Reply to comments by Anonymous Referee #1

The authors appreciate for your constructive comments and suggestions. We will revise the manuscript by taking your comments into account.

Below are the *reviewer #1 comments* in blue and our response in black.

Summary:

The manuscript by Sugidachi et al. describes a new Peltier cooled frost-point hygrometer to measure stratospheric and tropospheric water vapor. This development expands to ability to measure stratospheric water vapor, which is desperately needed.

The manuscript describes some instrumental details as well as some comparisons and uncertainty estimates. It is heavily based on previous work on cryogenically cooled frost-point hygrometers and adopts many of the same ideas and principles.

The descriptions are often a little superficial and skip on several details. There appear to be many more profiles than the authors show here, which would allow them to do a statistical comparison between Skydew and CFH or Skydew and MLS. This seems to be missing and would add significantly to support the impact that this instrument may have. As is, the selection of profiles appears a little selective.

Below, I outline several points for improvement of the manuscript. I assume that these will take some time to implement. For these reasons I am hesitant to recommend publication of the manuscript without some major revisions.

Please find below our responses to your comments. In most cases, we agree with your suggestions and will revise the manuscript accordingly.

Detailed comments:

In the description of the sensor the authors state that a temperature difference of 90°C can be achieved. Here they should clarify that this is the temperature difference between hot and cold side, not between ambient temperature and cold side. Later, they state, that the achievable temperature difference between ambient temperature and cold side can be > 55°C at the surface and 30°C at an ambient temperature of -70°C. In the summary they state a temperature difference of 40 K in the stratosphere. Figure 2 clarifies that these values only apply at a wind *speed of 0 m/s. Adding wind of 5 m/s, a realistic temperature difference may probably by around 20°C smaller. Figure 12 seems to indicate a cooling limit of 25 K. In line 365, the authors state that an extreme limit at which Skydew cannot take measurements would be dew-point depressions larger than 60°C at the surface. Considering the statements above, that limit seems to be more than optimistic under real world flight conditions. It would be good to expand that discussion and consolidate the different statements throughout the manuscript.*

The maximum cooling capacity of the Peltier element used for SKYDEW is a temperature difference ΔT of 90°C as described in the datasheet. But this value is measured under ideal conditions, namely in a vacuum and at 27°C. We acknowledge that this sentence may cause confusion, so we will delete the sentences at lines 136 and 137.

The left panel of Figure 2 displays the temperature difference at a wind speed of 2 m/s, not 0 m/s. Indeed, the ΔT of >55 K mentioned at line 226 was measured at a wind speed of 2 m/s, not 5 m/s which is the standard ascent rate. We lack the experimental facility to generate a wind speed of 5 m/s under low temperature and low pressure conditions. Therefore, we estimated the cooling limit using theoretical Equation (7). We will explicitly describe this fact to reflect the temperature difference ΔT at an ascent rate of 5 m/s throughout the manuscript. (The estimated ΔT is approximately 45 K at 20°C and 1000 hPa, and ΔT is 30 K at -70°C and 100 hPa in the lower stratosphere under 5 m/s ascent rate.)

We will add estimated values using Equations (7) and (8) as dashed lines for each condition in Figure 2. Furthermore, the values for the lower stratosphere (-70°C, 100hPa, and 5m/s ascent) will be represented as a grey line. In the original manuscript, the dashed line in Figure 2 was simply a fitting curve for each condition.

Figure 12 shows a sounding during the daytime. Therefore, because the hot side of the Peltier element is heated by solar radiation, the cooling limit is lower than the estimated values. We will provide more detailed information on this below.

We will remove the description "(dewpoint depression > 60 K at the surface)" at line 365. In this section, we explain the quality control, and the temperature difference is not used in the actual QC procedure. We use only the Peltier current and scattered light to detect the cooling limit.

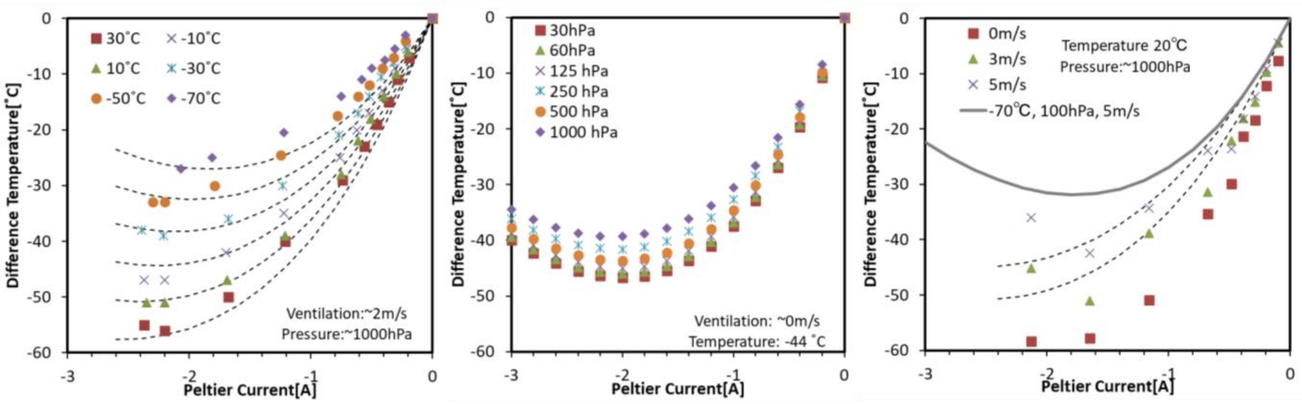

Figure R1-1. The revised figure 2 about the cooling performance. The dashed lines indicate the theoretical values calculated by Eq. (7), using the parameter $\alpha = 0.0099, \beta = 0.0432$, $R_e = 0.48$, $and\ S = 0.00025$. See the detailed information shown within each panel for different symbols, colors, and lines.

