Review of "A modular field system enabling cavity ring-down spectroscopy of in-situ vapor observations in harsh environments: The ISE-CUBE system"
By Andrew Seidl et al. AMTD

This paper presents a new compact packing system for a commercial cavity ring-down laser spectrometer that is intended for in-situ deployments in harsh (here cold, polar) environments. The manuscript also presents a few additional modules such as a cold trap module and a profiling arm. While I find the paper well-written and very interesting to read, I have a few fundamental critiques in particular with respect to the structure and focus of the paper that the authors should reflect upon and address before acceptance of the manuscript.

1) **Technical innovation and significance:** While I really like the level of technical detail and completeness of the description of the ISE-CUBE, I do not fully understand why it stands in the center of a paper publication. Dozens of previous scientific investigations have been conducted in different in-situ installations of cavity ring-down spectrometers in containers, cars, ULM, ventilated aluminum housings, tents or aircraft racks. All these deployments were done in such a way as to address the scientific question at hand in the best possible way. The ISE-CUBE seems a useful packing method for exactly the chosen deployment: namely near-surface profiling of stable water isotope gradients in cold environments. But already in the midlatitudes and especially in the tropics the chosen setup would not work due to overheating. In my view the technically relevant and innovative part of this study is not the CUBE but the profiling arm, which however is only very sparsely addressed. Therefore, in my view the profiling arm should stand in the center of the story framing. The full use of the compact packing provided by the ISE-CUBE only becomes obvious, when combined with the profiling arm. The authors should seriously reconsider their storyline, provide a better literature-overview of existing studies with different in-situ deployments, and justify why such a detailed presentation of a very specific packing is useful to the community. To me the fact that the system is not autonomous in terms of power use is a big drawback and doesn't make the system so much more flexible than a sheltered installation with a long inlet-line.
2) **Motivation for a profiling system of the near-surface profiles within 2 m above the surface:**
   - As mentioned above I really like the profiling arm and think that this is a clear innovation and add-on to the current state of the art in the isotope literature. It also has in my view clear potential for scientifically relevant investigations. The authors should mention these in the introduction more explicitly: why is it important to measure near-surface humidity/isotope and potential other trace gas gradients up to 2 m height?
   - Normally bulk fluxes are computed using measurements over about 10 m depth near the surface, why are the authors interested in the lowest 2 m?
   - The authors should highlight more clearly in the introduction why in a polar environment it is of utmost importance to have short inlet lines (due to strong interactions with the tubing walls at low concentrations, longer response times, lower precision at low concentrations).
   - What makes a profiling arm with free choice of sampling height more valuable than a setup with a manifold and inlets at discrete heights? This is an essential argument

for the profiling arm and should come very early in the manuscript. It is now mentioned only at the very end at L. 465.

3) **Section 4.1 & 4.2:** this section is much too detailed: 7 pages to state that the measurements were essentially unaffected by the harsh environmental conditions seems exaggerated to me. I am conscious of the effort that the authors put into the data analysis to come to that conclusion (stated at L. 374-376) and I fully acknowledge that this effort is worthwhile. Figures 5 to 7 with respective tables and shortened text would make an excellent supplement. But the information given in the paper should be succinct. The DAS temperature is not that relevant for the measurements, much rather the cavity temperature and pressure should be kept stable (this can be summarized in a few sentences). The WLM discussion in Section 4.1.3 remains inconclusive to me. The importance of the air prewarming by using the exhaust of the pump module can be mentioned in the methods section. A maximum 1-page summary of these results putting forward mainly the results of Section 4.2 (L. 399-402) with Fig. 10 should be sufficient to describe the main results and keep the reader's attention focused.
4) **Cold trap module:** This is an interesting adaptation of the Peters & Yakir 2010 system. However, if no data from this system is shown and compared to the CRDS data, then this part should be left away. Currently, this part of the paper is difficult to assess without data.
5) **Field calibration expansion module:** I do not understand why a calibration module is useful for such a deployment, which needs manual handling of the system anyways. Recent studies have shown that CRDS systems operate reliably over the timescale of several days with minimal drift (without calibration), such that a calibration every few days (1-3) is entirely sufficient and can be done in shielded temperature regulated conditions. See also the statement of the authors themselves at L. 220.
6) **Profiling module performance:** as mentioned above, I think that the real innovation of this paper is this profiling arm, which also makes the need for a low-footprint and modular box clear to keep the length of the inlet line at a minimum. Unfortunately, the authors put much more effort in sections 4.1 and 4.2 than in the key sections 4.4 and 4.5.

