Response to reviewer #1

We thank the first anonymous reviewer for the effort of reviewing the manuscript. The constructive comments helped to improve the quality of the manuscript, especially with respect to references and the nuances on how they are embedded. In the following response, each point is answered separately. The reviewers' comments are included as black text, while the blue text are the responses.

Convection over the tropical Atlantic is frequently coinciding with Saharan Air Layers and alters deep convection (e.g. Wong and Dessler (2005)) and shallow convection (e.g. Narenpitak et al. (2023)). The observations described and analysed in this study from the BOWTIE campaign support previous studies and provide a higher resolution, particularly in the vertical than previous studies have analysed in this context.

Unfortunately, the manuscript includes a large amount of references that are out of context, are not supportive and mixed up. The authors state that artificial intelligence tools have been used to support "the literature research by finding relevant literature", but do not appear to have checked the results. This becomes particularly apparent by including a colorscale description in a sentence that can be found nearly identical online.

Based on these findings the manuscript currently does not gain my trust and I recommend rejection of the manuscript. All references need to be thoroughly checked and algorithms, products and results correctly described and interpreted.

We highly appreciate the careful review of the manuscript and the references by the first reviewer. We appreciate the effort to check the references and point out the incorrect context or wrong citations of the papers. Incorporating the comments and a re-evaluation of the literature in general will improve the quality of the manuscript.

However, we want to clarify the use of AI for this study. We only used the AI tool ResearchRabbit to assist us in finding articles related to other articles that we had already identified. We did not rely on results found by AI for specific statements, nor did we create summaries of papers we found. Other than ResearchRabbit, Google scholar was used to find relevant literature.

Major concerns:

l. 193f: The algorithm description is nearly identical to https://tropic.ssec.wisc.edu/misc/sal/ info.sal.g16split.html and including the colorbar description “denoted by yellow pink shading”. While in the next sentence the manuscript states “ the pink-white shading”, presumably still referring to the same plot and quantity, the dust layer.

Thank you for pointing out this concern. This is true, the wording is indeed identical, which happened accidentally when incorporating a co-author’s comment without checking the wording against the original description. The sentence is reformulated and merged with the following sentence to address repetition and inconsistent terminology. However, most of the technical wording did not change since this sentence describes an existing algorithm for which terminology must remain concise. The original sentence on the webpage, the one in the submitted manuscript and the revised sentence are given below:

Webpage: “The “split window” algorithm is sensitive to the presence of dry and/or dusty air in the lower to middle levels of the atmosphere (~600-925 hPa or ~4,500-800 m) and is denoted by yellow to pink shading (Dunion and Velden 2004)”

Submitted manuscript: The algorithm of the Split Window product is sensitive to the presence of dry and/or dusty air in the lower to middle levels of the atmosphere (600-925 hPa or 4,500-800 m) and is denoted by yellow to pink shading (Dunion and Velden, 2004). The pink-white shading qualitatively indicates likely SAL regions with increased amount of dust content and dry lower-tropospheric air.

Modified sentence for the manuscript: *As the algorithm of the “split window” product is responsive to the presence of dry and/or dusty air in the lower to middle levels of the atmosphere (~600-925 hPa or ~4,500-800 m), the yellow to pink shading qualitatively indicates SAL regions (Dunion and Velden 2004).*

References:

l. 28: Nicholson, 2018 focuses on the African landmass, where the term ITCZ is highly controversial. It does not support the claim here.

Yes, that’s true, this reference does not fit here anymore. It originates from the Master’s thesis on which this manuscript is based. The intention of this sentence was to remind the reader of the role of the ITCZ in the context of global circulation. We removed the reference Nicholson (2018) and focus more on the global circulation (and Hadley cell) by referring to Hadley (1735) and Kraus (2005). The sentence is now rewritten as follows:

Modified version in line 27f: *In a general, simplified scheme of atmospheric circulation, the Intertropical Convergence Zone (ITCZ) is known to be the rising branch of the Hadley cell (Hadley 1735, Kraus 2005) where directional and speed convergence govern the formation of convective clouds.*

l. 41: Foltz and McPhaden, 2010 investigate the ocean dynamics of Atlantic meridional and Nino modes and is not about clouds. Foltz et al. 2019 is also not a very fitting citation here as it is has a strong focus on the ocean rather than the atmosphere.

