Synergistic retrievals of ice in high clouds
Response to Reviewers

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Reviewer 1: ***General Summary:*** *This a well-written paper that fits within the scope of Journal of Atmospheric and Oceanic Technology and presents important and exciting results. The authors construct profiles of ice clouds based on CloudSat observations and then simulate retrievals based on the instruments of the planned AOS mission. There are, however, a few important ways in which the paper could be strengthened. I recommend publication after the following comments are addressed.*

We thank the reviewer for their perspective and constructive suggestions. Our point by point response is provided below.

Major Comments: *1) There are several large sources of uncertainty that will be present in real retrievals from AOS that are ignored in this paper. While there is certainly still value in such analysis, it needs to be made more clear in the paper that this is an idealized scenario, and there should be more discussion about the sources of uncertainty that will affect real-world retrievals. Several in particular stand out to me. For one, it appears that all of the simulated measurements have been created at the same resolution (presumably, the resolution of CloudSat). This should be clarified, and the authors should discuss how the lidar, radar, and radiometer will all have different resolutions and how this might affect results. Second, the electromagnetic scattering properties assumed in this study are unlikely to perfectly match reality. Finally, it appears that no measurement uncertainty has been assumed. The last paragraph of the paper does briefly mention many of these uncertainties, but they need to be introduced earlier in the manuscript, and more details are needed about the specifics of how IMPACTS will help mitigate them.*

We thank the reviewer for the uncertainty discussion. Yes, indeed the resolution of the simulated observations is that of the CloudSat radar, while the AOS instruments are expected to have different resolutions, which might impact results. In the revised manuscript, we included statements to convey this aspect. We agree that the assumed electromagnetic scattering properties assumed in the study, although reasonable, may contain significant uncertainties. We acknowledge this aspect in the revised manuscript as well. Regarding the measurement uncertainties, we realistically account for them in the radar and radiometer simulations, and in a rather idealized way in the lidar simulations. Specifically, we assume random errors with 0.0 mean and 0.5dB standard deviation in the radar observations [1], and a Noise-Equivalent-Delta-T (NEDT) of 1K, which is a readily achievable level for modern satellite radiometers [2]. A complex model of space-borne lidar measurement uncertainties was developed in [3], but its parameters are difficult to reliably quantify from theoretical considerations based on instrument expectations alone. Instead, we assume a simple multiplicative error model, with the multiplicative factor model as a log-normally distributed random variable with 0.0 mean and 0.1 standard deviation, which results in observation uncertainties similar to those in the in evaluation study of the Cloud-Aerosol Transport System (CATS) lidar-system. We acknowledge the limitations of this approach in the manuscript. Regarding the IMPACTS mission, we added statements in the revised manuscript to discuss how it will help mitigate the uncertainties. Specifically, to achieve its objectives, which were driven by the need to improve the understanding of snowfall processes, remote sensing of snow, and the prediction of banded snow structures (McMurdie et al. 2022), IMPACTS relied on a suite of active and passive instruments deployed via a satellite-simulating aircraft. These included multiple radars, one of them operating at Ku-band, a 532 nm elastic lidar, and a sub-millimeter-wave radiometer similar to the one considered in this study. While the objectives of IMPACTS were snowstorms, the instruments used in the campaign sampled a wide range of clouds, including high ice clouds. The IMPACTS observations associated with high ice clouds may be used to derive IWC estimates that may be directly validated using "in-situ" measurements, as the high-altitude aircraft flew in coordination with a cloud penetrating aircraft that carried cloud and ice particle probes. These data, although not fully available yet (as the sub-millimeter-wave radiometer was deployed only in the 2023 campaign), are expected to provide valuable information on the accuracy of the IWC retrievals from synergistic lidar, Ku-band radar and sub-millimeter-wave radiometer observations and enable the refinement of the retrieval algorithm formulated in this study.

2) You say in your abstract (and conclusion) that "the characteristics of the instruments e.g., frequencies, sensitivities, etc. are set based on the expected characteristics of the AOS mission." However, the AOS mission specifications are still very much in flux. Therefore, for reproducibility, I suggest adding a table explicitly laying out the frequencies and sensitivities you assume for each instrument (radar, lidar, radiometer). Some of these (e.g., the radiometer frequencies) are mentioned in the text, but it would be nice to see it all in one place.

