REVIEWER 1

The paper addresses a significant logistical and scientific challenge in Antarctic climate research: the manual sampling of precipitation for stable water isotope analysis.
Traditional manual sampling presents logistic challenges and exposes samples to post-depositional effects (mainly sublimation and metamorphism) prior to collection, which can alter the snow pristine isotopic values. To overcome this, the authors present an autonomous method for the continuous and combined measurement of both water vapor and precipitation δD using a single laser spectrometer.
The aim is to provide high-temporal-resolution data to better understand the Antarctic atmospheric water cycle, evaluate general circulation models, and improve the interpretation of past climate signals in ice cores.

We thank the reviewer very much for this constructive review and for the relevant suggestions that helped improve the manuscript. The answers are inserted in red and citations from the manuscript are indicated in blue.

Specific comments

Line 21 (Abstract) I would not use the term "condensed water" here

1) Thank you for this remark, we changed the expression by: "capable of analysing very small amounts of water"

Line 30 (Abstract) Is it LMDZ6iso or LMDZ6-iso (with the hyphen) as in other parts of the manuscript? I think it is LMDZ6iso

2) Thank you, we changed all occurrences to LMDZ6iso

Line 52-53 change "Adding the water isotopes in the models permits to test and improve the representation of the atmospheric water cycle in the models"with "Adding water isotopes to the models allows for the testing and improvement of the representation of the atmospheric water cycle"

3) Thank you for the suggestion, we modified the text accordingly.

Line 78 Change "weird" with "anomalous"

4) This is corrected

Line 111-116 How do you differentiate between snowflakes and water vapor signal when using the "snowflake inlet"?

5) Thank you for this important question. The differentiation between measurements obtained from the "snowflake inlet" and the "water vapor inlet" is ensured through a dedicated log file that records the valve position continuously to identify precisely the timing of each inlet switch.

However, we would really like to clarify that it is not possible to distinguish between signals originating from snowflakes and those from water vapor when using the "snowflake inlet" alone. The measured signal in this configuration inherently includes both contributions whenever snowflakes enter the inlet. We clarified this in the text: "The second inlet ("snowflake inlet") is oriented upward with a small aperture (1/8") to allow snowflakes collection in addition to water vapor."

To isolate the snowflake contribution, we rely on an alternating measurement strategy between the water vapor inlet and the snowflake inlet. The "water vapor inlet" measurements are used to estimate the background vapor signal, which is then subtracted from the "snowflake inlet" measurements as described in section 2.5.

Figure 1 I see that the snow sampling tray is attached to a wood platform, which is higher than the tray itself, plus the sampler is very close to a blue shelter; don't you think this could heavily interfere with the snow precipitation sampling?
The wind vane, which I suppose turns in line with wind direction, is also placed close to a building which shields the wind (and snow) from that direction

6) Thank you for this important remark. We acknowledge that the snow sampling setup is not ideal and may be influenced by shielding effects from the surrounding buildings. The positioning of both the sampling tray and the wind sock was primarily chosen according to the dominant wind direction, see new Table 1 in section 2.1, to indeed limit the influence of the nearby buildings. For logistical considerations, in particular the need to ensure safe and practical access for over-wintering staff during harsh winter conditions, it was important to place the tray and the wind sock close to the buildings and access path. Moreover, the sampling tray is primarily designed to collect vertically falling snow, for which the influence of the nearby shelter is expected to be limited.
Yet, we recognize that shielding and some influence of local airflow disturbances cannot be fully excluded and represents a limitation of the current setup. We add this sentence in section 2.3: "The collection device location was constrained by logistical requirements (easy access during harsh winter conditions) but oriented toward the dominant wind direction, minimizing potential shielding by nearby buildings."

Line 181 The time of the plots seems to go between 15:30 and 16:30, more than between 15:00 and 18:30 UTC

7) Thank you, the text was modified accordingly

Line 186 Change "In the example shown on Figure 2" with "In the example shown IN Figure 2"

8) Thank you, this is corrected

Line 208 What is the "The δD of precipitation"? Does it refer to the snowflake sampling or to the manual precipitation sampling? It is not clear. You should also define δDp and δDcp

9) Thank you for the remark, we added a definition of δDp and δDcp in lines 212-213:
"The δD of precipitation (for both online sampled precipitation δD_p and manually collected precipitation δD_{cp}) is consistently enriched relative to vapor δD_v and exhibits large intra-event variability, with rapid changes reaching ~100 ‰ within ~3 hours, as observed in Figure 4b"
The definition is also recalled in Figure 4 caption.

