

Authors' response to the comments of the anonymous Reviewer #2 (RC2)

First, we would like to thank anonymous Reviewer #2 for the time and effort invested and the constructive feedback (<https://doi.org/10.5194/egusphere-2026-1696-RC2>).

To address your recommendations regarding the text passages and figures, we respond to them point by point below. Unless otherwise specified, the page and line numbers refer to the revised manuscript. The reviewer's comments begin with "RC2:", are reproduced verbatim, and are highlighted in gray, while our direct responses begin with "AC:". All text changes made to the revised manuscript in response to RC2 are highlighted in magenta.

General review

RC2: The paper describes an assessment of ceilometer cloud base height estimates provided by firmware with a calculation from extinction profiles stemming from a camera in the visible range and from the ceilometer backscatter profiles using different cloud base definitions. The topic is absolutely relevant and the main idea of the experiment makes sense. However the presentation of the manuscript and the derivation of the methods need very major reworking to make the manuscript publishable. The authors should not hesitate to re-write larger parts of the paper.

AC: Thanks again to Reviewer #2 both for the effort put into the very thorough review and for the extensive and valuable comments, which have greatly improved our revised manuscript. We hope that our last sentence in the "Acknowledgments" section now adequately conveys our gratitude to you.

Specific comments

RC2: Please make an assessment on the differences you expect due to the wavelength difference from using ceilometers in the infrared on the one hand and the blue channel of the camera on the other hand to infer your cloud bases according to the definitions. Also, you mention that PollyXT does not have a extinction profiles delivered at 1064 nm and is thus not suited for validation. However it seems to provide extinction at 532 nm what would be even closer to the ceilometer wavelengths. But the overlap might possibly be an issue for low clouds. Please comment on this and re-formulate your introduction.

AC: We have added a new paragraph to the introduction (**lines 113–134**) that deals with the elastic (Rayleigh and Mie) scattering properties in the different wavelength ranges used by ceilometers and by the camera's blue channel. We outline our expectations regarding the quality of CBH detection in optical thick water clouds and optically thin clouds. Furthermore, we provide at least a rough CBH difference between ceilometers and camera-based methods. We have supported our claims with numerous relevant sources, all of which are included in the bibliography.

As for the two Raman lidars, we have first added the relevant reference (Klett, 1981) regarding the introduction of the Klett inversion method (**line 87**) and corrected the wavelength at which RAMSES operates (**line 91**). We have deleted the sentence: "Taking into account Eq. (1) ... for a reasonable comparison." We have revised our explanation of why neither RAMSES nor Polly^{XT} is suitable for validating extinction profiles in the presence of low-level clouds. We now also discuss incomplete overlap and explain why it is less critical for estimating low CBHs with ceilometers (**lines 93–104**). For example, Appendix 4.1.27 of the international tender specifications for the CHM8k states that multiple scattering can allow signals from particles to be detected down to 0 m, despite the onset of overlap at approximately 80 m. Thus, incomplete overlap does not necessarily limit the

determination of low cloud bases. However, this is a confidential document that we can nevertheless make available to the reviewers for inspection upon request.

Just as a side note: Polly^{XT} provides σ -profiles at a wavelength of 532 nm; however, the differences relative to the ceilometer wavelengths in the 853–1064 nm range are no smaller than they would be for the unavailable σ -profiles at 1064 nm. In fact, the wavelength differences between the ceilometer and Polly^{XT} are 321 nm to 532 nm (for the Polly^{XT} wavelength of 532 nm) and -211 nm to 0 nm (for the Polly^{XT} wavelength of 1064 nm), respectively.

RC2: *Some sources of the (empirical) methods applied appear unclear, e.g. Eq. 3. I think it is important to have a sound reasoning for your method transmitted to the reader which is not the case here. In the end this is a key of your estimation of the extinction from camera, so we need to know what is going on here.*

AC: Admittedly, Wikipedia is not a sufficiently reliable scientific reference. Therefore, we are now providing the relevant citation (IEC 61966-2-1, 2003) for the reverse transfer function actually used to obtain linear RGB values. In addition, we now explain the characteristics of this international standard mapping function (**lines 170–181**).

RC2: *In addition the description of the method for the camera estimates and its parameters needs improvement to make it more understandable. Especially the method B for the calculation of the extinction profile appears questionable or at least hard to understand. I would encourage the authors to come up with a more comprehensive description and derivation of the method.*

AC: We have extensively revised the explanations for approaches A) and B) and hope that all variables and derivations are now clear. Our changes begin with the sentence: “The Michelson contrast C_n is determined for ...” on **line 201**. In addition, the explanations regarding Eq. (3) and Eq. (4) have been improved.