We agree with the reviewer that, unfortunately, the AOS mission is still in flux. We added a table in the revised manuscript to summarize the assumed instrument characteristics.

Minor Comments *Line 38: Where is 8.0 dBZ coming from? There should be a citation here, or it should be stated that this is an estimate of the radar's expected sensitivity.*

This is an estimate of the radar's expected sensitivity provided by Japan Aerospace Exploration Agency (JAXA), which is the AOS International partner contributing the Ku-band radar in the inclined orbit.

Lines 50-51: While your approach is justifiable and worthwhile, it is worth noting that your CloudSat IWC retrieval also makes numerous assumptions about ice cloud microphysical properties and structures (as laid out in Section 2a). So the problem is not entirely avoided by relying on "observations" instead of models, as the observations were of radar reflectivity, not of ice water contents or particle sizes.

We agree. We do not consider CloudSat retrievals uncertainty or bias free, but we prefer them because they are consistent with an observed vertical distribution of reflectivity at W-band. The vertical distributions of radar reflectivity derived from satellite or ground-based observations are generally difficult to reproduce by model simulations. We added statements to this effect in the revised manuscript (lines 67-69 in the Introduction).

Lines 65-67: This is fine, but it should be noted that in doing this, you are underestimating the true retrieval uncertainty. The particle distribution assumptions and electromagnetic scattering properties that you use in training the retrieval algorithm will almost certainly not perfectly reflect the actual size and shape distributions of ice particles around the globe.

We agree. As described explained above, our objective was to derive IWC distributions consistent with the observed vertical distribution of CloudSat reflectivity, without implying that the resulting distributions are uncertainty or bias free. To make the performance assessment more realistic, in the revised version of the manuscript, we perturbed the N_w value (which in the first version of the manuscript was parameterized as a function of height) used in the derivation of IWC as a function of observed CloudSat reflectivity by a random log-normally distributed factor. This accounts for uncertainties in the PSDs (at least from the perspective of the normalized PSD framework) and makes the evaluation realistic. Nevertheless, as observed by the reviewer, the electromagnetic scattering properties are an additional source of uncertainty. We included additional statements in the manuscript (bottom of page 8 and top of page 9) to describe N_w perturbation approach and discuss uncertainties in the electromagnetic scattering properties that are not quantified in the manuscript.

Lines 83-90 and Eq. (1): I looked over the Testud et al. (2001) paper, and could not find anything that directly supported this statement, or any equations that resembled Eq. (1). Please add further explanation, either here or in an appendix, to help the reader follow your logic.

We apologize for the confusion. Testud et al. (2001) only show that normalization by N_w (N_0^* in their notation) explains the variability in Z-R relationships (fig. 9d). Ferreira et al. (2001) [4] make the more general statement that any two integrated rainfall parameters (X, Y) may be related through a power law of the form $X = mN_w^{(1-n)}X^n$, where m and n are constants almost independent of N_w. Delanoe et al. (2014) [12] show that the same relationship holds for ice PSDs. We added statements to this effect in the paragraph following the definition of Equation (1).

Figure 2: Neither the caption nor the text adequately explains what is being plotted in this figure. What does each individual point represent? The text says, "Figure 2 is a scatter plot representation of the class multiplicative coefficient ai as a function of relative height," but if that were the case, I would expect there to be many more points on the scatter plot (i.e. 18 classes times a certain number of relative height levels, to yield hundreds of points).

We apologize for the confusion. The points in figure 2 were derived from a classification of the IWC profiles into 36 classes. We initially used 36 classes in the analysis, but we reduced the number of classes to 18 to make the classification and the Ensemble Kalman Filter more robust. For consistency and simplicity in the revised manuscript we derive IWC-Z relationships for each CloudSat radar bin, with the bins indexed relative to the freezing level bin. The relative height is defined as the distance between the height of the associated bin relative to the freezing level. Results are rather similar, although the overall performance is degraded to some degree due to the inclusion of the N_w perturbation component.

Lines 120-121: How is the data in Fig. 2 used to parameterize Nw as a function of height? Do you fit a line/curve to the data in Fig. 2? This needs to be clarified.

Yes, we fit a line to determine the variation of $ln(a)$ as a function of height and then divide the slope by $(1-b)$ to get the variation of $ln(N_w)$ as a function of height. We added explanations to this effect in the revised manuscript (lines 114-124).