Line 228 Change “(Affolter et al., 2014) showed that differences” with “Affolter et al. (2014) showed that differences”

10) The text has been changed accordingly in lines 170-171 and 228

Line 294-295 While it is true that precipitation on the East Antarctic plateau has limited amounts, the frequency is not so low and you have to consider that it is also hard to discriminate between real snowfall events, blowing snow, hoar frost and diamond dust. All the aforementioned events produces snowflakes which, in your automated system, might be misinterpreted as snowfall, if not supported by other types of observations

Line 304-307 I think you have also to consider the input of possible blowing snow inside the inlet without precipitation occurring. A larger diameter inlet could facilitate the snowfall sampling, but it will eventually collect also more wind-drifted snow

Line 309-311 Compared to the system used in this study, the proposed setup would likely be more susceptible to blowing snow contamination

11) Thank you for these important remarks. We agree that with the current instrumental setup it is not possible to unambiguously discriminate between different types of solid precipitation and near-surface ice particles (snowfall, blowing or drifting snow, etc.).
However, we believe that this new type of combined measurements provides valuable information, as each type of ice/snow particle is associated with distinct formation and transformation pathways that may leave specific signatures in the isotopic composition. These new measurements may help to better constrain and eventually disentangle these different contributions, even if they cannot be directly separated “physically” with the current setup. To better interpret these signals, additional observations are required. These include surface meteorological measurements (such as wind speed and direction), snow particle counters, snow imaging systems (to characterize particle shape and type), and remote sensing instruments (radars, lidars). This multi-instrumental approach is being developed within the framework of the AWACA (Atmospheric WAter Cycle over Antarctica) project.

We have added one sentence to explicitly state this at the end of section “2.2 Water isotopes instrumental set-up”:
It should be noted that the present set-up does not allow discrimination between different types of ice particles (e.g., snowfall, drifting or blowing snow). Additional observations are required to disentangle these contributions on a physical basis, such as based on wind speed and direction, snow particle counters, snow imaging systems (to characterize particle shape and type), and remote sensing instruments (radars, lidars).

Line 335-336 Change “does not reproduce well” with “does not accurately reproduce” and change “simulations often showing a too smoothed evolution” with “simulations often showing an overly smoothed evolution”

12) The text has been changed accordingly.

Chapter 4.2.2 Your claim is that the metric Δ(δD) can be interpreted as an indicator of the temperature or altitude difference between the surface and the level of snow formation, provided that the vertical isotopic gradient remains approximately constant.
Are you sure that the vertical gradient remains constant? Isn't it possible that precipitation occurs during temperature inversion and thus the vapor equilibrating with snowflakes is enriched in δD compared to the surface vapor you measured? Do you have vertical temperature profiles from the periods you studied?
Have you also considered that snowflakes might experience sublimation during their descent?

13) Thank you for this remark. We agree that the interpretation of Δ(δD) as a proxy for the temperature or altitude difference between the surface and the snow formation level relies on several, strong simplifying assumptions. We note that a similar metric has been used in previous papers for such retrievals (Lowenthal et al. 2011, 2016).
There is indeed no reason to expect the vertical isotopic gradient to remain constant under all atmospheric conditions, especially in the presence of temperature inversion during precipitation, which would complicate the temperature interpretation of Δ(δD). We also agree that sublimation during snowflake descent is another process that may affect the isotopic composition. This effect is not considered in our interpretation and is mentioned as a limitation at the end of section 4.2.2 (line 504 in the manuscript with track changes).
Our intention is not to provide a quantitative retrieval of formation height or temperature, but rather to propose a first interpretation of Δ(δD) variability for the selected event, which we think may reflect changes in cloud processes, under idealized conditions. We do not have systematic, high frequency vertical temperature profiles for the studied events. In future work, a complete analysis combining complementary observations and more in-depth model analysis would be necessary to further investigate the Δ(δD) metric (e.g., its sensitivity to variations in vertical temperature gradients). Section 4.2.2 has been revised to better clarify the assumptions, limitations, the interpretation of the results and the perspectives.

Line 337-338 Although LMDZ6iso seems to capture pretty well the isotopic composition of precipitation, it looks like it fails to reproduce its variability within the event, especially for the final part of precipitation events; how do you explain that?

14) Thank you for this remark. At this stage, we think that part of these model-data differences can be explained by the role of blowing/drifting snow contribution to the measured isotopic composition, which is not implemented in the model, and could be dominant when the precipitation amount is low. For case 2, during the last part of the event (starting on April 17th), we typically observed no precipitation samples in the sampling tray (vertical precipitation), and no signal is observed with the micro rain radar on the lower gate, whereas snow was still collected by the wind sock sampler. In addition, the ceilometer (CL31) measured this day an intense backscattering signal in the first meters (0-80 m) with no precipitation above. This could indicate a larger contribution from blowing snow/drifting snow which is not accounted in the model, and explain the mismatch observed in δD between the model and the observations on April 17th. Some elements of this discussion have been added in section 4.2.1:
"This discrepancy is particularly marked when surface snow was still collected despite no recorded vertical precipitation, suggesting a dominant contribution from blowing and/or drifting snow not represented in LMDZ6iso, which may explain the δD_p model-data mismatch. The overall model-data δD_p agreement could indicate that precipitation isotopic signals during the three events are mainly controlled by large-scale moisture transport and conditions during condensation at higher altitudes,

while surface vapor isotopic signals are dominated by boundary-layer processes that are challenging to represent at the model resolution."