Related to this, the upper altitude at which the Skydew sensor can measure is not well supported. The authors point out that there is a systematic difference between day and nighttime measurements. Is the 25 km limit (not considering contamination) for nighttime achievable for all geographic locations? Is this limit a maximum under optimal conditions, or an average? Is this an optimistic estimate?

The limit of 25 km at nighttime is deemed achievable in many locations, including tropical and mid-latitude regions. However, the performance in polar regions remains uncertain due to the limited soundings conducted with SKYDEW in these areas.
The estimated cooling limits are plotted on the actual sounding results in the Figure R1-2. The top panel shows the nighttime sounding at mid-latitude. The estimated maximum cooling, represented by the grey line in the left panel, is calculated using the actual hot side temperature and the temperature difference estimated by Equation (7). For this example, the estimated maximum cooling is consistently lower than the dewpoint depression below 25km, which is within the cooling limit. On the other hand, the mirror temperature (frost point) reaches the estimated cooling limit at 25 km. This suggests that the cooling limit for this sounding is around 25 km. However, the mirror temperature is lower than the estimated cooling limit above 25km. As a result, this estimation might be conservative, and there is a possibility that the SKYDEW could cool slightly more in reality.
The bottom panel in Figure R1-2 shows the nighttime sounding for the tropics. The estimated ΔT at the tropopause is less than 30 K due to the low temperature. However, since the dewpoint depression is typically small at this altitude, SKYDEW does not face the cooling limit even above the tropopause. At higher altitudes, the dewpoint depression is larger, and SKYDEW

needs to create a larger temperature difference to maintain the mirror temperature at the frost point. The maximum cooling is also larger due to the low air density, which results in less heat transfer. As a result, the cooling limit is located near 30 km for this sounding. During the daytime, the hot side is heated by solar radiation, especially in the stratosphere. This leads to a lower cooling limit.

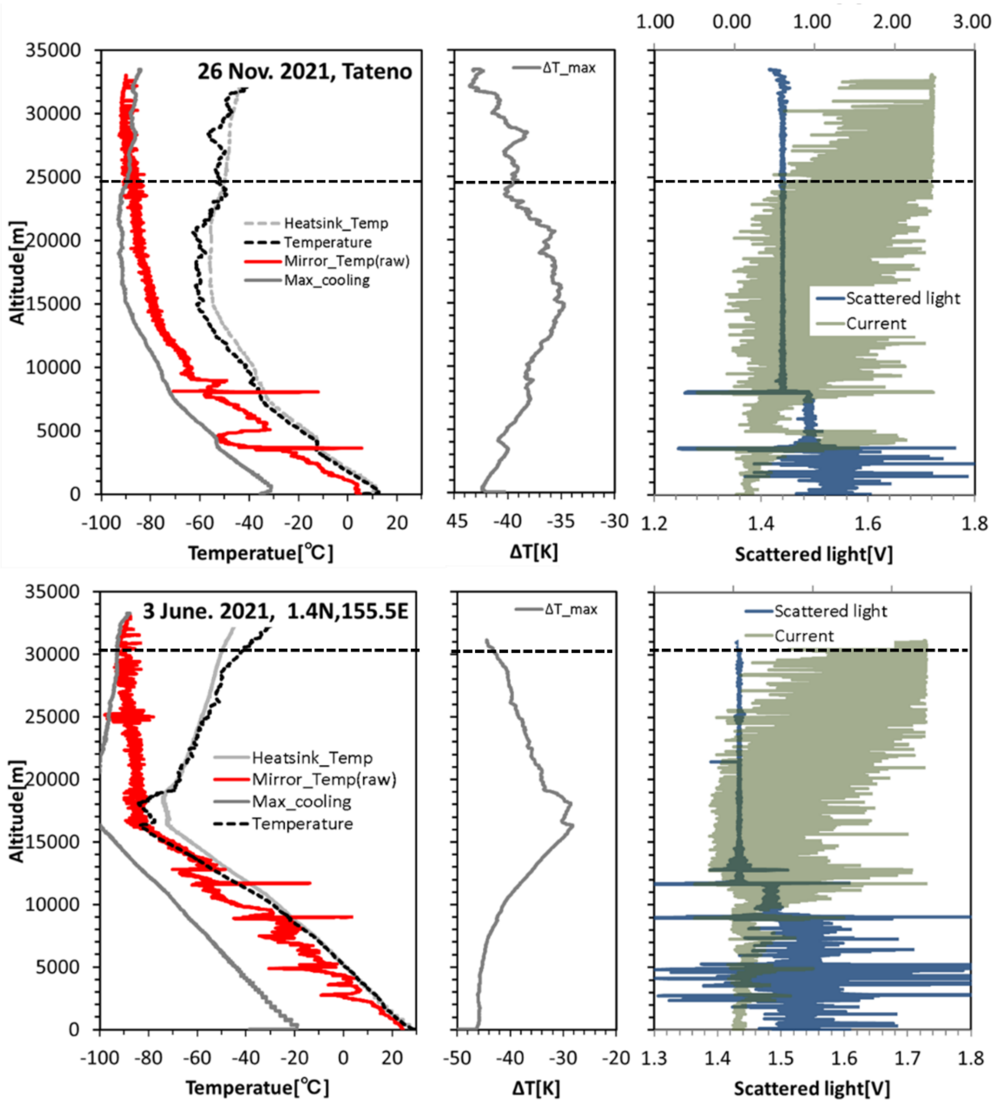

Figure R1-2. Vertical profiles of the comparison of observed temperature differences and the cooling limit estimated by Equation (7) for nighttime measurements. The top panel shows the profiles on 26 November 2021 at Tateno, Japan (36.06° N, 140.13° E). The bottom panel shows the profiles on 3 June 2021 on board the research vessel Mirai over the Northwestern Pacific Ocean (1.4° N, 155.5° E)

In the abstract, the authors report a difference of 0.5 K between the CFH and Skydew. This does not properly summarize the results in the text and gives a wrong impression of the comparison between the two instruments. This difference was found in only one of two soundings they discuss. There are more comparisons between the two instruments, which are not discussed in *the text.*

There are over 40 soundings using Skydew with different development stages. The descriptions in this paper, I assume, refer mostly to the current two-stage Peltier cooled version. How many soundings have there been using that version compared to previous ethanol cooled versions? How many comparisons with CFH or other reference quality instruments are there? Can the authors provide statistically significant results? How robust is the instrument performance across many soundings, considering that these instruments are inherently disposable?