Figure 3: It would be helpful to add vertical lines to these plots to indicate the detection thresholds you are assuming for the radar (8 dBZ?) and lidar.

We included vertical lines to indicate the detection thresholds.

Line 220: It is my understanding that an ensemble Kalman smoother assumes normal distributions. How much does it matter that IWC is not normally distributed in nature?

Yes, the ensemble Kalman smoother assumes normal distributions. We derive a Kalman gain, i.e. a matrix of type $\mathbf{Cov}(\mathbf{X},\mathbf{Y})\mathbf{Cov}(\mathbf{Y},\mathbf{Y})^{-\mathbf{1}}$, for every class, and although IWC is not normally distributed in nature, its conditional class distributions are not likely to be as skewed as the entire distribution. This is most likely why the Kalman smoother produces rather accurate results when observations from all instruments is available. Nevertheless, from the theoretical perspective, the Kalman Smoother results are suboptimal given the non-Gaussian distribution of IWC. This does not necessarily mean that more accurate results may be derived in practice given the multitude of uncertainty factors that may impact the results.

Typos*Line 276: "wells" should be "well"*

Thank you for pointing that out. We corrected it in the revised version of the manuscript.

Line 308: Figure 8 should be Figure 9

Thank you for pointing that out. We corrected it in the revised version of the manuscript.

Line 315: IWP should be IWC

Thank you for pointing that out. We corrected it in the revised version of the manuscript.

Line 361: eliminate "out

Thank you for pointing that out. We corrected it in the revised version of the manuscript.

Reviewer 2:
The study investigates the synergy between observations from elastic backscatter lidar, Ku-band radar, and sub-millimeter radiometer observations to retrieve ice concentrations. The topic is undoubtedly timely, considering that AOS, NASA's upcoming satellite mission targeting remote sensing of the atmosphere, is currently in its design phase. The methodology is sound, albeit slightly contrived. Methods and results are presented in an accessible way. Nonetheless, there are significant conceptual flaws in the study.

We thank the reviewer for their perspective and constructive suggestions.

First of all, I can't entirely agree with the study's framing. Currently, the study investigates the synergies achieved by adding lidar and radar observations to radiometer observations. However, since the active sensors will likely have a much narrower swath, combining these observations will only be possible at a small part of the radiometer's swath. Given the significantly higher information content of the active observations, the more relevant question is: What is the benefit of including radiometer observations when radar and lidar observations are available? Simply showing an added value from adding radar and/or lidar observations to radiometer observations, which is the current main conclusion of the paper, is a trivial result.

Although active observations are characterized by much narrower swaths and significantly worse temporal sampling, they are nevertheless crucial in process studies and the development and validation of products from radiometer-only observations ([5],[6],[7]). The particular combination of active and passive observations considered in this study is justified by the current design of the AOS mission. Specifically, the Ku-band radar is intended to enable accurate estimates of the convective precipitation. Although it was originally envisioned that AOS would deploy two Ku-band radars, one in an inclined orbit and one in a polar orbit, currently, only the inclined orbit is expected to feature a Ku-band radar contributed by Japan Aerospace Exploration Agency - JAXA. The polar orbit radar will operate at higher-frequencies (Ka- or W-and) and have cloud-detecting capabilities (i.e. high-sensitivity), but will not be able to provide direct observations of intense convective precipitation due to attenuation. Moreover, the polar radar will have nadir-only capabilities, while the inclined orbit radar will have a swath of about 200 km. Both the polar and inclined orbit radars will operate in tandem with a lidar and a high-frequency radiometer. We are interested in the inclined orbit despite the significantly lower sensitivity of the Ku-band radar because the synergistic Ku-band radar, elastic lidar and radiometer estimates of ice in anvil clouds can be studied jointly with various parameters (i.e. precipitation intensity, vertical velocities, etc.) characterizing the convective processes responsible for their creation. Although the polar orbit instruments are likely to enable the derivation of more accurate synergistic ice estimates, their severely limited ability to quantify convection makes synergistic ice retrievals from the combination of instruments studied in this manuscript relevant to the problem of relating the strength of convection to the horizontal extent of anvil clouds and their impact on climate [8]. Although the improvement in the ice estimates due to the inclusion of radiometer observations may seem trivial, it is rather significant given that neither the lidar nor the Ku-band radar can provide by themselves accurate and complete ice estimates, the lidar due to attenuation and the radar due to its limited sensitivity and complex Particle Size Distribution (PSD) variability.