RC2: *Between section 3 and 4 you should logically also introduce the Klett inversion. Possibly the image analysis and the Klett inversion would be best suited as subsections of a new section 3 entitled something like "determination of the extinction profile". I see you have it in the appendix, but at least a small introduction and reference to it in the main text would be necessary if you don't want to move everything up. Also make sure that the CBH definitions given in the current section 4 can be applied to ceilometer extinction profiles and camera extinction profiles and give a more detailed description on the reasoning of them.*

AC: We have followed your suggestion and created a new Sect. 3 entitled "Determination of the extinction profile". It now contains the subsection 3.1 "Image analysis" (original Sect. 3) and the new subsection 3.2 "Klett inversion method". Subsection 3.1 (**page 7 onward**) has been largely rewritten.

Regarding the Klett inversion method (KIM) in the new Sect. 3.2 (**page 11 onward**), we explain the concept of deriving extinction profiles from the actually measured backscatter signal and present the derivation of the basic equations. Then we refer to Appendix A, where the description of our technical implementation of the KIM has been improved.

We have rewritten almost the entire Sect. 4 (**page 12 onward**) and changed the title to “Different CBH definitions”. We now begin by introducing the concept of visual range. We have added the relevant citation Koschmieder (1924) and present the derivation of the three visibility measures - MOR, VOR and SOR - in greater detail. In addition, we have rearranged some original sentences and even entire paragraphs to improve clarity and flow.

Please also note our respective responses to your comments on specific parts of the manuscript.

RC2: *A lot of editorial work on the manuscript is required, here some examples. At some occasions, the manuscript has a tendency to describe what follows later in the manuscript (e.g. l. 94 - 104, l. 112-116, ...) or to repeat what has been said before instead of just making a reference to the subsequent paragraph. Moreover some information which I consider belonging to the introduction follow in other sections (e.g. l. 126-128). In addition some detail information are not relevant or out of context and other phrases could be completely omitted (e.g. l. 128-130). Similarly don't overly*

repeat what you already stated earlier (e.g. l. 131-132). This can result in a somewhat fuzzy reading experience. Please do quite some re-structuring, also on points which I haven't mentioned (there are too many).

AC: The paragraph in lines 92–104 of the original manuscript has been rephrased and appears now in lines **106–112**. In addition, we created a new paragraph in **lines 135–138** that includes some original information. The paragraph in lines 112–116 of the original manuscript has been rephrased, starting with an information moved from the abstract, and the sentence: “The photos pertaining ...” has been removed (**lines 146–149**). In our opinion, especially in light of the first reference to Fig. 1, the information in lines 126–128 of the original manuscript are appropriately placed and now appear in **lines 159–161**. The sentence in lines 128–130 of the original manuscript has been removed. The sentence in lines 131–132 of the original manuscript has been shortened, combined with the following sentence and now appears in **line 164**.

Review of specific parts of the manuscript

RC2: - l. 3-4: formatting error with spacing and right margin

AC: Adding the citation header most likely ruined the formatting of these two lines in our preprint's abstract. The PDF that I uploaded to "MS records" on 13 May 2026 is correct. I cannot imagine having overlooked this formatting error while approving the preprint. Unfortunately, no changes were possible after publication of the preprint. Admittedly, this comes across as unprofessional, and we apologize for that. Hopefully, this won't happen again in the revised manuscript.

RC2: - l. 6-8: too much details for an abstract in this sentence

AC: We have moved the subordinate clause to Sect. 2 (**line 146**).

RC2: - l. 31: I suggest to remove "However" or replace it by a more appropriate formulation.

AC: “However” has been removed in **line 30**.

RC2: - l. 60: "\tau is the optical depth" -> "\tau is the optical depth between the instrument and range r"

AC: The variable r was originally introduced directly above Eq. (2). Implementing your suggestion would result in another repetition. Therefore, we have rephrased the text introducing τ and r below Eq. (2) in **line 64**.

RC2: - l. 60: I would move the sentence "The different methods..." to the next paragraph

AC: Done. This sentence now appears in **lines 68–69**.

RC2: - l. 68-69: why capitalizing the cloud types?

AC: We have corrected uppercase to lowercase letters in the cloud types (**lines 73–74**).

