Dear Editor and Referees,

In reply to the comments of the referees, we have made substantial changes to the manuscript. This includes large portions that have moved or been removed in the work, including additional analysis. In addition, many figures have changed, including the order. We believe that we have been able to address all comments and questions by the referees, and we believe that the work has been improved. Below we provide a point by point response. Please find the tracked changes of the manuscript in an additional attachment.

The Authors

RE: https://doi.org/10.5194/amt-2022-208-RC1

Original referee comments in blue, with author response in black.

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.

We have reframed the manuscript to present a field profiling system. The name of the manuscript has also changed to "*A modular field system for near-surface, vertical profiling of the atmospheric composition in harsh environments using cavity ring-down spectroscopy*"

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?

We now introduce this importance in Section 1 Introduction.

- 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?

Now put forward in Section 1 Introduction L.13: "Near surface (<2 m) gradients of scalars can be strengthened significantly due to the stable stratification that often occurs in these regions

(Jocher et al., 2012; Zeeman et al., 2015) , which ultimately govern the fluxes of trace gases such as methane, carbon dioxide, and water vapor.”

- 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).

We now introduce this importance in Section 1 Introduction.

- 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.

We now put forward the advantages of the arm over a fixed-height inlet system in Section 5 Discussion, as well as some general shortcomings of fixed-height inlet systems in Section 1 Introduction.

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.

Section 4.1 has been significantly shortened, including a more clear and concise presentation of the WLM results, which now includes further analysis.

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.

We now present two analysed samples for assessment in Section 4.4 Cold Trap module performance.

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.

We have removed Section 2.6 Field calibration expansion module (proposed), but have moved some of the points to Section 5 Discussion regarding longterm deployments.

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.

Analysis in this section is now much more comprehensive.

- 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.

These lines have not been moved to the methods but have been modified into a more detailed presentation of the inlet response. See also the answer to the next comment.

- 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?

We have now revised the Section 4.3 Profiling module performance to focus on the system's response and the necessary time at a given height.

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

This temperature intercomparison has been moved to the Supplement

- 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.

Figure 12 has been remade into Figure 10. General wind information is given in the text of Section 4.5 Example of a profiling operation, as it was from a single level.

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.12: Changed to "knowledge gaps on these processes"

- 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.13: "Near surface (<2 m) gradients of scalars can be strengthened significantly due to the stable stratification that often occurs in these regions (Jocher et al., 2012; Zeeman et al., 2015) , which ultimately govern the fluxes of trace gases such as methane, carbon dioxide, and water vapor.”

- 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?

We have removed this sentence.

- 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?

We have removed this sentence.

- L. 26: remove “or so” (spoken language)

L.21: We have removed the time reference. “Laser spectroscopy has enabled…”

- 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.36-39: We present limitations of multi-line manifolds.

Most of the advantages (as well as limitations) of the arm are now put forward in Section 5 Discussion.

- 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.36: “Therefore, short, heated inlet lines limit potential interactions between water vapor and the inner walls of tubing. The use of short inlet lines also promotes a faster response of the CRDS analyzer”

L.113: “1/4 inch stainless steel tubing (Swagelok Inc.), heated to 60 °C with self-regulating heat trace cable (Thermon Inc.), and surrounded with 2 cm thick foam nitrile insulation.”

Response times will be elaborated upon in Sect. 4.3 Profiling module performance

- L. 101: stand-alone field operation -> no power (how much in total?) is needed

L.105: “Together, the Analyzer module and the Pump module are the two essential modules for in-situ isotopic measurement of water vapor.”

- L. 169: I would say this is a typical example of an unstable situation at least over the open ocean.

Agreed. When referring to stable conditions/startifications in the manuscript, it pertains to the measuring conditions over the snow.

- L. 190: An overview … is given.

L.190: “The ISE-CUBEs produce two main data streams, pertaining to the Analyzer, and Profiling modules, with each module internally recording its own respective stream. The Cold Trap expansion module does a similar task for its own data stream. An overview of the information contained in these data streams is given in Table 1.”

- L. 204: if that is a central tool to this publication it should be made available online along with the data

We now detail and list the different time resolutions used in the manuscript on L.204-209.

- 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.

Our lab benchmark is approximately 5 times larger than the field dataset. In the lab, the analyser sampled at 1 Hz, while in the field the analyser had a sampling rate of 4 Hz. However the comparisons done between the two environments are both at 1 Hz. As mentioned above, these details are given on L.204-209.