So far, we have compared the final production version of SKYDEW with the CFH over ten times (Sugidachi 2019). The profiles from the five intercomparison soundings, excluding soundings with some doubtful data either of the two instruments, indicate that the SKYDEW readings are slightly smaller (<~0.5K) compared to the CFH in the lower stratosphere. However, these data have not yet been analyzed using the golden point method. We plan to re-analyze these data using a new algorithm, modified in response to the reviewer's comments, and will present the results in the revised manuscript.
In terms of the comparison between the previous ethanol-cooled version and the final version, we have not conducted a comparison between these two, though we do not expect essential differences because the only differences are the heatsink shape and the use of ethanol.
More than ten dual soundings using two SKYDEW instruments have been performed at Lindenberg since 2023. However, as these results will be reported in another paper, we will not include these dual sounding results in this paper.

References:
Sugidachi, T. : Meisei SKYDEW instrument: analysis of results from Lindenberg campaign, in 11th GRUAN Implementation and Coordination Meeting, Singapore, 23 May 2019 https://www.gruan.org/gruan/editor/documents/meetings/icm-11/pres/pres_0803_Sugidachi_SKYDEW.pdf (last access: 2 June 2024)

Line 91: The data quality of the cryogenically cooled instruments has certainly changed over the last 4 decades. The authors could just delete that statement.
We will delete the sentence of line 91.

The calibration uncertainty will be the limiting uncertainty for long term trends. The calibration uncertainty is quite reasonable, but can the authors show how they verify this uncertainty in long term production.

Each PT100 sensor of SKYDEW is calibrated against an SI-traceable reference. We will add a

description about the SI-traceability in the manuscript.
"Each PT100 is calibrated at four temperatures (40°C, 0°C, –45°C, and –95°C) against an SI-traceable reference to characterize the individual Resistance-Temperature (R–T) relationship of the PT100."

Does the instrument measure the temperature of the warm side of the Peltier device to verify that the sensor performs within the expected range? If so, if would be very useful to show these results.

Yes, we consistently monitor the temperature of the hot side of the Peltier element. We will add this temperature profile to Figures 9, 12 and 13 in the revised manuscript.

Equation 7 is not obvious. If the heat conductivity across the device is considered small and the heat input into the cold side is small, then the temperature delta across the device becomes proportional to the resistive heating of the device. That does not appear to make sense for a Peltier device. The source for equation 7 is cited as Sugidachi (2014). The source for the same equation in that thesis is cited as Sugidachi (2011). The link in that reference no longer works and this thesis cannot be easily accessed. Thus, it is difficult to trace the reasoning for this equation. (On that note, in other places as well, the authors do not cite the original literature, but rather papers citing earlier publications. It would be better if they directly cited the earlier or original work.)

The explanation in the original manuscript was indeed insufficient. We will add the description of the processes to derive Equation (7) in the revised manuscript as below:
The heat balance per unit time, which includes the heat transfer between the ambient air, is written as follows:

$$m\,c\,\frac{dT_m}{dt} = \alpha\,T_m\,I + \frac{R_e I^2}{2} + \beta(T_h - T_m) + HS(T_{\mathrm{air}} - T_m) \tag{1}$$

where T_m is the temperature of the mirror surface at time t , m is the mass of the mirror and the Peltier element, c is their specific heat, R_e [ohms] is the resistance of the Peltier device, I [A] is the current through the Peltier device, α [V K–1] is the coefficient derived from the material and the size of the Peltier device, T_{air} is air temperature, β [W K^{-1}] is the coefficient associated with the thermal conductivity of the Peltier device, H [W m^{-2} K^{-1}] is the heat transfer coefficient between the mirror surface and air temperature, and H [m2] is the mirror surface area. Assuming that the heat sink quickly equalizes with the surrounding air and can always be approximated as T_h=T_a, T_m can be expressed as follows:

$$T_m = \Delta T\left\{1 - exp\left(-\frac{-\alpha I + \beta + HS}{m\,c}\right)t\right\} + T_a,$$

$$\Delta T = \frac{\frac{RI^2}{2} + \alpha I T_a}{-\alpha I + \beta + HS}$$

(2)

In this equation, at t=0, T_m=T_a, indicates that the mirror temperature is equal to the ambient temperature. Over time, the mirror's temperature decreases exponentially, implying that it creates the maximum temperature difference ΔT after sufficient time has passed. The sign of the Peltier current is by (our) definition negative in cooling mode and positive in heating mode. The reference, Sugidachi (2011), has now been made publicly accessible. We will add the URL to the reference list in the manuscript. This will make it easier for readers to access the source.