Figure 8 The caption should be clearer: Δ(δD) should be better explained and the difference between Δ(δDp) or Δ(δDcp) should be described

Line 375 Change "at 01:00 UTC" with "between 00:00 and 05:00 UTC"

Line 376-377 You have to specify that this rise occurs on April 17^{th}

15) Thank you for these helpful suggestions. We modified Figure 8 caption, replaced "at 01:00 UTC" with "between 00:00 and 05:00 UTC", and specified that the increase occurs on 17 April. Δ(δD), Δ(δDp) or Δ(δDcp) have been detailed in the caption as well. In addition, parts of this paragraph have been rephrased to improve clarity and readability.

Line 378-379 You wrote "a first part occurring in April 15th before the cloud descent to ground level with low values of Δ(δD)": first, change "in April 15th with ON April 15th", then you say that the Δ(δD) values are low in this timeframe while, as confirmed in figure 8, and in line 381-382 you state that "In the first part of the event, from April 15th to before 03:00 UTC on April 16th, Δ(δD) shows much lower values, around ~-65 ‰ with a large variability. During this period, the MRR mostly (change "mostly with "mainly") shows high reflectivity values (10-20 dBZ) extending from the surface up to 3 km, with cloud tops exceeding the radar's observational range, indicating that snow particles likely originate from high altitudes."

However, when looking at figure 8, the cloud base height during this timeframe seems quite low (at least after 05:00 UTC on April 15th); how do you explain it? Do you think this precipitation formed in a cumulonimbus (although, to my knowledge, these clouds were never observed at Dumont D'Urville Station, with the exception of pyrocumulonimbus clouds), at a significantly higher height than the cloud base?

16) Thank you for pointing to this specific point. Our observations do not allow us to conclude that the precipitation originated from a cumulonimbus cloud, and addressing this question would go beyond our dataset and the scope of this study.
The main point of this paragraph is that in the first part of the event (before the vertical dashed line), based on the MRR observations, ice particles are detected throughout a layer extending from near the surface up to 3 km (maximal range), with high reflectivity values and positive fall velocities. This suggests that the snowflakes collected at the surface may have undergone growth over a large vertical extent, rather than forming exclusively near the cloud base. In other words, the cloud base height does not necessarily represent the primary region of particle formation or mass acquisition. The vertical development of the cloud could partly explain the relatively low values of Δ(δD), although this is a first-order interpretation, based on crude assumptions. The corresponding paragraph of section 4.2.2 has been rewritten to better explain this point, together with the changes detailed in answer 13).

Line 384-388 Please specify whether these two maxima in Δ(δD) are either Δ(δDp) or Δ(δDcp): looks like Δ(δDcp) to me. You should always specify when referring to either Δ(δDp) or Δ(δDcp), or to both of them

17) Thank you for this remark. Indeed, the local maxima are observed on Δ(δDcp), the text was modified accordingly. We also added this sentence (line 478-480, track changes version):
We also note that on April 17th after 07:00 UTC only $\Delta(\delta D_{cp})$ is observed (associated exclusively with wind sock samples, see Figure 4), while no snowflakes entered the inlet and no precipitation signal was detected by the MRR.
At some point in the paper you should talk about the snow accumulation from the three snowfall cases you studied and possibly compare the measured accumulation with the model output

18) This is indeed an important point, which we think would benefit from a dedicated and more detailed analysis in a separate study. In the present manuscript, our primary objective is to introduce the sampling methodology and to highlight open questions and perspectives, rather than to provide a full model/data comparison.
There is a snow gauge at DDU, specifically designed to reduce the impact of blowing snow, but it is not entirely immune to contamination (Wiener et al., 2025). As a result, the recorded accumulation cannot be directly interpreted as true snowfall, so there is a need to correct or filter the data, which is beyond the scope of this study. We nevertheless examined the snow gauge data for case studies 1 and 2 (the instrument was not operational during event 3) and performed a preliminary comparison with model outputs. This comparison remains difficult to interpret quantitatively at this stage and this is why we did not include these preliminary data in our manuscript.
One notable feature in case study 2 during the last part of the event (starting on April 17th) is that no precipitation is recorded by the snow gauge, which confirms at least qualitatively the absence of precipitation with the MRR and the absence of snow samples in the sampling tray (see answer 14 for more details).