Thank you for sharing your thought on the use of these references here. The focus of the passage was to emphasize the coupling of multiple disciplines (oceanographic and atmospheric research), which was indeed pointed out by Foltz et al. (2019). Further, Foltz et al. (2019) illustrate the difficulties of operating any kind of instrumentation on the open ocean, even for oceanographic observations. The reference to Foltz and McPhaden (2010) indeed does not contribute to these claims. Hence, we decided to remove the Foltz and McPhaden (2010) but keep the Foltz et al. (2019) reference in the revised version. Additionally, we added the Omanovic et al., 2026, to include a study aiming to understand observations of cloud microphysics by the means of high-resolution large-eddy simulations.

Modified version l 40-44: *A comprehensive analysis of cloud properties and their evolution require the synthesis of models and observations to unravel the coupling of various cloud-related processes and patterns (e.g., Foltz et al., 2019; Omanovic et al., 2026). However, over oceanic regions, atmospheric observations are under-represented because deployment of instrumentation is difficult and maintenance of instrumentation is negatively impacted by water, salt, erosion and waves (Foltz et al., 2019).*

l. 91: please explain in more detail what is meant by "ERA5-based retrievals", as Schnitt et al. (2024) use ERA5 quantities only for validation.

As over the ocean there are few observations for the training of a MWR retrieval, we used ERA5 data to train the retrieval, while Schnitt et al. (2024) used radiosonde observations for the training. The method, however, is similar as described in Schnitt et al. (2024) and the code can be found in Foth et al. (2023).

Modified version l.91f: *For the MWR, a set of ERA5-based retrievals specifically for the tropical Atlantic was created as described for an observation-based retrieval in Schnitt et al. (2024) using the pyMakeRetrieval routine (Foth, 2023; Foth et al., 2024) with ERA5 data for the training.*

l. 98: Wrong reference. Should be Acquistapace et al. 2022 (https://doi.org/10.5194/essd-14-33-2022)

Thank you for catching this mistake. The author's name was correct however the year was unintentionally wrong. The reference should target EUREC4A, because they used the ship-borne observations and corrected for heave. The reference is replaced.

l.139f.: Is IER really from IWC and lidar extinction? In the cited papers and the cloudnetpy software I can only find radar products for IER.

Thank you for this comment. You are right, in the final retrieval of IER the lidar extinction is not considered anymore, since the Z-T relationship (eq.7) of Griesche et al. (2020) is implemented in the current cloudnetpy version. Delanoë et al. (2007), however, provided the more general concept of deriving IER from IWC, which was used by Griesche et al (2020) to develop the new empirical relation. In the end, we remove the reference Delanoë et al. (2007) because it does not contribute to the understanding and data processing of this study.

Modified version for l 139f*: Cloudnet derives Ice Effective Radius (IER) from IWC using the reflectivity–temperature relationship ship of Griesche et al. (2020)*.

l. 250: Gutleben et al. (2019b) shows no “enhanced heating at the ocean surface”. It shows a stronger net cooling in the marine boundary layer. How a “heating at the ocean surface” “leads to lower tropospheric stabilisation” is not clear to me.

Thank you for this comment. You are right the Gutleben et al. (2019b) publication suggests altered radiative heating profiles by SAL and the associated dust and water vapour. After reviewing the article again, we found that we had inadvertently mixed up the height levels of radiative cooling and heating. The net effect is stabilisation when a SAL is present.
When reviewing this reference, we also noticed that Gutleben et al. (2019b) contrary to Wong et al. (2009) considered an enhanced concentration of water vapor on the SAL. In our case we rather found lower relative humidities in the SAL. Nonetheless, Wong et al. (2009) also found stabilising effects of the SAL, also with drier SAL layers than the environment. The adapted and reformulated passage is given below.

Modified version for line 250 – 253: *However, as e.g., evident in Fig. 3 G, the relative humidity in the lower part of SAL is higher than in the upper half. The low relative humidity and dust radiative effects lead to positive heating rates in the mid-levels of the troposphere, and negative radiative heating anomalies in the marine BL, respectively. This in turn leads to lower tropospheric stabilisation and mid-level tropospheric destabilisation (Wong et al., 2009; Barreto et al., 2022).*

l. 290: How does Yang et al., 2013 support this claim. Yang et al., 2013 is about dust sedimentation and not about convection.