- L. 387: the fact that the vials have to be manually changed in the cold trap module should be mentioned in the methods.

L.149 now reads “*After the sampling period is complete, the flow is shut off with the needle valve (Figure 3, "D"), and the vial is manually removed, sealed, and stored until laboratory analysis, which can be done from the same vial.*”

- 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.333-336: “While this indicates that the measurements in the field have the potential for larger uncertainty, obtaining an exact quantification of uncertainty from this difference is non-trivial and requires access to proprietary analyzer details. Therefore, we now proceed with an alternative method to quantify the quality of the water isotope measurements from the ISE-CUBE system.”

- L. 360-362: so then why such a detailed discussion of these metrics?

We have done further analysis with the laboratory benchmark, limiting comparison to times when only synthetic air is being used. This makes the comparison much more pertinent. Additionally, we now introduce the Zeppelin Observatory dataset in Section 3, as its own reference period for the WLM.

- 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.

As mentioned above, we now include two analysed samples for comparison in Sect. 4.4

- Fig. 11 should go to the supplement.

This figure has moved to the Supplemental Material.

- 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.

We present the inlet response in Section 4.3

- L. 449: “strongly stable”, this is even an inversion

Section 4.5 Example of a profiling operation has changed substantially, including this line.

- L. 454: which isotope gradients do you mean here?

Section 4.5 Example of a profiling operation has changed substantially, including this line.

- L. 458: we captured d18O and dD (leave away "isotope signatures" of, that is a repetition)

Section 4.5 Example of a profiling operation has changed substantially, including this line.

- L. 462: what does "the temperature gradient… converged" mean here?

Section 4.5 Example of a profiling operation has changed substantially, including this line.

- L. 463: not shown -> but that would be very interesting!

We are currently preparing a manuscript for Earth System Science Data.

- L. 463: this is very important and should be mentioned in the introduction as a motivation for the ISE-CUBE with profiling arm system.

We now mention this and other advantages in Section 1 as well as in Section 5 Discussion

- L. 483: "Flexibility of the measurement's height… with strong tides" interesting, but I missed that argument in the results part of the manuscript

Now mentioned in Section 5 Discussion

- L. 486-491: the authors should compare the cold trap sampling to the CRDS measurements or leave it away.

As mentioned above, we now include two analysed samples for comparison.

- L. 492-498: As mentioned above, I do not understand why this is needed, as long as no autonomous operation over months is targeted.

We move the discussion of deployment duration to Section 5 Discussion

- 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.

This figure (now Figure 10) has changed substantially, including larger text.

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.

References

Jocher, G., Karner, F., Ritter, C., Neuber, R., Dethloff, K., Obleitner, F., Reuder, J., and Foken, T.: The Near-Surface Small-Scale Spatial and Temporal Variability of Sensible and Latent Heat Exchange in the Svalbard Region: A Case Study, ISRN Meteorology, 2012, 1–14, https://doi.org/10.5402/2012/357925, https://www.hindawi.com/journals/isrn/2012/357925/, 2012.

Zeeman, M. J., Selker, J. S., and Thomas, C. K.: Near-Surface Motion in the Nocturnal, Stable Boundary Layer Observed with Fibre-Optic Distributed Temperature Sensing, Boundary-Layer Meteorology, 154, 189–205, https://doi.org/10.1007/s10546-014-9972-9, http://link.springer.com/10.1007/s10546-014-9972-9, 2015.

RE: https://doi.org/10.5194/amt-2022-208-RC2

Original referee comments in blue, with author response in black.

This manuscript describes a new modular box enclosure called ISE-CUBE that can be used to deploy water vapor isotopic analyzers and water vapor isotopic cold-trap systems in the field under extreme cold-weather conditions. The manuscript provides a short description of the enclosure and subsequently evaluates the isotopic analyzer's housekeeping variables from a two-week winter deployment in Svalbard. The housekeeping data suggest the analyzer is able to maintain satisfactory ranges for its Data Acquisition System temperature, its cavity temperature and pressure, and its warm box temperature. The analyzer's water isotopic measurement precision in the field is also comparable to its measurement precision while sampling calibration gas in a laboratory setting.

The manuscript also describes an optional "profiling module" for ISE-CUBE, consisting of a tripod with an articulating measurement arm, that can be used to position a heated inlet line for the isotopic analyzer anywhere from 4 to 205 cm above the ground surface. A 90-minute window of data is presented that shows water vapor concentrations and isotope ratios from six height levels within the articulating arm's 2-m range. The paper argues that the articulating arm provides a means to resolve and study the water and isotopic gradients closest to the surface, although doing so requires repositioning the inlet height via a manual pulley every few minutes.