Line 428-430 It is unclear whether this system can differentiate between drifting and falling snow. If it can, please detail the mechanism used to distinguish the two

19) Thank you for this remark. Indeed, the current sampling system does not allow us to directly distinguish between drifting snow and falling snow because both are collected indistinguishably at the inlet, a sentence has been added to state this at the end of section 2.2. We note that these two types of snow may be associated with different physical processes that could be reflected in their isotopic composition (see answer 11). In particular, drifting snow particles are likely to have interacted with near-surface vapor and may approach isotopic equilibrium with the local atmospheric conditions. In contrast, freshly falling snowflakes are expected to retain an isotopic signature more representative of their cloud origin, as they experience limited time for equilibration during descent. We modified the corresponding sentence as follows:
"This opens up new possibilities for evaluating the implementation of cloud processes and their impact on the isotopic composition of precipitation, for improving the representation of snowflake-atmosphere interactions (particularly during sublimation), and for constraining the contributions of falling, drifting and blowing snow, which arise from different transport and transformation pathways."

REVIEWER 2

In the manuscript titled "Combined and autonomous online measurement of water isotopes in precipitating snowflakes and atmospheric water vapor in East Antarctica" Thomas Lauwers et al. describe a new method to measure stable water isotopes of precipitation in coastal East Antarctica. Potentially this method could replace very labor intensive and unreliable manual sampling of precipitation while also improving time-resolution. With calibration and post-processing both water vapor and snow precipitation can be analysed quasi in-situ with one single instrument.

We thank Reviewer 2 very much for this constructive review and for the relevant questions that helped improve the manuscript. The answers are inserted in red and citations from the manuscript are indicated in blue.

General comments

This new instrumentation together with post-processing is a great addition for process understanding of the water cycle. In Antarctica precipitation is known to be difficult to sample and to avoid fractionation or mixing with older snow or hoar. It is clearly pointed out to be a prototype and event-based study. Still, it would be helpful to discuss some points more comprehensively, e.g. by answering the following questions throughout the discussion:

- Could the instrument replace a dedicated instrument to measure stable water vapor isotopes?

  Yes, we think that this method can be used to measure stable water vapor isotopes. We added this sentence in the first paragraph in section 4.1.2:

  "The present setup can also provide continuous water vapor isotope measurements, although interruptions occur during snowflake sampling (~15 min every hour). These gaps are not expected to affect long-term observations, including annual, seasonal, diurnal variability studies. At the event scale, abrupt variations reaching 0.5 ‰/h in water vapor $\delta^{18}O$ have been reported [Landais et al., 2024] and typically last a few hours, making the setup also suitable for capturing such variability."

- Could the instrument replace manual precipitation sampling?

  In the long term, this may become possible, but for now we consider the instrument as complementary to manual sampling rather than a full replacement. Autonomous sampling is particularly well suited for remote environments and low-precipitation conditions where manual sampling is difficult or not feasible, as already mentioned in the conclusion:

  "Compared to manual collection, the automated approach offers clear advantages: it requires no human intervention and is sensitive enough to detect very small precipitation volumes, making it well-suited for deployment in the low-precipitation environments of the Antarctic plateau."

  In stations with larger precipitation amounts, manual sampling remains interesting because it provides lower measurement uncertainty and allows the retrieval of second-order parameters such as d-excess. To clarify this point, we added the following sentence at the end of the first paragraph of the conclusion:

“At this stage, the method is complementary to manual sampling rather than a full replacement, especially in places with large precipitation amounts, where manual collection provides lower uncertainty and access to second-order parameters.”

- Is the instrument suitable for all kinds of precipitation (drifting snow, blowing snow, diamond dust, agglomerates)? What kind of “snowflakes” are expected to be able to be sucked into the sampling line with the status quo?

  In principle, the instrument can sample all types of “snowflakes”, including drifting snow, blowing snow, diamond dust, and agglomerates. However, the main limitation arises from the inlet geometry and wind direction, as discussed in section 4.1.2 (second paragraph). In the current configuration, large agglomerates (> ~3 mm) may be blocked at the entrance of the sampling line or may partially fragment at the inlet, which could introduce a sampling bias. The second paragraph of section 4.1.2 has been modified to include these details.

- What would be your recommendations for the optimal instrumentation?

  The optimal instrumentation setup is summarized in the last paragraph of section 4.1.2. It would include i) an adjustable and orientable inlet to maximize snowflake collection efficiency, ii) a water vapor analyzer adapted to the range of humidity measured both on snowflakes and vapor at Dumont d’Urville station, and iii) a dual-analyzer configuration to enable continuous, high-frequency measurements of snowflakes without interruptions associated with vapor measurements.

To me, the measurement set-up is missing the option of a standard injection unit to calibrate precipitation measurements. At this point the comparison to manually sampled precipitation looks rather convincing, but I think a future version of the instrument would benefit a lot on the opportunity to independently calibrate the precipitation measurements. A syringe injection unit, like Affolter et al. (2014, https://doi.org/10.5194/cp-10-1291-2014) used, could help to optimise post-processing and design of the inlet line. Did you do any experiments to characterise sublimation in the snowflake line or liquid evaporation to optimise post processing?