You are right that this citation was wrongly listed in the context of convection, and we removed it. We furthermore replace the reference Wong and Dessler (2005) by Wong et al. (2009), which is explained in the next comment.

Modified version for line 290: *Weak convection either cannot overcome the energy barrier at the inversion, or it dries out by entrainment in the lowest part of the SAL (Wong et al., 2009; Gutleben et al., 2019b).*

At various locations in the manuscript Wong and Dessler, 2005 is cited. That study is about the impact of SAL on deep convection with a brightness temperature of 250K, much higher than the low and mid-level clouds studied here.

Thank you for identifying this issue. You are right, the conclusions drawn by Wong & Dessler (2005) account for the effects of SAL on deep convection, which extends beyond our definition of Cumulus Congestus. The authors Wong & Dessler (2005), however also state: "The choice of the threshold brightness temperature does not affect the main conclusion of this study." Further, their analysis of modified temperature and humidity profiles suggests that a convection barrier is lifting the LFC and suppressing convection, leading not only to a reduction of deep convective clouds but also shallow cumulus and cumulus congestus. Nevertheless, all references of Wong & Dessler, have been checked and placed in the correct context. At the following lines we decided to remove the reference or rephrase the sentence:

L 253: We replace the reference Wong and Dessler (2005) by Wong et al. (2009).

L 302 – see next comment

L 318f.: Wong and Dessler (2005) reference removed: *Lower CTHs aligned with previous studies (e.g. Gutleben et al., 2019a), which have found that shallow marine trade wind clouds exhibit reduced CTHs*, ...

L. 371f – The word "Weak" was removed*: Convection is capped by the SAL base, because the energy barrier for further ascent is increased (Wong and Dessler, 2005).*

l. 302: "CuCon clouds decreases in the presence of Saharan dust [...], which is in agreement with Wong and Dessler (2005)" does not hold. Wong and Dessler (2005) looked at deep convection with cloud tops way below freezing, according to the table in the supplement in this manuscript "CuCon" have CTH below the freezing level.

See comment above. The specific correction for the mentioned citation:

Modified version for l 302ff: *In particular, the frequency of CuCon clouds decreases in the presence of Saharan dust by about one third compared to when no dust was observed, which is in agreement with Gutleben et al. (2019a). Additionally, the decrease in Cb aligns with findings from Wong and Dessler (2005) who considered the effect of Saharan dust on deep convective clouds.*

Tab1.: max vertical range of 14km does not agree with the sounding profile in Fig. A3 which goes already to 18km.

Thanks for pointing this out. This might be a mistake from editing the table. The radiosondes sometimes reached beyond 30 km, the entry in Tabl1 is modified accordingly (to "mostly < 30 km").

Minor comments/typos:

throughout: the term “alto clouds” is rarely used in the scientific literature. Please use “mid-level clouds” or another common term.

We carefully considered this suggestion. The term alto cloud in this paper developed from the Master’s thesis, by summarising the subtypes altostratus and altocumulus. However, the word mid-level clouds would be easier for readability. Therefore, we decided to replace “alto clouds” by “mid-level clouds”. This also matches the naming of “high clouds” better.

l. 27: “direction and strength of convergence”

fixed

l. 59: unrendered reference

fixed

l. 114 + l. 191f: these long URLs disrupts the reading flow quite a bit and might also not persist. Could these figures with the URL as reference be added as supplement?

This is very true. We will address this in the final manuscript, considering either a supplement or the usage of footnotes.

Fig. 4: The lower bar indicating the dust cases and no-dust periods includes some white areas (Sept 3 - Sept 21) which are explained in l. 260ff, but it would help to state here in the caption as well that these are cases are discarded from the direct comparison of SAL/no-SAL.