Sugidachi, T.: Development of a balloon-borne hygrometer for climate monitoring, master's thesis at Graduate school of Environmental Science, Hokkaido University, 138 pp. (*in Japanese*), https://eprints.lib.hokudai.ac.jp/dspace/bitstream/2115/92715/3/takuji_sugidachi_2011.pdf (last access: 10 July 2024), 2011

Section 2.6.1: During the time of the phase transition, the instrument is most likely controlling not around the dew-point or frost-point, but rather around the change in reflectivity between the two phases. During this period, the dew-point or frost-point is not defined by the mirror temperature. However, the control around the dew-point seems to stable until about 1400 s, i.e., super cooled water persists for that long (not 500 s). A region of ice crystals seems to grow in the upper right corner of the mirror. Does the detector even see these very large crystals? Do the authors have any information about the temperature gradients across the mirror? Could the crystal growth in the corner be caused by a significant temperature gradient? (Same questions for the very large ice crystals at the edge of the mirror in Figure 5.)

Indeed, when both ice and water co-exist on the mirror, the mirror temperature is not strictly the dew point nor frost point. However, this experiment shows that the mirror temperature is close to the dew point as long as supercooled water remains on the mirror. We will add the phase transition timing on the profile of dew/frost point in the revised manuscript to clarify the additional uncertainty due to phase ambiguity near this altitude.

We have not measured the temperature gradients on the mirror. Instead, we have roughly estimated the temperature gradient in the thickness direction of the mirror using Fourier's law. The mirror material is silicon, which has high thermal conductivity (148 W/mK). Because the mirror is located on the Peltier element, the top side and bottom side of the mirror have a slight temperature difference due to the internal temperature gradient. For instance, if the cold side of the Peltier element is cooled down 30K compared to the ambient air, and assuming H =50, the thickness of the mirror L =0.28mm, the temperature difference is 0.015K. This potential error is included in the uncertainty budget.

The reason for the large ice growth on the edge of the mirror is considered to be because the mirror edge acts as ice condensation nuclei. The polished surface of silicon has little roughness, but the sharp edge is easily ice nucleated. The SKYDEW is sensitive to this large ice on the edge as well. This large ice is not preferable because it causes a slow response due to the slow evaporation/growth rate.

Lines 307 ff: There is one fundamental difference between cloud physics and the condensation processes on the mirror: The mirror itself can act as an ice-forming nuclei. Therefore, the temperature dependence is likely very different than that for the free atmosphere. The growth rates are possibly similar.

We will delete the Line 306-309.

Sections 3.2 and 4.2: The derivation of the golden points and their uncertainty is very unclear. As written, the authors first create 5 Hz data by linear interpolation of the original 1 Hz data, then electrical noise is removed, lastly the data are smoothed using a Gaussian filter with a width of 1 to 3 s depending on altitude. The authors should remove the electrical noise first. How large is the electrical noise and how can it be distinguished from that generated by the PID controller? Secondly, why do they create 5 Hz data, if they are smoothed anyway? The linear interpolation combined with the Gaussian smoothing may even generate a little bit of extra noise, although that may be small. In the uncertainty estimation they use the number of 5 Hz data points in their statistics and assume they are random and uncorrelated. They are clearly not random and uncorrelated since they were created by linear interpolation and smoothing. This needs to be corrected. How is the uncertainty of the mirror temperature in the golden point selection process quantified? Is it the uncertainty of ±1 s in the detection of the golden point and the spread of the mirror temperature over these three data points? Figure 8 does not clarify this question. In line 383 the authors state that the extracted points are smoothed with a Gaussian filter. However, the extracted points are unevenly spaced, and Gaussian smoothing would achieve odd results under these conditions.

Regarding the removal of electrical noise, we found an error in the parameter setting for the width of the Gaussian filter in the original manuscript. The profiles in Figure 7 were generated using a Gaussian filter with a width of 0.4 – 1.5 seconds. During the development phase, we adjusted the width to be between 0 – 1 second at the surface and about 1 – 3 seconds in the stratosphere. In the figure in the original manuscript, these parameters are mixed. We will correct this in the revised manuscript.

When there is water droplet on the mirror, the fluctuation of scattered light signal is strong enough to overcome the noise. Therefore, there is no problem with using a narrow-width Gaussian filter. On the other hand, the fluctuation of scattered light signal is weak when there is ice particle on the mirror and can cause detection errors for the golden point if smoothing is not applied. Figure R1-3 shows an example of golden point detection with and without a Gaussian filter. For the troposphere, it is difficult to clearly distinguish between noise and real signal. However, the signal-to-noise ratio is good. The effect of the Gaussian filter is negligible and it is considered to be almost not necessary. For the stratosphere, there are many detection errors of golden point because the maximum and minimum points generated by the noise are detected in cases without smoothing. The oscillation period caused by the PID controller is typically 10 - 20 seconds. To avoid deleting these oscillations, we applied a Gaussian filter with a width of less than 3 seconds.

Regarding the creation of 5Hz data by linear interpolation, we assume that the golden point may sometime exist between the 1Hz sampling intervals. Figure R1-4 shows an example of detection from 1Hz data and from 5Hz data. The asterisks indicate the golden point from the smoothed 1Hz sampling data. This detection has a small shift from the detection point from 5Hz data. However, this shift does not cause a large error in frost point determination, and this value is negligible compared to the estimated uncertainty derived from the golden point error. Therefore, we decided not to apply up sampling with linear interpolation. Figure R1-5 shows the case of 1Hz data for the original Figure 9. There does not seem to be a significant difference. Increasing the original sampling rate could improve golden point detection. Although this cannot be implemented immediately, we will consider implementing higher-temporal-resolution sampling for the next firmware version in the future.

We described that the uncertainty of this golden point detection is random. As you pointed out, this uncertainty is correlated vertically because of smoothing. But this uncertainty should be treated as uncorrelated among different sounding profiles. We will modify lines 419-420.