   - I recommend restructuring these sections and showing more results on this essential part. In my view L. 421 to 428 should be in the methods.

   - The response time of the system & precision at the encountered concentration should inform about the ideal length of the measurement periods at a given height. This point should be discussed. Is 30 s averaging ideal also from a signal-to-noise ratio perspective? Or should it be longer?

   - The temperature sensor calibration is a good thing to do, but a Supplement figure would be sufficient.

   - Fig. 12 is (one of) the most interesting figures (together with the very nice technical drawings in Figs. 1-3) but it is difficult to read because it is shown as a time series (and too small with many panels). A profiling arm allows to measure profiles, so why not show profiles? When looking at Figure 12 and considering the main aim of the paper (providing a modular system that is able to measure near-surface isotope and trace gas profiles in cold environments) then I wonder: can the proposed setup resolve the vertical gradients given the uncertainty of the measurements at these low concentrations? The authors should show the vertical profiles under different

conditions including the total uncertainty of the measurements and discuss this very important question. Also, in addition to the isotope, temperature and wind information they should add the water vapour mixing ratio and dexcess.

Detailed comments:

I refrain from a detailed list of language and technical comments here, given my advice above for fundamental reorganization of the paper. I however chose to list a few points that need clarification in the text:

- L. 15: which processes? Those relating to fluxes?
- L. 17: during stable stratification -> really only then? I can imagine many situations in which the stratification is not stable and in which near-surface measurements would be very useful.
- L. 20: "disentangling water vapor of different origin and undergoing different" processes -> do you want to disentangle the water vapor? Or the different sources of the water vapour?
- L. 23: did Steen-Larsen et al. 2013 investigate moisture sources? Or airmass origin? Maybe choose a different reference here. Also Sodemann 2017 is a proposal that is not accessible online and not a document that I would expect to be referenced in a peer-review paper. Maybe Sodemann et al. 2017 is meant?
- L. 26: remove "or so" (spoken language)
- L. 30: what is the advantage of an in-situ system such as ISE-CUBE-profiling-arm over a line with a manifold? Please be more explicit. This touches upon the key innovation of this work.
- L. 51: Wall effects -> indeed very important and how is that addressed? How long are the response times of the system? This is important for the profiling strategy (i.e. how long does the arm stay at a given elevation)
- L. 101: stand-alone field operation -> no power (how much in total?) is needed
- L. 169: I would say this is a typical example of an unstable situation at least over the open ocean.
- L. 190: An overview … **is given**.
- L. 204: if that is a central tool to this publication it should be made available online along with the data
- Section 3.4: I have difficulty assessing if the comparison dataset from the lab is an adequate one to use. Is the amount of data (sample size) and sampling frequency comparable to what was used in the field? The description is a bit vague in this respect.
- L. 387: the fact that the vials have to be manually changed in the cold trap module should be mentioned in the methods.
- L. 353: I cannot follow the argument why the only slightly larger RS at low water vapour mixing ratios in the field necessarily implies less accurate measurements in the field.
- L. 360-362: so then why such a detailed discussion of these metrics?
- L. 416-419: I don't understand why the authors introduce the cold trap module if they don't use its data. That makes it difficult to assess if the system is fulfilling its purpose.
- Fig. 11 should go to the supplement.

- L. 424: how were these response times estimated, to me the averaging intervals are also a key factor for optimizing the signal-to-noise ratio and obtaining the best possible precision.
- L. 449: “strongly stable”, this is even an inversion
- L. 454: which isotope gradients do you mean here?
- L. 458: we captured d18O and dD (leave away “isotope signatures” of, that is a repetition)
- L. 462: what does “the temperature gradient… converged” mean here?
- L. 463: not shown -> but that would be very interesting!
- L. 463: this is very important and should be mentioned in the introduction as a motivation for the ISE-CUBE with profiling arm system.
- L. 483: “Flexibility of the measurement’s height… with strong tides” interesting, but I missed that argument in the results part of the manuscript
- L. 486-491: the authors should compare the cold trap sampling to the CRDS measurements or leave it away.
- L. 492-498: As mentioned above, I do not understand why this is needed, as long as no autonomous operation over months is targeted.
- Fig. 12 is very small and difficult to read. Also, the information would be much more accessible (and interesting) in the form of vertical profiles instead of timeseries.

In summary, I very much like the profiling system presented by the authors and strongly encourage them to focus on this aspect, presenting its performance and limitations in an accessible way to the readers and the community. I think that the paper will gain in attraction, if shorter and more focused on the vertical profiling capability.