Comments

Where this paper really advances our measurement abilities is in the design and presentation of the box enclosure for the isotopic analyzer and cold trap; yet most of these details are in the Supplemental material instead of in the main paper. I would recommend revisiting Figures 1-2 and using these to convey specific details about the box connections and tubing materials, something more akin to the Connectors Template in the SI. As currently presented, Figure 1a is simply too dark to make out details, and Figure 1b requires more detail and explanation. For example, what are the "power out" and "data" ports used for? Where are the fan inlets and exhaust ports? Where do the boxes connect to one another? Is the CRDS inlet unheated after the check valve? Which lines are PTFE and which SS? In addition, the main text mentions components such as an "adapter," an "exterior inlet bulkhead," "incoming ventilation tubing," and "manifold tubing." Can these be labeled on Figs 1-2? The list of components in Appendix A is fantastic. Consider also a corresponding diagram (again, like the Connectors Template) that shows where all these components go and telling readers how many of each part are required to replicate the system. Another way to think about this: what would a purchase list look like?

As mentioned during the discussion, the focus of the manuscript has shifted towards the system as a profiling platform. We have updated Figures 1 and 2 (now Figure 3), to include annotations. Details on Figures 1-3 are found in the text of Section 2. We've also assembled detailed lists that describe the components necessary for each module, as well as cost estimates, in the Supplemental Material (Section 1.1)

It would be helpful if the manuscript discussed the relevance of the ISE-CUBE enclosure to the wider measurement community. Much of the manuscript is specific to the deployment of a Picarro CRDS water vapor isotopic analyzer. Would ISE-CUBE work for other types of isotopic analyzers? If the enclosure is specific to the size and shape of the Picarro systems, could ISE-CUBE work for other gas-phase Picarro analyzers? Moreover, based on the short two-week deployment in Svalbard, is there any sense whether ISE-CUBE could last for longer periods for unattended measurements?

We now discuss these points in Section 5 Discussion

On a related note, the Data Processing section (Sect. 3.2, including Table 1) presents ISE-CUBE as producing three data streams generally, but these three streams are specific to the way the modular system was set up for testing during ISLAS2020. It would be helpful if the paper distinguished more carefully which aspects of the design are generic and applicable broadly vs. specific to the test case configuration.

Section 3.2 Data processing has been rephrased to distinguish between primary and expansion data modules.

The enclosure is presented as novel, in part, for minimizing disturbance to the environmental flow, but I think this claim might be overreaching, since most ground-based installations are designed to minimize flow disturbance (e.g. flux towers). The real draw of the enclosure in my mind is the ability to deploy a water vapor isotopic analyzer in an environment with minimal infrastructure support (e.g. nothing more than a power drop) and/or to reduce the length of inlet lines and thus measurement hysteresis.

We now discuss the novelty of the ISE-CUBE stack alongside the profiling arm for profiling purposes in Section. 5 Discussion**.** In this same section, we discuss the individual value of the ISE-CUBE stack for deployments (similar to the comment two above).

To evaluate ISE-CUBE, the water vapor isotopic analyzer's performance in the field is compared to its performance in the laboratory. The intention is to compare two distinct environmental settings. However, there is another relevant difference that needs to be communicated more transparently: in the laboratory, the analyzer samples reference gas continuously, whereas in the field, the analyzer is measuring real variability related to the environment. I would not be surprised if this difference in sampled air causes the differences in humidity-binned standard deviations presented in Fig. 10 or results in the differences in spectral-fit residuals (RS) presented in Fig. 8. The paper concludes that the field data are "marginally less precise," but, again, I wonder if this is not just a reflection of the environmental air. Would one reach the same conclusion if the analyzer were measuring reference gas while deployed as part of ISE-CUBE in the Arctic?

We now conclude Section 4.2 Measurement quality of water vapor isotopes with (L.370) "In summary, the field deployment exhibits consistently higher variability for isotopic measurements, as compared to the optimal measurement conditions in a well-controlled research laboratory" and further mention this possibility in L.372: "but could also be due to the more variable composition of the ambient air used to quantify stability"

While I'm not sure it is necessary for the point the paper is trying to make, it would be awfully interesting to see how the isotopic analyzer and cold trap compare during the ISLAS2020 deployment. Such a comparison could provide some indication of the accuracy of the isotopic analyzer when deployed with ISE-CUBE.