Regarding the uncertainty estimation of golden point detection, Figure 8 will be revised as shown in Figure R1-6. Grey shadow area indicates the area between the error of +/-1 second. The absolute values of +1 s and -1 s error are not equal. So, the larger values are used for the uncertainty estimation as follows:

$$u_{GPerror} = \frac{|Tm_{GP} - Tm_{GP+1}|}{\sqrt{3}}, \text{ for } |Tm_{GP} - Tm_{GP+1}| > |Tm_{GP} - Tm_{GP-1}|$$

$$u_{GPerror} = \frac{|Tm_{GP} - Tm_{GP-1}|}{\sqrt{3}}, \text{ for } |Tm_{GP} - Tm_{GP+1}| < |Tm_{GP} - Tm_{GP-1}| \quad (3)$$

Regarding the smoothing after the selection of golden point, we used the Gaussian filter on unevenly spaced data. This process will be modified in the revised manuscript. We will generate

1 Hz data with resampling before applying the Gaussian filter.

As described above, we will make significant modifications to the process and related figures. These modifications, including the revised algorithm, figures, and detailed explanations, will be included in the revised manuscript.

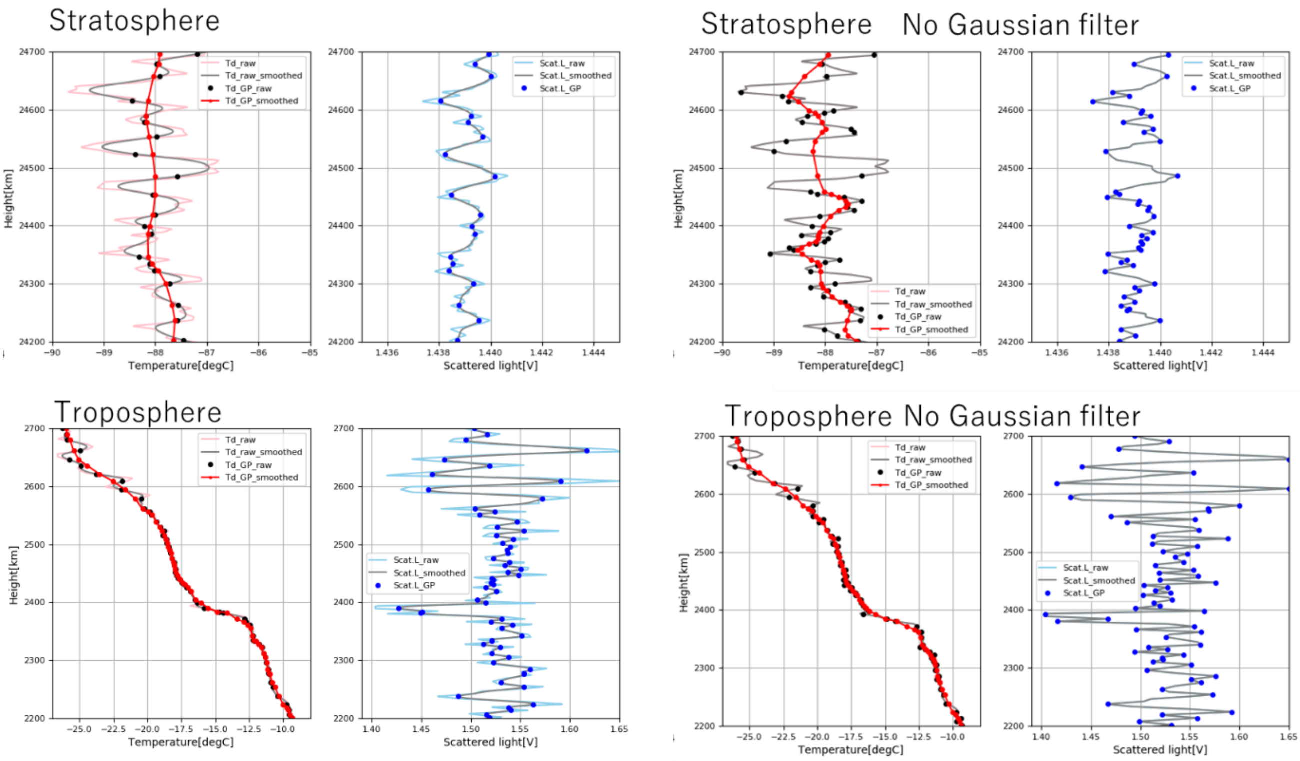

Figure R1-3 Example profiles of golden point detection with and without a Gaussian filter. The left panel displays the profile with a Gaussian filter, which is the same as Figure 7 in the original manuscript. The right panel shows the profiles without a Gaussian filter. The bottom panels represent the profile at the troposphere, while the top panels represent the profile at the stratosphere.

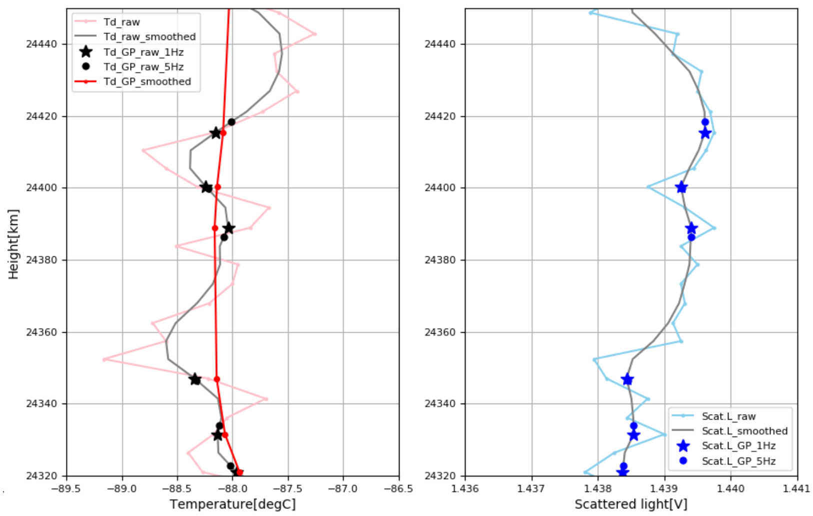

Figure R1-4. The difference because of the detection from 1 Hz and 5 Hz data. Left panel is the mirror temperature, and light red line is raw data, grey line is smoothed one, dot is the golden point detected from the 5Hz data, and asterisk is the golden point detected from the 1Hz data, right panel is the scattered light signal. Dot and asterisk is same as mirror temperature.

