Response

We thank the reviewer for reviewing our manuscript and providing very helpful and constructive suggestions. We have carefully considered all comments and have made changes to the manuscript. Please find below a detailed point-by-point response to all comments.

General Comments:

1) The general impression is that the manuscript is written in a rush way and few ideas are mixed. English spelling is poor, but develops for the better as the manuscript progresses. A careful reading is necessary to smooth the whole document both the structure to there are many redundant sentences, the spelling, and miss-referencing of figures. May the authors ask a help from an native speaker at the department(s) for an assistance if necessary.

All the above suggestions have been adopted. The revised manuscript is concise. All co-authors have read through the manuscript and are happy with this version.

2) Data, methods, and results are mixed. Please structure the publication carefully. Temporal and spatial resolution is missing for any kind of data used and should be given in the data section. The description of the total precipitable water retrieval is missing in the data section. ERA5 is a powerful dataset that provides a great variety of meteorological variables, but which of them used for the study and their description is missing.

We have restructured the manuscript by adding the methodology section and moving the relevant parts into the data section and method section as suggested.

The temporal and spatial resolutions have been added.

The total precipitable water has been deleted as another reviewer suggested.

The ERA5 description has been added.

3) In my view, there are to many figures. The authors carefully should reduce the amount of figures. Move figures to appendix/supplementary material. Zoom on the FCS. Please show the 102 m W m^{-2} $(cm^{-1})^{-1}$ isoline in any of the satellite images (radiance, CTH, COD) to clearly identify the FCS.

The number of figures has been reduced substantially. Figures 21-24 have been replaced by a single figure. Figures 25-26 have been deleted. Several other figures have been merged. The figure number has been reduced from 26 to 15.

The radiance has been converted to brightness temperature as suggested by another reviewer. The new Figure 7 shows the images with the threshold lines.

Be consistent (UTC) vs. [UTC] or ( ) vs. [microns]. Please use round brackets.

We have replaced the labels of Figures 9-10. They are now consistent.

Specific Comments

1) line 93: The authors state they follow the tracking methodology of Fiolleau and Roca (2013). It is an automatic tracking algorithm and identification is based on image segmentation using ve brightness temperature intervals. This is a contradiction to manual

tracking based on one radiance threshold value. Please clarify.

We did not using the tracking algorithm of Fiolleau and Roca (2013). Instead, we were inspired by their method. We have clarified in the revised version. “We follow their method to characterize the flower system on 2 February 2020.” has been changed to “We were inspired by their method for detecting ing and tracking a cloud system using a brightness temperature threshold to characterize the flower system on 2 February 2020.”

2) lines 269-276: I wonder how the propagation speed was calculated. Which method was used to identify the arcs, etc pp. Please describe it in the methodology section.

The following has been added in the methodology section.

A cloud arc associated with a cold pool moves due to a combination of factors, including its propagation, advection by the average winds within the cloud-bearing layers of its environment, and interactions with other cold pools. As a result, the true propagation speed of the cloud arc is the difference between its actual speed and the advection speed in cases where there are no interactions between cold pools. To determine the propagation speed, we first positioned the satellite image so that the flower was centred within the frame at each time. Next, we measured the distances between the cloud arcs at two subsequent times and used these measurements to estimate the propagation speed. Our speed estimation was limited to periods when the cloud arcs were well-defined and easily distinguishable.

3) line 351: The authors state that the increase in SST, which is only 0.3 K, plays an important role in increasing the boundary layer height, which allows for higher cloud top. Vial et al. (2019) considered LES as well as in-situ and satellite observations to conclude that SST and associated variations in sea-surface fluxes unlikely to play an important role in driving the diurnal cycle of marine trade-wind cumulus cloud cover in the Carribean. This is contradictory to your statement. Please clarify.

Sea surface temperature is a a controlling factor as discussed in the Introduction. We added the following in Section 6: Vial et al. (2019) considered Large Eddy Simulation (LES) data, as well as in-situ and satellite observations, and concluded that SST and its associated sea-surface flux variations are unlikely to be significant contributors to the diurnal cycle of marine trade-wind cumulus cloud cover in the Caribbean. However, Chen et al. (2023) explored the influence of minor SST anomalies at the submeso- to meso-scale level on the daily average of trade cumulus cloudiness, using satellite observations validated against ship-based measurements collected during the Atlantic Tradewind Ocean-Atmosphere Mesoscale Interaction Campaign (ATOMIC), conducted in conjunction with the EUREC^{4}A field campaign. Their findings highlighted that localized variations in SST have the potential to influence daily cloud cover, with the primary driving force being the spatial diversity in surface-induced turbulence and surface heat flux, rather than the effects of surface or boundary layer convergence. Although the small change in SST of this case was unlikely of considerable importance in the development of the flower, the exact effect of SST is worth further investigation.