Section 4.4 Cold Trap module performance now includes two analysed samples, compared to CRDS measurements.

Lastly, for Fig. 12, it appears there are environmental data missing during the period highlighted in the text (9:07, onwards). In addition, it would be helpful to know, are these data from the AWS? And can the figure be made larger?

Figure 12 (now replaced by Figure 10) has been refashioned to better represent a profiling operation.

Overall, the paper is very clearly written; however, a few minor comments on presentation are provided below:

L 15 - perhaps “components” instead of “compartments”

L.20: “ different reservoirs of the hydrological cycle”

L 29-32 is a bit awkward and could be presented more clearly

This sentence has been broken up into smaller pieces in the introduction: L.24-27 pertains to previous structures, and L.28-33 now focus on the inlet lines.

L 39 and elsewhere - “pneumatically” might be the wrong word as this implies compressed air

There really isn’t a great single word to replace this; we have now used “sample transmission” or “gas transmission” instead.

L 165 - is there a reference for ISLAS2020?

The data paper for this campaign is in preparation for Earth System Science Data.

L 177 - Does the ISLAS2020 data span 21 Feb to 14 Mar?

It does, though part of that time (29 Feb to 3 Mar) had our instrument installed up at Zeppelin Observatory (472 m ASL). The remaining days were dedicated to calibration and maintenance inside the Marine Laboratory.

L 187 - “reliable” seems like the wrong word for what is intended

We have removed this paragraph.

L 219 requires clarification

L.227: Now reads, “For both isotope species, this standard deviation is similar (or smaller) than the standard deviation typical during any individual calibration.”

L 239 and elsewhere - “minutely” means “meticulously.” I think the paper intends to say “1 minute”

We have changed all occurrences, and will certainly remember this for future works.

L 375 - since “field” and “laboratory” have specific meanings, I would use “remote observatory” or some other phrase here

This line has been removed in the revised manuscript.

Fig. C1 - caption says observatory margins are for the same “time” but not the same dates, right? Perhaps clarify.

The Figure C1 caption now reads “Black shading denotes spread between 2nd and 98th percentiles of Warm Box temperatures during laboratory benchmark. Gray shading indicates the same, for the deployment period in the Zeppelin Observatory. Blue lines are brief site visits, lasting on the order of 5 to 10 minutes.”

L 521 - says “could be” but should it say “is” or does it really mean “might be comparable” (as in it’s unknown)?

L. 532 now reads “Accordingly, recovery time for the TWB would also be longer during the profiling periods, though the magnitude of the TWB dip/spike is independent of site visit type.”

Fig. D1 seems to be missing the gray shading mentioned in the caption

This figure (or a version of it) has now moved to become Figure 6. Additionally, we have removed the linear regression line and the associated grey shading.

RE: https://doi.org/10.5194/amt-2022-208-RC3

Original referee comments in blue, with author response in black.

General comments:

I found this to be a clearly written paper, well-structured and systematic. It is a little long for the content so an attempt to shorten some sections would be welcome.

Section 4.1 has been substantially reduced, though Section 4.3 Profiling module performance has been extended due to the manuscript now describing a profiling system.

It provides a detailed account of the design and testing of a well engineered set of enclosures for the isotope analyser and some peripheral equipment. It looks to be quite costly - please provide the cost of the parts to build this. Many users of these instruments would look for low-cost solutions to building enclosures and peripherals and in many cases sensible cost cutting can be made without substantially affecting performance.

Section 1.1 in the Supplemental Material now includes detailed breakdowns of what each module contains, in addition to approximate cost. We also put forward some cost-cutting alternatives that wouldn't affect system performance.

The paper would benefit from a better justification of the need for this design, i.e the advantages of this design compared to other solutions to obtain such data. For example, given that a power source must be nearby (presumably within a few 10's of meters) why couldn't instruments be installed indoors with inlet tubing to the outdoors? Long inlet tubings are routinely used on tall masts with an appropriate pump rate? Are there particular problems regarding lag time? Or memory effects? Or disturbance of the air stratification? Why couldn't an existing mast be used with multiple inlet heights and the use of a manifold? A nearby EC tower is mentioned in the paper.

These points are addressed in the revised Section 1 Introduction as well as in the newly added Section 5 Discussion.

The publication could be seen as premature given the preliminary and incomplete parts (cold trap, pivot arm and standard gas supply module). However, it may be that the authors had limited opportunities to test the device in this remote location.