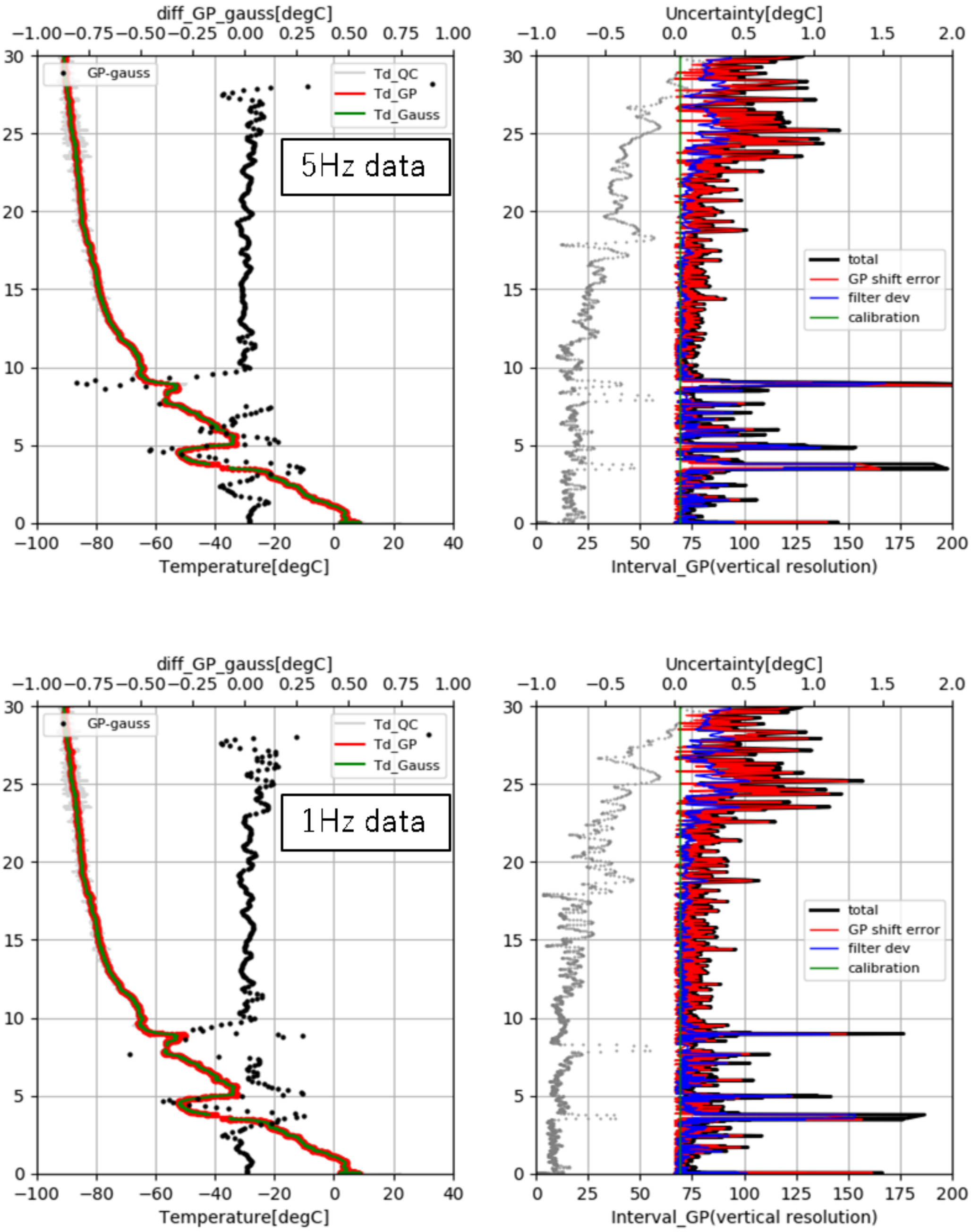

Figure R1-5. The profile of golden points detected from 1Hz data without up-sampling. The top panels are the same as panels (c) and (d) in Figure 9 of the original manuscript. The bottom panels display the same content, but the profile is derived from 1Hz data.