Thank you for this relevant suggestion. We agree that the implementation of a standard injection unit to mimic a snowflake sublimation would be valuable for the system optimisation, for calibrating precipitation measurements and to better quantify instrumental uncertainties.

At this stage, however, implementing such a setup in Antarctica would be technically challenging and likely require regular human intervention, reducing the advantage of a fully autonomous measurement system, which is the aim in this study.

We already performed preliminary syringe injection tests in the laboratory to validate the concept before deploying the prototype in the field, but with a different setup which is not representative of the prototype running in Antarctica (different sampling volume, heating temperature, material, etc). More robust laboratory experiments with a realistic setup would be highly valuable to help disentangle instrumental noise from natural variability discussed in section 3.2.1. We have added the following sentence at the end of section 4.1.2 in the manuscript: “In addition, controlled experiments using standard injection systems would be valuable to better characterize the setup, in particular to disentangle instrumental noise from natural variability (section 3.2.1) and to assess the influence of external parameters (e.g. snowflake water content and isotopic composition, background humidity).”

In section 2 Methodology, I would prefer a subsection with a site description, where the climatic conditions (temperature, wind direction and speed, typical conditions for precipitation, drifting snow, blowing snow) the sampling sites (also in relation to prevailing wind direction) and also the remote sensing instruments can be described. It could be restructured a bit to make clear which instruments have been there before and what and when instruments were newly used for this study.

Thank you for this helpful suggestion. In response, we have added a subsection (Section 2.1) providing general information about the site, including climatic conditions, as well as a brief description of the meteorological and remote sensing instrumentation. We also clarified the positioning of the ATMOS building with respect to the prevailing wind. In addition, the first paragraph of Section 2.2 has been revised accordingly to avoid repetition regarding the isotopic measurements.

Specific comments

Line 35: Be careful with the word "first", I would prefer "early".

Correction implemented.

Line 36: Again "first". Do you mean precipitation was measured before vapor? The title of Dansgaard, 1953, Tellus V is "The Abundance of O18 in Atmospheric Water and Water Vapour", so cryotrap sampling was also used early on.

Thank you for the clarification. We have removed "first" to avoid implying a chronological sequence, as early studies (e.g., Dansgaard, 1953) already included both precipitation and atmospheric water vapour. And in the following paragraph, we specified that laser spectroscopy made possible the "continuous" measurement of water vapor isotopic composition.

Line 78: Please replace "weird" with a more precise expression, maybe "extreme".

"weird" has been replaced by "anomalous".

Line 85: DDU abbreviation, please introduce every abbreviation first.

Correction done.

Line 91: See General comments. Please make a section with site description and introduce all abbreviations (e.g. also ATMOS). Add some details on the meteorology of the site and describe the remote sensing instruments.

Thank you for this remark, this has been included in the new section 2.1.

Line 92: Which Picarro model is used?

It is a L2130-i Picarro. Text was modified accordingly.

Line 94: See above, what is the ATMOS building?

This is now detailed in section 2.1.

Line 96: I think it is low-humidity level generator.

Thank you, this has been corrected.

Line 99: When was the new sampling line installed?

The new sampling line was also installed during the summer season 22-23, this has been added in the text.

Figure 1/Right: All valve symbols except valve 6 have to be rotated by 90 °.

There are arrows drawn where the water vapor and snowflake inlet lines meet the sample lines of the analysers. I do not understand the meaning and would expect a simple T-connection.

Thank you for your remarks, Figure 1 has been modified as follows:

- The two arrows were removed
- A detailed view of the snowflake inlet was added
- “Snowflakes inlet” was replaced by “snowflake inlet”
- The valves were rotated by 90°
- Valve V0 was removed, as it did not provide any additional information
- Ports A, B, and C were added to V6

Are there any open splits upstream of the analysers?

There is no “open splits” upstream of the analysers, only the two flushing pumps are situated upstream.

Line 109: What kind of filter (material and size)?

This is a sintered filter made from stainless steel with a 16 mm diameter, this has been added in text.

Line 112: What is meant with aperture? Is it just the end of the 1/8” tube? Is there a detail picture of the snowflake inlet available?

The aperture is a 1/8” hole made on a stainless steel Swagelok cap mounted on the 1/4” tube, see in the modified figure 1.

Line 112: What is meant with “to be pumped”?

I removed this expression with this new sentence: “The second inlet (“snowflake inlet”) is oriented upward with a small aperture (1/8") to allow snowflakes collection.”

Table 1: V6 is illustrated as a three-way valve, so it is not clear what open/closed means.

We changed open/close by the position with the three ports A, B and C.

Line 125: What is the height of the sampling devices? Does the wind sock also collect drifting snow? Where is the sampling place in relation to wind obstacles?