Secondly, since the dependency of the multiplicative factor a is parametrized by relative height, the authors essentially use a single-moment retrieval to determine the profile database upon which they base their whole study. This approach will significantly underestimate the variability in the retrieved PSDs. Pfreundschuh et al. 2020 show that this variability causes considerable uncertainty in radar-only retrievals of ice water path and is one of the dimensions in which radiometer observations can help to improve radar-only retrievals. Neglecting this variability likely significantly overestimates the accuracy of the proposed retrievals. Finally, I do not consider that the omission of seven (!) of the ten radiometer channels is justified, given that the authors use the inability of the radiometer to reproduce the vertical distribution of ice particles as a principal motivation for studying the synergy with active observations.

We agree with the reviewer. The parametrization of the multiplicative factor a is equivalent to a single-moment retrieval. In the revised manuscript, to investigate the impact of variability in N_w, we randomly perturbed its parameterized value by a log-normally distributed random variable with zero mean a standard deviation of 0.5. This was achieved by generating one-dimensional vectors of independent random variables with a standard normal distribution and applying a Gaussian smoothing filter [10]. The filter size was chosen to be two CloudSat radar bins. The initial standard deviation was scaled to result a standard deviation of 0.5 after smoothing, which is roughly consistent with the variability in N_w observed in real observations [11].

Moreover, the revised manuscript includes all radiometer channels in the analysis. It should be noted though that the 183.31 and 321.15-GHz radiometer channels are centered on water vapor absorption lines. The differences in the brightness temperatures observed by these channels are driven by the vertical distribution of water vapor and have only indirect impact on the retrieval of ice. This is particularly true for the simulated observations considered in this study, which are not characterized by large variability in the vertical distribution of water vapor. Consequently, the inability of the radiometer to reproduce the vertical distribution of ice particles is not likely related to the omission of these channels. We nevertheless included all channels in the retrievals, as the vertical distribution of moisture is likely to be important in future versions of the methodology when more diverse environmental contexts are going to be considered.

Major comments: *1. To address the first-mentioned issue above, the authors should include radar-only, lidar-only, and radar-lidar retrieval in their analysis.*

We agree with the reviewer. The revised manuscript includes radar-only, lidar-only, and other combinations of instruments retrievals in the analysis. Shown in Fig. 1 are density plots of single-instrument retrievals of IWP and IWC as a function of their reference values. As apparent in the figure, retrievals from single active instruments exhibit significant deficiencies. This is not surprising given that the lidar observations are subject to significant attenuation, while a Ku-radar with the AOS specifications misses a significant fraction of the ice clouds. A W-band cloud radar would do much better by itself, but as already stated in our response, a Ku-band radar in conjunction with a backscatter lidar and a high-frequency radiometer is a very exciting prospect, as it enables the study of the relation between the strength of convection and the characteristics of associated anvil clouds.

2. To improve the representation of microphysical variability in the retrieval, I suggest that the authors at least randomize the parametrization of the multiplicative factor a in a way that covers the spread of the distribution shown in Fig. 2.

