I assume that the 1 s calculated rise rates were binned in 500 m bins. The spread shown in this Figure is most likely due to noise in the pressure data. If the rise rate was calculated based on GPS altitude or better still based on 500 m altitude bins, the spread should decrease significantly. I do not think it is necessary to redo this plot, but it would be good to explain the spread.

Figure 6: What does "time averaged" mean in this Figure? If the same time averaging was used on ascent and descent and then the data were binned to consistent altitudes, I would not be surprised of biases due to the different descent rate profile compared to ascent. However, that may not be the case. Please clarify.

Figure 7: What is the axis label "LTS [K]"? I assume this is the 700 hPa potential temperature, but the axis label indicates something else.

Figure 8: The legend refers to humidity profiles on descent below the last received data. However, the Figure does not refer to descent measurements. This sentence can probably be deleted.

Figure 11 f shows some trajectories but does not fit with the rest of the panels and is not described in the text. This panel could be made a standalone figure and address my point regarding the average drift.

Figure 12: The legend indicates that this Figure shows 437 profiles, but the sum of North and South does not add up to that number. More than 100 profiles seem to be missing.

The appendix describes some results of the Modem radiosonde launches. I would suggest to add a few sentences to the general data processing similar as the description of the Vaisala data. Was all data QC done by the Modem software? Were similar data levels (0/1/2) generated?

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