RC2: - l. 68-69: "to several 100 m or even a few 1 km for Cumulonimbus". Please rephrase and clarify

AC: “several 100 m” referred to a cumulonimbus cloud without precipitation. To avoid any misunderstanding, we now only mention the worst-case scenario of “... up to a few 1 km (3281 ft) for a cumulonimbus cloud with heavy rainfall.” (**lines 73–74**).

RC2: - l. 113: suggestion "... which is located 178m from the mast and 2m less in the third CircaHH phase as explained in the caption to Table 1." --> "... which is located 176-178 m from the mast (see Table 1)."

AC: We have adopted your suggestion (**lines 148–149**).

RC2: - l. 115: temporal resolution is confusing here. The shutter of the camera was not open for 1 minute, I guess. Moreover I think this information might not be needed here as you describe it in more detail at line 134-136. You can also consider to move the latter one upward, but don't duplicate.

AC: We have removed this sentence.

RC2: - Eq. 3.: I would like a more explicit reference than just "Wikipedia" from where this empirical formula is coming and also an explanation on the reason for using this mapping function rather than another.

AC: We refer here to our response to your second specific comment above.

RC2: - Eq. 4: Could you consider using another symbol than C to avoid confusion with lidar constant?

AC: The lidar constant has been renamed $\hat{C}_a$ in our Eq. (1), now has the subscript "a" and is a function of the wavelength λ (**lines 59–60**) The apparent and inherent contrast ratios are now denoted as C_n and $C_{n,0}$ in discrete and $C(h)$ and $C_0(h)$ in continuous notation, respectively (see response to your comment on lines 152–169 below). That means the Michelson contrast refers to a certain height h . We hope we have now introduced these variables in a clear and understandable way. Given the different indices and the dependence on either λ or h , we do not believe there is any risk of confusion with the lidar constant. In addition, "C" is commonly used as a symbol for contrast (e.g. Peli, 1990).

RC2: - l. 155-157: Please add a formula for this normalisation, it is needed for the good understanding of this statement and section.

AC: In the revised version of the manuscript, we have corrected Eq. (4) to reflect the Michelson contrast that we actually used in our source code for image analysis. In addition, we have added two relevant citations (Michelson, 1927; Peli, 1990) and explain why we have used this contrast (**lines 191–199**). In the original manuscript we had included Eq. (4.1) from Zinke (2016). Unfortunately, this equation refers to the Weber contrast, in which the red segment is implicitly treated as the background/reference. Consequently, the Weber contrast can become very large when the reference luminance is low. A normalization by the contrast value of the lowest red/white segment pair makes sense for the Weber contrast, if we assume they represent the reference visual condition. However, we actually applied the Michelson contrast and use the inherent contrast values as reference. A normalization is generally not required for the Michelson contrast. Therefore, we have deleted the two consecutive sentences: " C_n is normalized by ... Otherwise, low clouds would appear ... is unrestricted." in the revised version of the manuscript. We sincerely apologize for this confusion and hope we have clarified the matter in a way that is easy to understand.

RC2: - l. 152-169: I struggle to understand the difference between C_n and C and then their difference to $C(h)$ and $C_0(h)$. I was under the impression that C_0 must be the contrast value from the lowest 2 segments, but then how about $C_0(h)$. Similarly, what is the difference between h and H . Please clarify the variables used in this section.

AC: We have updated the variable names:

C_n	Michelson contrast for "apparent" conditions in discrete notation
$C_{n,0}$	Michelson contrast for "inherent" conditions in discrete notation
$C(h)$	C_n in continuous notation with dependence on height h
$C_0(h)$	$C_{n,0}$ in continuous notation with dependence on height h

Following Fig. 1 the optical path s is actually the distance d . We have explained this in the new Sect. 3.1 (**lines 201–211**) and hope that everything is now clear and easy to understand. The capital H is a typo and is replaced – where necessary – by a lowercase h .

RC2: - Eq. 6: I would keep it general with just Eq. 5 here and then move Eq. 6 to approach A) at line 172.

AC: We have incorporated your suggestion (**lines 213–217**).

RC2: - l. 181 and Eq. 9: Please explain the reasoning of setting $V=d$ and of using Eq. 9. For me it is unclear why you use Eq. 9 just to set $V=d$ afterwards. And you should also state what d is. Moreover, I don't understand why you come up with an equation from Weitkamp 2005 instead of just solving Eq. 5 with your linear sigma assumption. With this you should also come up with Eq. 10 without detour over external references and a reference to a subsequent equation and an obscure $V=d$ setting.