4) line 354: The authors make use of the sea-surface temperature and argue there is cold advection at the surface. This is misleading because the velocity of the sea surface, as is known, is different to the wind speed at 10 m above sea-surface. Would it not be better to use the 2 m temperature from ERA5?

We have deleted the figure containing the cold advection. As the reviewer suggested, number of figures has been reduced by combining and simplifying.

Technical Comments

1) Please be consistent in wording. E.g. SAFIRE-CORE = French ATR aircraft = microphysiscs airborne platform (PMA) observations. And others …

A consistent way of wording has been adopted as the reviewer suggested. The text has been changed accordingly.

2) line 89: Can you please clarify what MCSs is.

MCSs has been changed to “mesoscale convective systems (MCSs)”.

3) line 119: Please give more information. To my knowledge CPSD is not a GOES-16 ABI retrieval.

The cloud particle size is an ABI Level 2 retrieval (Heidinger et al., 2020).

The following has been added in the description of COD and CPS.

The ABI Level 2 Cloud Particle Size (CPS) product provides the effective radius of the particles in a single cloud layer. Different algorithms are used for daytime and nighttime conditions. The retrieval of daytime cloud optical and microphysical properties is extensively explained in Walther and Heidinger (2012) and in the Algorithm Theoretical Basis Document by Walther and colleagues (2013). This methodology relies on measurements obtained from two distinct channels: one operating in the visible range as a non-absorbing window channel and another in the near-infrared part of the spectrum, which is weakly absorbing. Optical thickness is primarily determined through the visible channel's reflectance, but the absorption channel furnishes additional insights into absorber volume, enabling the estimation of an effective particle size. Furthermore, it indirectly aids in adjusting cloud optical depth (COD) estimates by accounting for variations in the phase function due to changes in forward-scattering direction resulting from differing particle sizes.

4) Section 3 Synoptic pattern, line 146: ...a frontal system... Is the frontal system always associated with FCSs or is the pre-cold frontal area favourable for FCSs?

“So-called flower clouds (Bony et al. 2020) were evident around 60 °W, near Barbados.” has been changed to “So-called flower clouds (Bony et al. 2020; Schulz, 2022) were evident around 60 °W, near Barbados. It will be shown later that the flower cloud system studied in this paper developed from lines of convective clouds in the pre-cold frontal area.”

5) lines 154 : This is methodology.

The paragraph in Lines 154-158 has been changed to “The detection of a cloud system is carried out by specifying a brightness temperature threshold on an IR image to mark the border of contiguous cloudy regions where the brightness temperature smaller than the threshold (e.g., Maddox, 1980). The threshold varies from case to case, in the range of 205 K to 233 K for mesoscale convective systems with deep convections (Maddox, 1980; Machado et al., 1998, Fiolleau and Roca, 2013). After a series of tests and trials, we found that a brightness temperature threshold of 290 K of GOES-16 IR channel 14 (11.2 μm) was a good choice to identify the flower system. The threshold is consistent with Figure S2 of Bony et al. (2020).”

This paragraph has been moved to the methodology section.

6) line 157: After a series of ... Can the authors describe why the chosen threshold is suitable for the identification of the FCS.

Please see the above, i.e., reply to comment on Line 154.

7) lines 165 : This is methodology or maybe introduction.

This paragraph has been moved to the methodology section.

8) line172: The life cycle is divided in three stages based mainly on the area of the FCS. The subsequent lines describe CTH is another constraint. Please indicate the constraints.

"based mainly on the area of the system" has been changed to "based mainly on the area and the CTH of the system.

9) line 179: ITC -> UTC

This has been corrected.

10) line 179: ...at high levels -> do you mean at cirrus level? Please clarify.

"The decaying stage was marked by a decreasing cloud area from 1900 ITC (Figure 3a) although some clouds still remained at high levels (Figure 4)." has been changed to "The decaying stage was marked by a decreasing cloud area from 19:00 ITC (Figure 3a) although some clouds still reached approximately 3 km above sea level (Figure 4)."