The sampling systems (tray and wind sock) are installed at an approximate height of 1 m above the surface, this is now added to the legend of Figure 2. The wind sock may indeed collect drifting snow, but this contribution has not been specifically quantified in this study. We have added the following sentence in Section 2.3 to clarify the sampling conditions:

“The collection device location was constrained by logistical requirements (easy winter access) but oriented toward the dominant wind direction (south-east), minimizing potential shielding by nearby building.”

Line 132: What were the criteria of event selection?

For this first study aimed at testing the performance of the sampling method, we did not propose a particular protocol for event selection. A new sentence in section 2.3 has been added:

“The selection of events was based on practical criteria (availability of the overwintering staff) and meteorological conditions. The overwintering staff ensured that continuous manual sampling could be performed without interruption when forecasts indicated precipitation lasting more than 24 hours, in order to obtain a few long and high-resolution datasets. Outside of these events, manual sampling was performed only during standard working hours.”

Line 149: This would mean that the Picarro would be a more suitable instrument to measure both stable water isotopes, at least for sufficiently high vapor backgrounds – later you mention that. Did you try the snowflake inlet with the Picarro at some point?

Thank you for this remark. We have indeed recently tested the snowflake inlet with the Picarro L2130-i (in 2025), following the validation of the method with the AP2E instrument presented in this manuscript. This new dataset will allow us to evaluate the performance of the Picarro under high-humidity conditions and to investigate second-order parameters (d-excess). However, the data processing and comparison with individually collected snow samples are still ongoing and require additional time (snow samples are not back yet in Europe).

Line 176: In this set-up you can get the background humidity and isotopes from the Picarro also, or not? Does it change the results?

This is a very interesting question, and we provide here clarifications based on tests we conducted. We indeed evaluated a two-instrument configuration, in which the background humidity and isotopic composition are measured with the Picarro L2130-i during the snowflake sampling phase, instead of being estimated from the average of the last 5 minutes of the preceding vapor measurement segment, as done in the single-instrument approach presented in the manuscript.

This configuration did not lead to improved accuracy compared to the single-instrument method. For this reason, and in order to keep a concise, methodology paper, we chose not to include these intermediate results in the main text. We consider that the most robust evaluation criterion remains the comparison with independently collected snow samples.

The table below summarizes the accuracy obtained for the three case studies using both configurations. Note that the overall mean accuracy (deviation from the collected sample) reported in the manuscript using the single instrument configuration (−5.4 ‰) does not correspond to the average of the three cases, as each event contains a different number of data points and therefore contributes with a different weight.

	Single-instrument accuracy (‰)	Two-instrument accuracy (‰)
Case 1	-3.0	6.5
Case 2	-6.1	5.8
Case 3	-8.1	-5.1

No clear improvement is observed, and the accuracy remains of the same order of magnitude for both approaches. Although measuring the background humidity and isotopic composition simultaneously during the snowflake sampling phase may seem more appropriate, the two-instrument configuration may also introduce additional sources of uncertainty, in particular related to intercalibration between instruments, since the isotopic measurements are no longer performed by a single analyser.

Line 180: That is why I think the set-up could benefit from the addition of a syringe standard injection unit or similar.

We agree that the implementation of a syringe-based standard injection would help to better assess the detection limits of the setup (ie when the water content of the snowflake is very low). We refer to our response above for a more detailed discussion of this point.

Line 183: Why the first 4 minutes? What are the criteria?

After each switch, we observe a peak in humidity (see Fig. 3), particularly visible between 17:30 and 18:00. The 4 minutes have been adjusted based on the three presented cases, where we observed that this duration was sufficient to filter out the peak.

We modified the manuscript with the following sentence: "After each switch, peaks in humidity and δD signal, attributed to measurement artefacts (e.g., pressure fluctuations after valve switching), are observed. To filter them, we remove the first 4 minutes following each switch and then calculate the isotopic composition of precipitation δD_p (computed from Eq. 1)."

Line 186: Could you estimate the water content which would equal a 10 % increase in humidity?

The water content can be estimated, but depends on the background humidity. For a background humidity of 1000 ppm, a 10% increase corresponds to a snowflake sublimation contribution of 100 ppm, measured over ~ 10 min. With an incoming air flux of 50 mL/min, a temperature of 45°C and pressure of 50 mbar, the corresponding liquid water content is of 0.1 µL. This value is interesting for a comparison with the literature and similar systems (e.g., Affolter et al., 2014). However, we chose not to include this estimate in the manuscript, as it does not represent the water content of an individual snowflake (which would have a clearer physical meaning), but rather the integrated contribution of several sublimated snowflakes during the 11 minutes measurement period of the "snowflake mode".

Line 187: How common are situations like the last segment of figure 3? What could it be? How would figure 3 look like if you use the Picarro measurements as "background", are there any differences?