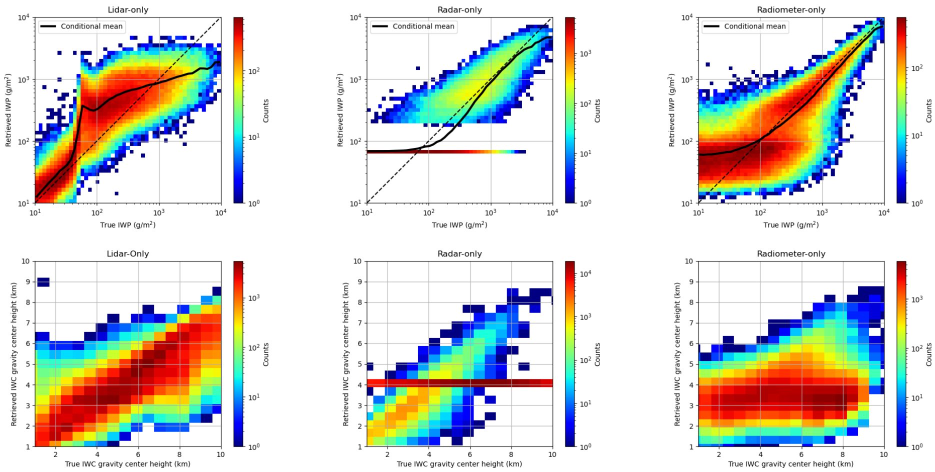

Figure 1: Top Row: Density plots of IWP retrievals from lidar-only (left), radar-only (middle), and radiometer-only (right) observations as a function of true IWP. Bottom Row: Same as in the top row but for IWC.

We agree with the reviewer. The revised manuscript includes a randomization of the parametrization of the multiplicative factor a. Specifically, we randomly perturb N_w by multiplying its parameterized value with log-normally distributed random variable with zero mean a standard deviation of 0.5. As explained above, we use a Gaussian smoothing filter to impose a vertical auto-correlation in N_w similar to that observed in field experiments. This is explained in lines 155-165 of the revised manuscript.

3. The authors should include all radiometer channels in their analysis.

As explained above, we included all the radiometer channels in the revised version of the manuscript.

4. The introduction's title and first sentence mention "high ice clouds". However, the authors do not provide any information on how the 'high ice clouds' are defined or what distinguishes them from regular ice clouds.

In AOS terminology, high clouds are convectively generated high clouds [9]. Such clouds are the result of strong vertical mass transport from the lower troposphere and horizontal transport in the upper troposphere. While preponderantly consisting of ice, high clouds may exhibit liquid phased hydrometeors. To make it clear upfront that we are not considering mixed-phase clouds in this study, we included ice in the title. We included clarifying statements on the definition of high-clouds in the manuscript introduction.

5. Although the authors state that the sensor characteristics are set based on the expected characteristics of the AOS mission instruments, it is unclear whether thermal noise is included in the retrieval experiments. The authors should mention this in the description of their methodology.

Regarding the measurement uncertainties, we account for them in the radar and radiometer simulations, and in a rather idealized way in the lidar simulations. Specifically, we assume random errors with 0.0 mean and 0.5dB standard deviation in the radar observations [1], and a Noise-Equivalent-Delta-T (NEDT) of 1K, which is a readily achievable level for modern satellite radiometers [2]. A complex model of space-borne lidar measurement uncertainties was developed in [3], but its parameters are difficult to reliably quantify from theoretical considerations based on instrument expectations alone. Instead, we assume a simple multiplicative error model, with the multiplicative factor model as a log-normally distributed random variable with 0.0 mean and 0.1 standard deviation, which result in observation uncertainties similar to those in the in evaluation study of the Cloud-Aerosol Transport System (CATS) lidar-system. We acknowledge the limitations of this approach in the revised manuscript. Statements to this effect are included in the paragraphs devoted to the descriptions of the forward models.

Minor comments *1. l. 29 - 31: Please provide more information on the planned or possible observation configuration of AOS, such as swath widths of the active instruments and the type of radiometer. The provided reference (Braun 2022) is missing, and I could not find conclusive information on this online.*

We apologize for the missing reference. We included a reference to an IEEE IGARS conference paper [9] that provides a brief description of the AOS mission. The IEEE document is publicly available and provides a link to an additional AOS document (https://aos.gsfc.nasa.gov/docs/ACCP_Science_Narrative-2021.07.19.pdf). However, the swath widths of the active instruments are not explicitly specified, as architectures and instruments have been not finalized. Nevertheless, the Ku-band radar will be based on the GPM DPR radar and is expected to have a swath width of 245 km. The elastic lidar will be in many respects similar to the CALIPSO/CALIOP lidar (https://calipso.cnes.fr/en/CALIPSO/lidar.htm) and will provide nadir observations, while the radiometer is expected to have a swath of around 1000 km.

2. l. 83 -90: The results by Testud 2001 are for raindrops assuming Rayleigh scattering. It is not evident that this is also generally true for ice particles.