AC: We agree that substituting the original Eq. (8) into Eq. (5) yields the same result in Eq. (10). Therefore, we have extensively reworded our explanations for approach B) and corrected the corresponding derivations (**lines 218–234**). We have deleted the sentence: “Further details on approach B) are given by Zinke (2016).” to avoid confusion in terms of the Michelson contrast and the derivation of σ_{vs} . Further, we have moved the paragraph “In Eqs. (8)–(11) σ_0 was derived from ...” to item B).

RC2: - From Eq. 10 -> Eq. 11. Please state that you are setting $s=d$. Actually why do you have s and d as distinct variables?

AC: We have now explicitly written in **lines 208–209**: “Figure 1 shows that $s = d \dots$ ”. We introduce the optical path as “ s ” beforehand, following Eq. (10) of Eberhard (1986), because a “ $\sigma(d) dd$ ” term in the integral can be confusing.

RC2: - l. 186-187: This sentence does not tell anything to the reader at the current stage. Probably best to remove it.

AC: This sentence has been removed in the revised version of the manuscript.

RC2: - Fig. 3: I was puzzled that you aligned the subfigures from bottom to top. Why breaking the convention of going from top to bottom?

AC: We would have expected the page to be rotated intuitively to the right and the subfigures (a) through (f) to be read from left to right. If Fig. 3 were rotated by -90° instead of 90° , (a) through (f) would be correctly oriented from top to bottom but would appear to the right of the subfigures. This would certainly not comply with the AMT formatting guidelines either. Therefore, we have decided to present Fig. 3 and Figs. B2–B5 without rotating them, so that the numbering for the subfigures (a)–(f) appears at the top. In our opinion, everything is still easy to read.

RC2: - Fig. 3 c): I still don't understand how you calculated the inherent contrast and what you are using it for. Please make this clearer in the manuscript.

AC: We have clarified this in **lines 203–204** of the revised version of the manuscript: “The inherent contrast of all mast segments is essentially determined once per measuring campaign under favorable conditions and then used as a reference for calculating a relative apparent contrast $C_n/C_{n,0}$.” By “favorable conditions” we mean no clouds ≤ 300 m and unobscured horizontal visibility or ideally clear sky conditions, as explained in the preceding sentence and in the caption for Fig. 3(c). We have updated this caption accordingly.

RC2: - Fig. 3 f): These are vertical, not horizontal dashed lines. But it might be a good idea to have a horizontal dashed line denoting the 1000 m threshold

AC: Fig. 3 and the Figs. B2–B5 are now presented without rotation so that “horizontal dashed lines” is correct for the respective subfigure (g). We have added a vertical gray line at 1 km to subfigure 3(g) and similarly to the Figs. B2(g)–B5(g). In addition, we have adapted the caption of Fig. 3.

RC2: - Fig 3 f): At the time you introduce Fig. 3 in the manuscript you have not defined MOR or SOR, so you should introduce the figure after this definition.

AC: Done.

RC2: - l. 199: I would not call this section “Attempts” but rather just speak of different cloud base definitions”

AC: We have changed the title to “Different CBH definitions” (**page 12**).

RC2: - Section 4: This whole section deserves re-writing. You should guide the reader on the reasoning behind the different definitions. You do this somehow (but not well enough) a bit later but it should be done at the time you introduce them instead of throwing all the definitions in first.

AC: We have thoroughly revised Sect. 4. in the revised version of the manuscript, we begin by providing an introduction to the definitions of the visual range used, including their derivation and respective meanings for the observer (pilot). This makes the following list of tested, independent methods for estimating CBH shorter and clearer (**lines 270–326**).

RC2: l. 222-223: "The CBH results..." -> This sentence does not have anything to do here.

AC: We have removed this sentence. We have expanded the previous sentence to also mention the use of the extinction profiles from the KIM (**lines 335–336**).

RC2: l. 224: but Eq. 13 does so as well...

AC: We agree, and have therefore rephrased the sentence to clarify our main point (**lines 327–328**).

RC2: - Figure 4: You state that SOR-based CBH lead to lower values, but the drawing shows them being larger than MOR. It is also in contrast to what you state in the main text. Moreover you have 2 CBH SOR which I assume are for the two different extinction profiles in the right panel, but you should mention this, the reader isn't assumed to guess.

AC: We agree that the caption of Fig. 4 is currently worded in a somewhat misleading way. Taking your recommendation into account, we have reworded the caption and hope that our original message now comes across correctly.