Thank you for this useful suggestion. We added a sentence to the figure caption:

Modified caption: *Figure 4. Ambient conditions as hours per day of cloud and rain occurrence, with classes “clear sky”, “probably cloudy”, “confidently cloudy” and “rainy” based on radar, ceilometer and rain gauge signals. In the lower bar orange indicates the cases of Saharan dust. The blue shading indicates the periods of dust free conditions in the same region as the observed dust, therefore serving as comparison period. The uncoloured (white) periods of the campaign in the lower bar were not considered for the comparison of dust vs. non-dust cases.*

l. 323f.: You mention that “enhanced occurrence of alto clouds in study periods only occurs AT THE TOP of the SAL”, but looking at e.g. case 3 on Aug 31, the mid-level clouds are even above the SAL and not visibly connected. This does not apply to all cases, but it is a nuance that should be mentioned. Did the authors consider running backtrajectories at the mid-level cloud height or analyse satellite images to understand the origin of these mid-level clouds and whether or not they are originating from outside the SAL?

Thank you for sharing your thoughts on this section of the manuscript. Yes, the mid-level clouds are indeed not always physically connected to the SAL and can occur rather above the SAL than at the top. Thank you for making us aware of this nuance; the modified sentence can be found below. However, understanding the origin of mid-level clouds is a complex topic and beyond the scope of this study. Other researchers from the ORCESTRA team are addressing this topic (Giovanni Biagioli, personal communication Sept 2026; Macintyre Syrett, personal communication Sept 2026; Syrett et al. (2026)). Secondly, yes, we considered performing HYSPLIT back trajectory analysis for this research as a proof of the origin of the airmass from the Saharan Desert. However, since the occurrence of SAL was confirmed by CAMS AOD and the GOES16 Split window, using HYSPLIT would have been exaggerated for this purpose. For understanding the cloud formation HYSPLIT might still be a good tool, but the focus of this study is not on understanding the mid-level clouds specifically, but on more cloud types in general. Therefore, we think a HYSPLIT analysis goes beyond the scope of this paper.

Modified version for l. 323f: *The enhanced occurrence of alto clouds in dusty periods only occurs above the top of the SAL in approximately 6 km altitude.*

References:

The references given in normal type were already included in the original manuscript. References in *italic* were added whilst preparing the response to the first review.

Acquistapace, C., Coulter, R., Crewell, S., Garcia-Benadi, A., Gierens, R., Labbri, G., Myagkov, A., Risse, N., and Schween, J. H.: EUREC⁴A's Maria S. Merian ship-based cloud and micro rain radar observations of clouds and precipitation, Earth Syst. Sci. Data, 14, 33–55, https://doi.org/10.5194/essd-14-33-2022, 2022.

Delanoë, J., Protat, A., Bouniol, D., Heymsfield, A., Bansemer, A., and Brown, P.: The characterization of ice cloud properties from Doppler radar measurements, Journal of Applied Meteorology and Climatology, 46, 1682–1698, https://doi.org/https://doi.org/10.1175/JAM2543.1, 2007

Dunion, J. P. and Velden, C. S.: The Impact of the Saharan Air Layer on Atlantic Tropical Cyclone Activity, Bulletin of the American Meteorological Society, 85, 353–366, https://doi.org/10.1175/bams-85-3-353, 2004.

Foltz, G. R., Brandt, P., Richter, I., Rodríguez-Fonseca, B., Hernandez, F., Dengler, M., Rodrigues, R. R., Schmidt, J. O., Yu, L., Lefevre, N., DaCunha, L. C., McPhaden, M. J.,

Araujo, M., Karstensen, J., Hahn, J., Martín-Rey, M., Patricola-DiRosario, C. M., Poli, P., Zuidema, P., Hummels, R., Perez, R. C., Hatje, V., Lübbecke, J. F., Polo, I., Lumpkin, R., Bourlès, B., Asuquo, F. E., Lehodey, P., Conchon, A., Chang, P., Dandin, P., Schmid, C., Sutton, A., Giordani, H., Xue, Y., Illig, S., Losada, T., Grodsky, S. A., Gasparin, F., Lee, T., Mohino, E., Nobre, P., Wanninkhof, R., Keenlyside, N., Garcon, V., Sánchez-Gómez, E., Nnamchi, H. C., Drévillon, M., Storto, A., Remy, E., Lazar, A., Speich, S., Goes, M., Dorrington, T., Johns, W. E., Moum, J. N., Robinson, C., Perruche, C., de Souza, R. B., Gaye, A. T., López-Parages, J., Monerie, P.-A., Castellanos, P., Benson, N. U., Hounkonnou, M. N., Duhá, J. T., Laxenaire, R., and Reul, N.: The Tropical Atlantic Observing System, Frontiers in Marine Science, 6, https://doi.org/10.3389/fmars.2019.00206, 2019.