The impact of normalization on Z-IWC relationship is shown in [12]. Soft spheres were assumed and the Mie solution was used in [12] to derive reflectivities from observed Particle Size Distributions, but [13] used more advanced backscattering models to calculate Z from observed PSDs. Their calculations are publicly available at https://github.com/dopplerchase/Chase_et_al_2021_NN and may be used to confirm the contraction of the (Z/N_w,IWC/N_w) distribution to almost one-to-one relationships. As explained in the manuscript, we use the self-similar Rayleigh-Gans approximation to model the backscattering property of ice particles but get a normalization behavior similar to that anticipated by Testud et al. (2001), shown in [12] and exhbited in the [13] database. We added statements to this effect in the paragraphs describing Equation (1).

3. l. 96: Please clarify: What do the authors mean by 'similarity'? Euclidean distance?

Yes, the similarity is measured by the Euclidean distance. We added a sentence to clarify this point.

4. l. 112 and Fig. 2: Please clarify: Why are there 36 scatter points in Fig. 2.? How is the relative height value of the scatter points derived?

We initially started with 36 similarity classes and derived (through linear regressions in the log-log space) an IWC-Z relationship for each class. The points in Fig. 2 represent the coefficients a as a function of the height of the class-average IWC peak for each class. We subsequently reduced the number of classes to 18 to make the classification and estimation process more robust. However, for consistency and simplicity in the revised manuscript we derive IWC-Z relationships for each CloudSat radar bins, with the bins indexed relative to the freezing level bin. The relative height is defined as the distance between the height of the associated bin relative to the freezing level. Results are rather similar, although the overall performance is degraded to some degree due to the inclusion of the N_w perturbation.

5. Fig. 2: Please add a line showing the parametrization used to derive the database.

Thanks for the suggestion. We added a line showing the parametrization used to derive the database.

6. l. 130: Please clarify: What is the assumed shape of the ice particles? The ice particles are aggregates of bullet rosettes, columnar crystals, and plates. The aggregation model and other details are comprehensively described in [14]. We added a sentence in the manuscript to clarify this point.

7. Fig. 3: Please clarify: Why is the distribution of radar reflectivities going to zero towards the freezing level? What about precipitating clouds?

We selected only CloudSat reflectivity profiles with no echo at and below the freezing level. Profiles that satisfy this requirement include anvil clouds, whose quantification in relation to the convective process with which they are associated is an AOS objective. We added a sentence in the manuscript to clarify this point.

8. l. 181 - 182: Please clarify: Do the authors mean surface emissivities?

Yes, we mean surface emissivities. We clarified this in the manuscript.

9. l. 198: Please provide more information on how the IWC and PSD parameters are estimated. If more than one moment is estimated, the authors must use a priori assumptions to resolve the ambiguity in the retrievals.

We derive the IWC as a function of CloudSat Z and N_w. The N_w is given by the vertical parameterization and perturbed using the random model described above and in the revised manuscript. Specifically, given N_w and the associated Z at W-band, we estimate IWC/N_w as a function of Z/N_w using the normalized tables and then derive IWC from IWC/N_w and N_w. We added a statement in the manuscript to clarify this point.

10. l. 203: I don't think that 200,000 profiles can be considered a large dataset by today's standards.

We agree that 200,000 profiles is not a large dataset by today's standards. However, these profiles are derived from a month of CloudSat observations. Specifically, all CloudSat and 2C-ICE product files from June 2019 were processed, and only profiles with no cloud echo at and below the freezing level were selected. Moreover, in the first version of the manuscript, we imposed the rather artificial condition that at least three CloudSat reflectivity pixels above the freezing level be classified as cloud for a profile to be select. These two conditions resulted in a rather small number of profiles. We removed the second condition is the selection of profiles used in the revised analysis. This resulted in about 800,000 of profiles. While 75% of these profiles consist of at most three pixels, there is sufficient vertical variability in the selected profiles to derive meaningful conclusions. We added statements in the manuscript to clarify this aspect.

11. Fig. 4: Please add lines marking the diagonal and conditional mean

Thank you for the suggestion. We added the lines to the figure.

12. Fig. 6: I think it would be valuable to check whether the classification step causes the multi-modal distribution in the retrieval results. Please produce a version of Fig. 6 in which the 18 centers of gravity of the clusters are marked using horizontal lines. The is, however, no need to include the figure in the manuscript if that doesn't lead to any conclusive results.

Thank you for the suggestion. We added the lines to the figure.

Typos *1. l. 323: Missing word*

Thank you for pointing this out. We added the missing word.

References

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