RC2: - Eq. 14: Please explain this equation, i.e. the geometry, where the 3 in the nominator comes from, what is the idea behind, etc. This also applies to Eq. 12 and 13, but here the lack of explanation is most striking.

AC: We have extensively rephrased the text (see our response to your comment on Sect. 4 above). Everything should now be clear and understandable (**lines 271–311**).

RC2: - l. 215: What is the value of SOR_thr, 5%? Please re-formulate.

AC: Done.

RC2: - l. 241-242: The first 2 sentences belong to the figure caption, not the main text.

AC: We have removed these two sentences, while adding the information "(represented by black symbols)" to the caption of Fig. 5. In addition, we have slightly rephrased the beginning of the next sentence: "The comparison in Figs. 5(a-d) ..." (**line 346**).

RC2: - l. 284-285: omit this question falling out of the blue and which seems not to be absolutely needed

AC: This question was actually intended to serve as a transition to the next section. However, we have removed it because, admittedly, it is not really necessary.

RC2: - l. 288-289: I don't understand how you associate the MORs at the different altitude levels with the plausibility of the increasing CBH. Please explain.

AC: Figure 6(a) visually suggests that the CBH should be around 175 m between 9:00 and 9:45 UTC. Both the PWD20 at 175 m and the one at 280 m show a MOR well below 1 km here, while the PWS100 at 2 m measures a MOR greater than 4 km (Fig. 6(c)). Therefore, both PWD20s measure conditions like fog, but the actual CBH can be located slightly above or below 175 m. Between 9:45 and approximately 11:15 UTC, based on visual observations (Fig. 6(a)), the CBH should have been at a height between 175 m and 280 m. During this time period, only the PWD20 at 280 m measures a MOR clearly below 1 km (Fig. 6(c)), while the MORs at 175 m and, in particular, 2 m indicate that we are no longer within the cloud up to 175 m. Since the MORs from all three visibility sensors hardly change, it can be assumed that the CBH changes only slightly.

Consequently, there is an increase in CBH from the first to the second time period, which is consistent with the MOR measurements taken at the three different heights, especially in 175 m. Visually, the CBH rises again to about 280 m at 11:15 UTC (Fig. 6(a)). The MOR at 175 m height raises gradually until 12:00 UTC, while the MOR at 280 m indicates conditions similar to fog (Fig. 6(c)), and the corresponding PWD20 still measures most likely inside the cloud except for inhomogeneities. The behavior described for the MORs at the three heights (Fig. 6(c)) is qualitatively consistent with both the visual CBHs in Fig. 6(a) and the derived CBHs in Fig. 6(b), even though the actual CBH may differ from the heights of the PWD20 sensors. We have clarified our statement in **lines 391–395** of the revised manuscript.

RC2: - I. 296-303: *What is the added value of this reasoning in addition to Figure 7 what you introduce below?*

AC: Figure 7 compares the extinction coefficient σ_{vs} derived from image analysis using approach B) with the corresponding MOR measurements by PWD20 at 175 m and 280 m. The (unchanged) argumentation in **lines 402–409** also considers the σ_{cs} -profile derived from approach A) to clarify that the PWD20 measurements (green squares) agree better with the σ_{vs} -profile shown in Fig. 3(e) and Figs. B2(e)–B5(e). This comparison provides the additional information of relating the measurements at both heights to the extinction profiles derived from image analysis. In contrast, the temporal evolution at 175 m and 280 m focuses on the agreement as a function of time and therefore conveys a different aspect of the comparison. In addition, the discussion about Fig. 3(g) refers to the MOR definition, which is not shown in Fig. 7.

RC2: - I. 307: *What sensor problems and of what sensor, PWD22 or camera? Please be more specific and don't throw in such sentences which the reader cannot really understand.*

AC: These were issues with the PWD20 sensor. Visibility measurements at 280 m height were occasionally affected by electromagnetic interference from the television transmitter, leading to a temporary deviation in the measured visibility. This effect was identified from comparison measurements between all visibility sensors under well-mixed boundary-layer conditions. As its intermittent occurrence prevented a reliable correction, the measurements were retained for the analysis. We have mentioned this in **lines 413–414** of the revised manuscript.

RC2: - Section 5.3.: *I would not call this a thorough error estimation. Please provide a more in-depth analysis*

AC: We believe the current analysis is meaningful and reasonable. We have explicitly pointed out that "... we are unfortunately unable to provide an absolute or relative error value here.", since there is no reference available. Accordingly, the section has been renamed to "Uncertainty estimation of image analysis" (**page 21**).