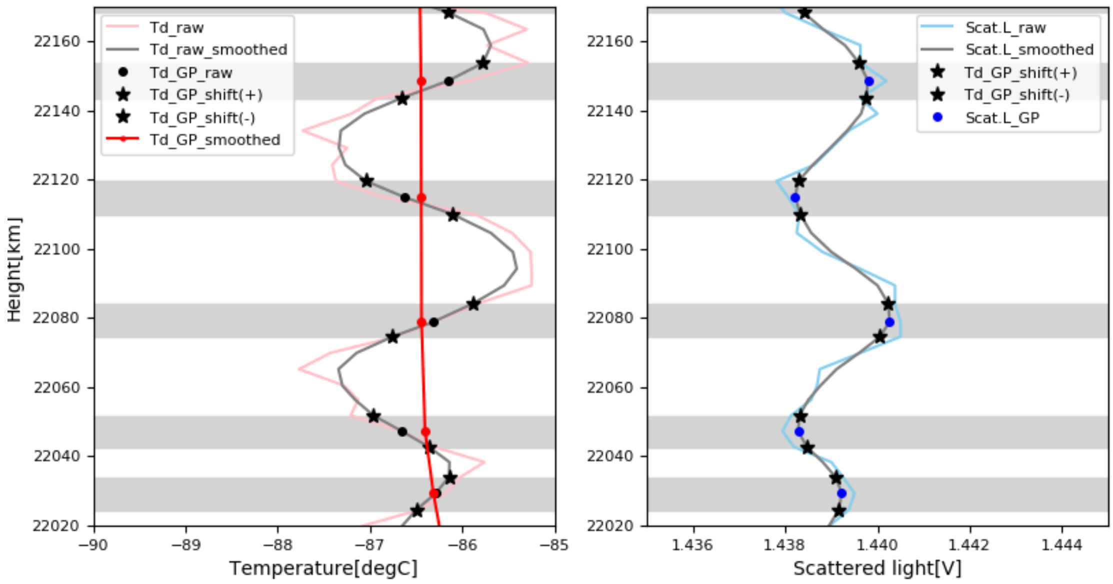

Figure R1-6. The profiles explaining the detection timing error of the golden point. The asterisks indicate the positions with a shift of +/- 1 second. The grey shadow represent the areas surrounding the error golden points.

Related to the derivation of the golden points and the smoothing, the authors should make a statement how that affects the vertical resolution of the observations and how the vertical resolution is given in the processed data.

We have described the topic of vertical resolution at lines 384-387 in the original manuscript. In the revised manuscript, we will replace the term "vertical interval" with "vertical resolution" for clarity.

The authors also state (line 548 ff) that the aggressive PID tuning and larger oscillations are required for the golden point detection. However, there is no such requirement in the derivation or golden points. The method should work with any amplitude of oscillation since it only requires a short period of constant reflectivity.

Yes. We will modify this sentence.
"Smaller oscillation is required for better measurement, although the oscillation with short periods due to the aggressive setting of the PID controller is needed to detect the golden point with high vertical resolution."

The authors find that direct gaussian smoothing of the data leads to slightly lower frost-point temperatures than that derived from the golden point method.

Section 5.2: There are more comparisons between the two instruments, which should be shown and discussed. Is the difference seen in the Lindenberg profile also seen in other comparisons? If so, this should be reported. If not, this should be reported as well. If any of the instruments in the other comparisons did not provide data in any of these additional comparisons, this could also be reported. As is, showing just two comparisons seems a little selective.

Yes. We agree with your suggestion and will include additional comparisons between the two instruments in the revised manuscript.

Section 5.4: Looking at the data referenced by the authors, the comparison with MLS in the tropopause region seems to look a little better than shown by the authors. The averaging kernel of MLS is broader than 1 km. The Skydew data should be smoothed using the MLS averaging kernel, or at least smoothed to the same vertical resolution. They may need to review how they do the comparison. However, as a result the disagreement on ascent starts at a lower altitude. It would be helpful, if the authors could create a relative difference plot with MLS as reference.

We will try to compare with MLS data by applying the MLS averaging kernels to the Skydew data. We will also show the difference plot in the revised manuscript.

One of the three "contaminated" profiles shows a significant jump in the upper troposphere, which is unlikely contamination. This may indicate additional complications not described by the authors.

If you are referring to the sounding on May 27, it's possible that cloud particles may have influenced the scattered light signal at 13 km. The oscillation in the scattered light signal at approximately 13 km corresponds to the near-saturated layer shown in the frost point profile. When SKYDEW passes through a cloud layer, such oscillation often occurs, especially in the upper troposphere, due to aggressive PID tuning. The sounding on June 1 was similar to that on June 3, with a thick cloud layer present in the upper troposphere.
We will clarify this point in the revised manuscript.

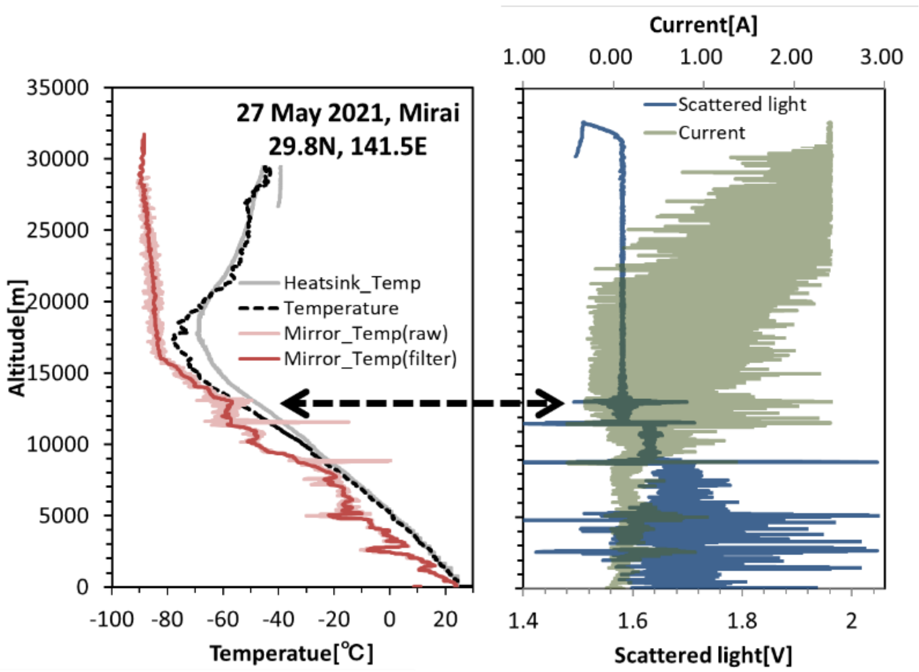

Figure R1-6. The sounding profile on 27 May. It appears that the SKYDEW detected something at 13 km. This could indeed be cloud particles.

Lines 628 ff: The sensor sits on top of Styrofoam box, which is very good for ascent measurements, but very bad for descent measurements. How would descent measurements be better for identifying contamination?