The last segment of Figure 3 illustrates a typical situation where no snowflakes enter the sampling line. No significant humidity peaks are observed, apart from the two peaks associated with the switching between vapor and snowflake modes (these are instrumental artefacts, see previous response). The relatively flat humidity signal remains slightly above the background level and we attribute it to residual water vapor from previously sublimated snowflakes that has not yet been fully flushed by the pump. A sentence has been added at the end of Section 2.5 to clarify this point. This so-called "memory effect" is well known in isotope measurements and is also associated with a small offset in isotopic composition. When this residual humidity becomes small (<10% above background), the denominator in Eq. (1) approaches zero, which can lead to divergence in δD_p and increased uncertainty. For this reason, such data points are excluded from the analysis.

Using the Picarro L2130-i as a background reference confirms the slightly lower background humidity, but do not significantly improve the overall measurement uncertainty (see previous answer).

Line 197: Please add a few more details for the events (maybe in a table): wind speed and direction, precipitation type and duration. Why did you choose these events? For the discussion: Are there additional events, where you doubt the quality of the results and what could be the reasons? Or are there other events where there was no accompanying high resolution manual sampling?

Thank you for this remark. We have added a table in Section 2.1 including mean temperature, wind speed, wind direction, and humidity for summer/winter periods and for the three selected events. The event duration is also reported and corresponds to the period during which snow samples were collected (including precipitation/blowing/drifting snow).

The precipitation type is not straightforward to determine, as there is no unambiguous observational criterion to distinguish snowfall from blowing or drifting snow. A combined analysis of remote sensing observations (micro rain radar, ceilometer), precipitation gauge data, and snowflake imaging could provide insights into the nature of the events, but this is beyond the scope of this study. Instead, we indicate in the table the sample type (tray, preferentially collecting vertically falling snow and wind sock, mainly collecting snow associated with a strong horizontal speed).

The selected events were chosen because they last more than 24 hours (detailed in section 2.3), allowing us to obtain long and high-resolution datasets adapted for quantitative comparison and for documenting intra-event isotopic variability of snowflakes in Antarctica. Over the period December 2022 to August 2023, we identified very few events with continuous manual sampling over more than 24 hours, and only the three presented events combine both uninterrupted manual collection and simultaneous instrumental online measurements. We also have additional events with instrumental online observations but without corresponding manual samples.

From this first dataset (December 2022 to August 2023), we did not identify any event showing strong disagreement, including shorter events. This study is intended as a proof of concept rather than a comprehensive statistical evaluation, but we agree that a long-term analysis (over one or two years), including also the shorter events would provide a more robust assessment of the method's performance, as well as the sampling representativity. We added this sentence at the end of section 4.1.2: "Future work should also include a longer-term statistical evaluation (e.g., over one year or more), including shorter events and periods with and without collected snow, to better quantify the representativity of the sampling system."

Line 208: Please introduce what subscript cp means.

Thank you, the text was modified accordingly.

Line 211 to 214: Move to discussion, but describe here the agreement/differences between the two sampling methods. Also, are there any obvious differences between the wind sock and tray samplings? Not everything you see in figure 4 is described. A definition of theoretical vapor is missing.

Thank you for this remark. When both wind sock and tray samples are available at the same time, no obvious differences in isotopic composition are observed for the three events shown in Figure 4. However, differences in collection efficiency are observed at the event scale, with some periods including only tray samples and others only wind sock samples. No periods were identified during which snowflakes were sampled exclusively by the autonomous online method. The number of samples collected by each sampling method is now reported in the new Table 1. The combination of the tray and wind sock helps maximize snow collection under varying wind conditions. The last paragraph of section 3.1 (previously line 211 to 214) has been revised to include these details.

The theoretical vapor at equilibrium is defined at the beginning of section 4.2.2 and is only mentioned in the caption of Figure 4. Section 3.1 is intended to focus on the description of the measurements obtained with the different snow sampling methods and water vapor isotope observations. Since the theoretical equilibrium vapor is only introduced for the interpretation of the $\Delta(\delta D)$ metric in section 4.2.2, we preferred not to include its definition earlier in order to keep section 3.1 concise and easier to follow.

Figure 4: It should be avoided to use red and green in the same figure. Also red and violet are hard to distinguish. Please also indicate in the figure description what is blue and what is dark blue.

Thank you for this remark, the figure and the description have been modified accordingly.

Line 230: Could you prove the kinetic effect in your sample line with some experiments? What kind of signal do you get outside of precipitation events, in calm conditions, during drifting snow, blowing snow events?

During precipitation events with low snowfall rates or drifting/blowing snow conditions with very few captured snowflakes, the observed δD signal has shapes similar to those described in Affolter et al. (2014) in some of the measurements presented at DDU. However, at this stage, we cannot demonstrate that these signals are exclusively caused by the kinetic effect associated with the sublimation of individual snowflakes, and we therefore slightly modified the sentence in line 230 (line 280 in track changes version). Indeed, under real field conditions, the signal shape may also result from mixing between several snowflakes or from variations in background humidity. Additional experiments using snow injections would therefore be necessary to robustly demonstrate and quantify this kinetic effect during sublimation, which could be an interesting topic for future work.