RC2: - I. 335: *Why do you consider that the Klett inversion is not directly applicable to operational use? Please specify.*

AC: This is discussed in the last paragraph of Sect. 5.4 and Appendix A. Accordingly, we have rephrased this sentence to avoid any misunderstanding at this point (**lines 441–442**).

RC2: - I. 346: *smaller or larger?*

AC: For clarity, we have rephrased the relevant sentence (**lines 452–454**).

RC2: - Fig. 3 e): *Can you attribute this behavior to an artefact described in Kotthaus et al. 2016?*

AC: We assume that you are referring here to lines 358–360 and Fig. 10 of the original manuscript. We have added the following two sentences to our revised manuscript (**lines 467–470**): "Kotthaus et al. (2016) recommend correcting artifacts in the lowest 100 m (328 ft) if the β_{att} data comes from an older CL31 running a firmware version lower than 2.03. With firmware version 2.101, which is used here, this correction is not strictly necessary, and the unusual behavior of σ in the upper range of Fig. 10(b) cannot be attributed to any of the near-field artifacts discussed by Kotthaus et al. (2016)." We have also added the relevant citation to the bibliography.

RC2: - l. 374: when making such a statement, it would be needed that you specify which firmware algorithm uses which definition. Moreover, I would be a bit afraid to give reasons for the differences while the firmware algorithms used by the manufacturers are mostly black box.

AC: We have clarified this in the revised version of the manuscript (**lines 483–485**).

RC2: - l. 378-379: This sentence is not placed well in the Conclusions

AC: We have moved this sentence to the introduction and rewrote it (**lines 109–111**).

RC2: - Appendix A: The main equations of your Klett inversion must be outlined here. As current, this section is not followable by the reader with reasonable effort. Moreover, I have trouble to understand why you use LD40 parameters where e.g. for pulse per power, diameter, etc. Such basic parameters should also be available for the other ceilometers.

Moreover it appears unclear to me what is your reference region for the backward Klett algorithm. I guess the molecular signal is not accessible with ceilometers in the conditions of interest to you, hence, also from your statement at l. 424-428, I assume you use the known lidar ratio in the cloud as reference region. But I don't want to speculate, can you please be more explicit in the description of the method. I will be happy to read this section again after revision.

AC: As mentioned above, we have added a new Sect. 3.2, which presents the basic concept and governing equations of the KIM. Appendix A describes the technical implementation of our KIM and is referenced in Sect. 3.2. We have revised the reasoning regarding the LD40 parameters, which are not relevant at all when using the attenuated backscatter coefficient. This is a workaround when using the range-corrected signal ($P(r)r^2$), and we did not want to conceal this fact. We agree that, going forward, the parameters specific to the respective ceilometer should be used in the source code to determine the lidar constant $\hat{C}_a$, at least when $P(r)r^2$ is used as input for the KIM.

In the revised version of the manuscript (**lines 241–242**), we clarify: “This Klett inversion method (KIM) is based on the single-scattering lidar equation (Eq. (1)) that relates to elastic scattering processes and neglects the significantly weaker molecular contribution.” Of course, we are referring to cloud hydrometeors, specifically cloud droplets.

We have deleted the sentence: “Both the extinction coefficient ... along the optical path.” as it is not appropriate in this context. Instead, we have added the following sentence: “Consequently, we use the known lidar ratio within the cloud as a reference.” (**line 538**). Other parts of Appendix A have also been improved, such as the calculation of the SNR.

RC2: - Appendix B: This would be better placed in a supplement than the appendix, I guess.

AC: In our opinion, Figs. B1–B5 are appropriately placed in Appendix B for several reasons. First, Fig. B1 directly relates to Fig. 5(d) and provides a visual explanation for the artificial jumps in VS MOR observed there. Second, Figs. B2–B5 directly relate to Fig. 6 and provide additional vertical profiles analogous to those shown in Fig. 3. Third, these figures support the findings presented and do not constitute an explicit supplement to the main article. Fourth, there are only a few additional figures, which could partly be combined if necessary. Finally, moving these figures to a separate PDF would reduce the readability and accessibility of the manuscript.

However, we have renamed Appendix B to “Supporting results” (**page 31**) and have deleted “supplementary” before Figs. B1–B5 throughout the revised manuscript. This should be a good compromise.

In addition, we have updated or corrected some references in the bibliography. These changes are not highlighted in color. Please also note the changes made to the revised manuscript in response to the comments from anonymous Reviewer #1, which are highlighted in cyan.