We will indeed remove the sentence: "SKYDEW descent data are also valuable for identifying contamination." from the manuscript.
The current version of SKYDEW, with the sensing part located on the top of the flight package, is designed for ascent soundings. However, our experience with other soundings indicates that descent data is hardly affected by contamination. It's plausible that almost water that is adsorbed on the surfaces of the payload evaporates in the dry conditions of the stratosphere. Once the water has desorbed, the payload's surfaces do not accumulate contaminants in the dry stratosphere; this only happens when the instrument descends into the troposphere. If the water vapor and ice are completely removed from the payload, the SKYDEW can measure air free from contamination in regard to water vapor measurements. However, the evaporation process at the stratosphere can take some time due to the cold temperatures at those altitudes. Enough time is necessary to evaporate the contamination completely.
We cannot confirm whether the contamination has been completely removed at the stratosphere. Therefore, the sentence at line 628 will be deleted. It is necessary to consider a configuration for descent measurement in the future.

Although the instrument is a frost-point hygrometer, could the authors please add mixing ratio *plots for the stratospheric sections of the profiles as they do in Figures 15 and 16? These panels could be added in almost all plots starting with Figure 7. In particular, the comparison plots in Figures 11-13 should show the mixing ratio and the relative mixing ratio difference for the stratospheric part of the profile (or a log plot for mixing ratio, which covers the entire altitude range).*

Yes, we will include the mixing ratio profile in Figures 11-13 in the revised manuscript. However, we will omit the mixing ratio in Figures 7-8, as these figures are primarily used to explain the golden point.

Data availability: Thanks to the authors for making some data available from the YMC-BSM campaign. The GRUAN data archive does not make frost-point observations available and is not a suitable archive for that purpose. It would be better if these data were publicly archived elsewhere.

We are currently in the process of developing the GRUAN data product for SKYDEW. However, since this development is not yet complete, these data have not been published on the GRUAN data server. Therefore, they are currently available only upon request. The soundings with RS41 include the XDATA packets in the RAW file. We plan to consider archiving the data used in this manuscript on an online data server.

Figures:

Figure 1: The actual sensor is very hard to see. The right-hand panel could be enlarged to make the sensor arrangement clearer, in particular the coupling of the Peltier element to the large radiator. It appears as if there is a cover over the actual sensor. This cover may accumulate contamination that could potentially be detected by the mirror due to its proximity. This should be discussed.

We will enlarge the right panel for better visibility. We will also add an explanation about the sensor cover in the manuscript. The revised manuscript will include the following sentence: "The sensor cover is made of aluminum and is painted black on the inside to prevent interference with the scattered light signal. The size of the cover is minimized to avoid the accumulation of water and ice."

Figure 2 should show the first two panels with a wind speed of 5 m/s (typical balloon ascent rate), rather than 2 m/s or 0 m/s. These parameters will give a much better representation of the *temperature gradients that may be achieved in a sounding.*

Due to limitations in our experimental facility, we don't have data for 5 m/s at low temperatures and low pressure. The data for 5 m/s is available only at room temperature. For the experimental results, we will add a theoretical curve at -70°C and 5m/s, as explained earlier.

Figure 4: The images of the condensate on the mirror do not come out well and could benefit from some contrast enhancements. Although the authors specify the air temperature at about 0°C, the mirror temperature is the relevant temperature (about -12°C). There is a red area underneath the enlargements, which also changes between the images. What does it represent?

Thank you for your suggestion. We will adjust the contrast and brightness of the images for better clarity.
We will also add information about the mirror temperature in the manuscript as follows: “Figure 4 shows the behavior of the phase transition at an air temperature of approximately 0°C and a mirror temperature of -12 to -13°C.”

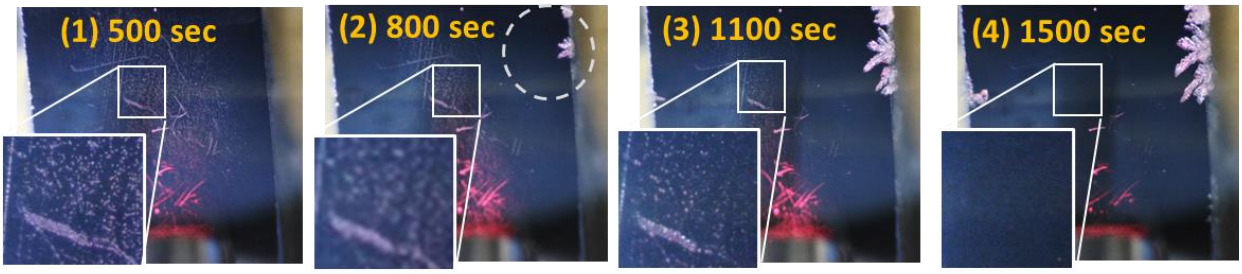

Figure R1-7. Adjusted the images in the Figure 4.

Figure 5: The caption lists the air temperature but should better give the mirror temperature at which these images were taken (they are listed in the legend of each panel). The text referring to this Figure should also list the mirror temperature, not the air temperature.

We will include the mirror temperature data along with the air temperature in the revised manuscript.

Technical comments:

We will correct as your suggestions in the revised manuscript.

Line 43: Replace “reduces” with “decreases”.

Line 49: change to "… employ capacitive RH sensors."

Line 85: replace "cooler" with "colder".

Line 136: Replace "at" with "a".

Line 235: Delete "that".

Line 545: Insert: … almost "always" …

Line 561: appear

Line 588: Figure 15 (not Figure 16)

Line 590: Replace "trail" by "train".

Line 615: Replace "over" with "of more".

Line 621: Remove the comma.

Line 624: "A dual sounding …"

Temperature differences are sometime expressed in °C and sometimes in K. Please use K for temperature differences throughout.

When referring to Figures, please use "Figure x" consistently and avoid using "Fig. x".