Foth, A.: remsens-lim/pyMakeRetrieval: pyMakeRetrieval v1.1.1, https://doi.org/10.5281/ZENODO.10014291, 2023.

Foth, A., Lochmann, M., Saavedra Garfias, P., and Kalesse-Los, H.: Determination of low-level temperature profiles from microwave ra diometer observations during rain, Atmospheric Measurement Techniques, 17, 7169–7181, https://doi.org/10.5194/amt-17-7169-2024, 2024.

Griesche, H. J., Seifert, P., Ansmann, A., Baars, H., Barrientos Velasco, C., Bühl, J., Engelmann, R., Radenz, M., Zhenping, Y., and Macke, A.: Application of the shipborne remote sensing supersite OCEANET for profiling of Arctic aerosols and clouds during *Polarstern* cruise PS106, Atmospheric Measurement Techniques, 13, 5335–5358, https://doi.org/10.5194/amt-13-5335-2020, 2020.

Gutleben, M., Groß, S., and Wirth, M.: Cloud macro-physical properties in Saharan-dust-laden and dust-free North Atlantic trade wind regimes: a lidar case study, Atmospheric Chemistry and Physics, 19, 10659–10673, https://doi.org/10.5194/acp-19-10659-2019, 2019a.

Gutleben, M., Groß, S., Wirth, M., Emde, C., and Mayer, B.: Impacts of Water Vapor on Saharan Air Layer Radiative Heating, Geophysical Research Letters, 46, 14854–14862, https://doi.org/10.1029/2019gl085344, 2019b.

Hadley, G. VI. Concerning the cause of the general trade-winds. Philosophical Transactions of the Royal Society of London, 39(437):58–62 (1735). ISSN 0261-0523, 2053-9223. doi:10.1098/rstl.1735.0014.

Kraus, H.: Die Atmosphäre der Erde, Springer Berlin Heidelberg, Berlin, Heidelberg, 3 edn., ISBN 978-3-540-20656-9, https://doi.org/10.1007/3-540-35017-9, 2007.

Nicholson, S. E.: The ITCZ and the Seasonal Cycle over Equatorial Africa, Bulletin of the American Meteorological Society, 99, 337–348, https://doi.org/10.1175/BAMS-D-16-0287.1, 2018.

Omanovic, N., Bötticher, D., Fuchs, C., and Lohmann, U.: Glaciogenic seeding-induced hole-punch clouds and their sensitivity to the clouds' background state, Atmos. Chem. Phys., 26, 5345–5353, https://doi.org/10.5194/acp-26-5345-2026, 2026.

Schnitt, S., Foth, A., Kalesse-Los, H., Mech, M., Acquistapace, C., Jansen, F., Löhnert, U., Pospichal, B., Röttenbacher, J., Crewell, S., and Stevens, B.: Ground- and ship-based microwave radiometer measurements during EUREC [4] A, Earth System Science Data, 16, 681–700, https://doi.org/10.5194/essd-16-681-2024, 2024

Syrett, M.M., J.H. Ruppert Jr., A.A. Wing, M.M. Bell, Examining mid-level stable layers over the tropical Atlantic during ORCESTRA, Geophys. Res. Lett., in review, doi:10.22541/essoar.15008923v1.

Wong, S., Dessler, A. E., Mahowald, N. M., Yang, P., and Feng, Q.: Maintenance of Lower Tropospheric Temperature Inversion in the Saharan Air Layer by Dust and Dry Anomaly, J. Climate, 22, 5149–5162, https://doi.org/10.1175/2009JCLI2847.1, 2009.

Wong, S. and Dessler, A. E.: Suppression of deep convection over the tropical North Atlantic by the Saharan Air Layer, Geophysical Research Letters, 32, https://doi.org/10.1029/2004GL022295, 2005.

Yang, W., Marshak, A., Kostinski, A. B., and Várnai, T.: Shape-induced gravitational sorting of Saharan dust during transat lantic voyage: Evidence from CALIOP lidar depolarization measurements, Geophysical Research Letters, 40, 3281–3286, https://doi.org/https://doi.org/10.1002/grl.50603, 2013.