Lines 240 to 243 and figure 5: From the figure 5 it is not clear to me why an integration time of more than 4 min is required. The mean of the flake phase measurements seems still to be close to the collected samples, especially if you consider different sampling times as you mention in the next section? What would you expect the variability of snowflake water isotopes to be? How can you distinguish between measurement noise and "snowflake variability"?

The mean value is indeed close to the manually collected samples and suggests that, at first order, similar values are obtained either with one 11-minute integrated sample or when averaging eleven successive 1-minute samples. However, reducing the integration time to 1 minute while maintaining a 45 minutes vapor measurement phase (ie one 1-min sample every 45 minutes) leads to significantly lower precision and accuracy. During the particular event shown in figure 5, the variability observed between successive 1-minute samples within a single snowflake phase often exceeds the ~100 ‰ isotopic jump observed during the event. As a result, a 1 min snowflake / 45 min vapor configuration would make it difficult to distinguish natural variability from instrumental uncertainty and could potentially mask part of the signal variability.

We replaced "required" by "preferred" in lines 240 to 243, as we agree that there is no strict threshold and the choice of sampling duration results from a compromise between several constraints discussed in section 4.1.1 (capture efficiency, temporal resolution, measurement noise, memory effect).

At this stage, disentangling instrumental noise from the actual variability between snowflakes collected during the ~11-minute integration period is complicated. We believe that better constraining instrumental uncertainties through dedicated laboratory injection experiments could help distinguish measurement noise from true inter-snowflake variability (see response above concerning the standard injection unit). Although the behaviour of this very high-frequency variability is still an open question, one may expect for example larger variability during periods when snowflakes formed under very different conditions coexist, for example during mixed situations involving both precipitation and blowing/drifting snow.

Line 272: What are the conditions/the goal of the set-up for the discussion in section 4.1.1.? Do you assume you want to have one single instrument to get vapor and precipitation measurements at once and have no additional manual sampling of precipitation?

Indeed, our goal was to develop and validate a completely autonomous system with only one instrument, to be deployed in the frame of the AWACA project (https://awaca.ipsl.fr).

We added details to clarify this at the beginning of section 4.1.1.

Line 291: Advantage over what?

Advantage over manual sampling, changed in the text

Line 342 to 344: What would be the instrumental set-up for studies of snow formation, sublimation and surface interaction?

Lines 364 to 366: I do not understand.

We acknowledge that this sentence is unclear, and more generally, that section 4.2.2 objectives are not clearly defined. The aim of this section is to propose a first interpretation of the $\Delta(\delta D)$ variability observed during the selected event, which may reflect changes in cloud processes, under idealized conditions. This interpretation is supported by remote sensing observations documenting the vertical extent of the cloud layer. Section 4.2.2 (including previous lines 364 to 366) is currently being revised to better clarify the objectives, assumptions, limitations and the interpretation of the results.

Line 365: What is the CALVA program?

The mention of the CALVA program has been removed from this paragraph, as it did not provide relevant information in the manuscript. CALVA is a research program dedicated to the calibration and validation of atmospheric models through observations in Adélie Land and Dome C, including the remote sensing observations shown in Figure 8. The program is acknowledged in the Acknowledgments section.

Line 366: Include ceilometer and micro rain radar in methods and explain what these measurements mean.

Thank you, this has been included in the new section 2.1

Line 385: You should mention the inverted axis already in line 379.

Done

Line 429: How can the differentiation between drifting and blowing snow be done?

Thank you for this question. We first clarify that the current instrumental setup is not able to discriminate between different types of snow (snowfall, blowing or drifting snow). We have added one sentence to explicitly state this at the end of section "2.2 Water isotopes instrumental set-up": "It should be noted that the present set-up does not allow discrimination between different types of ice particles (e.g., snowfall, drifting or blowing snow)."

At this stage, it is not clear whether the differentiation between drifting and blowing snow can fully achieved, but we believe that this new type of combined isotopic measurement provides valuable information, as each type of ice/snow particle is associated with distinct formation and transformation pathways that may leave specific signatures in the isotopic composition.
We modified line 429 to better explain this and to include also blowing snow:
"This opens up new possibilities for evaluating the implementation of cloud processes and their impact on the isotopic composition of precipitation, for improving the representation of snowflake-atmosphere interactions (particularly during sublimation), and for helping to constrain the contributions of falling, drifting and blowing snow, which arise from different transport and transformation pathways."

Technical corrections:

Figure 1: Snowflake inlet instead of Snowflakes inlet

Line 124: Snow sample collection and measurement instead of Snow samples collection and measurement

Done

Line 143: Remove "situated"

Done

Table 2: Dimensions of a1 and b1, the dot should be a multiplication symbol

Multiplication symbol added

Line 288: hours instead of hour

Done

Line 378: on April 15th instead of in April 15th

Done