With the new focus as a profiling system, we assert that this near-surface profiling technique is novel, and warrants writing about.

The paper is mainly an account of environmental measurements (T and P, spectral characteristics etc) of the analytical instrument when placed inside the housing under cold and windy conditions, rather than a more complete account of a test of actual isotopic measurements. Ultimately the most critical isotope test, i.e. a comparison of one or more constant gas supplies both in the field and the laboratory, could not be carried out due to the lack of a working standards gas supply module. Also, there seems to have been a missed opportunity to compare the real time isotope data with isotope data obtained from the cold trap sampling, why wasn't this done (or presented) here?

Sections 4.1.2 and 4.1.3 have been streamlined to better address the temperature and pressure stability of the measurement cavity, as well as the spectroscopic diagnostics. Section 4.2 now also better quantifies the measurement quality of the system. L.468 in Section 5 also now states “Currently, the integrated Cold trap is unsuitable to be used for calibrating CRDS measurements.”

Given the anticipated degradation of isotope ratio precision at low H2O mixing ratios (typical of polar regions), and the possible instrument drift due to the changing environmental conditions, it would be important to check standard gasses a regular interval (likely several times daily) in an actual measuring campaign, hence the need for a standard gas supply device. As mentioned, this has not been demonstrated in the current manuscript.

In Section 5 Discussion, we discuss the potential for the system to be used in longer deployments, in the context that a calibration unit is necessary for this.

A more thorough explanation of the stratified air column data obtained would benefit the paper as it would demonstrate the useful application of the enclosure and pivot arm. The need for an operator to use the pivot arm seems to risk disturbing the stratified air column, depending on wind direction (creating turbulence)

Figure 10 now shows isotope profiles from a period on 9 March. We also mention the benefit that an automated Profiling module would bring on L.511.

Specific comments:

P1 L12: I don’t think you can claim it would be satisfactory in all environments, e.g. in the tropics, overheating, condensation of humidity may be a different challenge, in environments with high day – night temperature contrast drift may be problematic

L.9: We now only mention Arctic environments.

P2 L41: What are the conventional approaches?

We have rephrased this part of the Introduction to make it clear that previous in-situ measurements rely on controlled environments.

P2 L49: But you have a nearby power source so why not house the instrument there?

The nearby power source is the EC mast, which does not have any adequate housing for the Picarro.

P2 L52: But long inlet lines (fluorinated plastics) are routinely used in tall towers

We now state the motivation behind using short inlet lines in the Introduction (L.35-38).

P3 L64: Is it pneumatic? Maybe just gas or airtight connectors?

The term “pneumatic” has been removed throughout the manuscript, being replaced by “gas” or “sample transmission”

P4 L85: Isn’t a lower flow rate preferable to increase precision in dry air? Especially since there’s a separate high-flow pump to deliver the air sample close to the inlet?

L.480-484 now put forward this potential.

P5 L106: So why wasn’t this done when there was no available standard gas module available?

This sentence has been removed, and we now discuss the suitability of the Cold Trap as a calibration method in Section 5 Discussion.

P6 L144: Does the presence of an operator disturb the stratification?

As mentioned above, we now also discuss the benefit that an automated Profiling module would bring on L.511, in terms of further limiting any potential disruption to the sampling site.

P7 L161: This would have been the most complete test of the system

In its absence, we maintain that the analyser operational/measurement diagnostics put forward provide adequate proof of operation, especially in conjunction with our uncertainty quantification put forward in Section 4.2.

P10 L211: Please state if these are liquid or vapour values

We specify that these are liquid standards delivered by the Picarro SDM system on L.218

P10 L220: This sentence is unclear – does it mean that measurements were within +/- 1 sigma?

Section 3.3 has been revised to better clarify the calibration procedure.

P10 L229: Not sure this is correct, there are numerous field applications documented on the Picarro web site.

This sentence has been rephrased to better emphasize the analyzer's optimal operating environment (L.236).

P11 L247: Don't think 'minutely' is a word (?)

Removed occurrences of "minutely" from the manuscript

P11 L259: Please specify where DAS is measured

L.263: We first use the Data Aquistion System (DAS) temperature (T_{DAS}) measured inside the analyzer housing as a proxy of the overall temperature and condition of the analyzer.

P16 L355: As mentioned above, wouldn't the normal low flow setup have been preferable?

L.480-484 now put forward this potential.

P22 L454: add 'for d18) and dD, respectively

Section 4.5 Example of a profiling operation has changed substantially, including this line.