11) line 188: I wonder what happens between 0500 UTC and 0900 UTC?

More information can be found in the discussion of Figure 7.

12) line 189: The authors state that the local maxima in CTH were related to convective activity. Isn't the development of the FCS also related to convective activity? Please be specific.

"The local maxima in CTH (Figure 6h – 6j, and 6l) were related to convective activity." has been changed to "The local maxima in CTH (Figure 6h – 6j, and 6l) and the development of the system were related to convective activity."

13) line 190-191: ...can be seen in Figures 5h - 5i... I cannot see it. Do you mean Figures 6h-i?

"detrained cloudy air due to cumulus development can be seen in Figures 5h – 5i." Has been changed to "detrained cloudy air due to cumulus development can be seen in a region to the north-west of 56.5 °W and 12.5 °N in Figure 5i."

14) line 193: expansion of growing cumulus -> Do you mean vertical growth?

"...was caused by the expansion of growing cumulus" has been changed to "...was caused by the vertical development of cumulus in the area."

15) line 195: What do you mean with 'linked by a linear cloud'? Do you mean 2 FCS form a cloud street, a cloud line or maybe shallow mesoscale overturning circulations (SMOCs; George, 2023).

The new figure indicated that the outlines of the two cluster were not in contact. “Two main cloud clusters are shown in Figure 7a linked by a linear cloud as mentioned above.” has been changed to “Two main cloud clusters had not yet merged as shown in Figure 7a.”

16) line 196: The authors describe a cloud region separated from the main cloud. Is the process of splitting and merging an important feature in the development of a FCS? Please clarify.

“The splitting did not cause the drop in total area of the system during this period (Figure 3a), which implies that the splitting did not hinder the further development of the system.” has been added after “A cloud region separated from the main cloud between 07:40 – 08:00 UTC as indicated by an arrow in Figures 7b-c”.

17) line 197: Please describe the term 'area of the detrainment/entrainment cloud region' in more detail

“The area of the detrainment cloud region in the central cluster, as indicated by an arrow in Figure 7f, was seen to increase further in the next two hours.” has been changed to “The area of the central cluster, as indicated by an arrow in Figure 7f, was seen to increase further in the next two hours.”

18) line 202/203: What do you mean with '... developed to higher levels and were not dominated by those with top heights lower than 1600 m...'?

“and were not dominated by those with top height lower than 1600 m after the fast development (Figure 4).” has been deleted.

19) line 204: ...some clouds... Do you mean convectively active parts of the FCS?

“Some clouds developed to higher levels (Figures 8.b1-b5).” has been changed to “The convectively active parts of the system developed to higher levels (Figures 8.b1-b5).”

20) line 208: at 1220 UTC there is only one Figure -> 8.c5

This has been corrected.

21) line 213: brown color -> dark red

“(brown colour)” has been deleted since the figure was replotted using a different colour bar and merged with other figures as another reviewer suggested.

22) line 222: there is no Figure 9.b1 – b5

This has been corrected.

23) line 225/226: Please give a reference to guide the reader

However, there was significant local variability and indeed inversion tops determined from some dropsondes reached about 3100 m.” has been changed to “However, there was significant local variability and indeed inversion tops determined from some dropsondes reached about 3100 m (Figure 14a in dropsonde analysis).”

24) lines 230-253:
Figure references, e.g 8.d6 = 1240 UTC

The figure has been replotted as another reviewer suggested. The time of each row has been added to the new figure.

Which type of distribution is shown in Figure 9 (absolute, relative, probability density)?

Figure 9 shows the drop size distribution function. See the reply to the next question.

What does #L/micron mean in Figure 9?

The drop number size distribution function is the number of particles per unit volume between size D and $D + dD$. The unit is the drop number per volume (e.g., liter) per micrometer of size range. The y-axis label has been changed to "Concentration (L^{-1} μm^{-1})"

What is the meaning of the dashed vertical lines in Figure 9?

The dashed vertical lines have been removed.

It could be worth to combine Figures 10 and 11.

Figures 11 and 12 have been combined.

What does mean volume diameter mean? Is it MVD = 2re?

"... the median volume diameter (MVD), defined as the median of the cumulative mass size distribution, were derived from the composite size distributions" has been described in the data section.

Lines 239-242 refer to a height, but there is no height visible at all in Figure 10.

The following information has been added in Line 250.

The aircraft passed the first three clouds at 1100 m above sea level before ascending from 2040 m to 3060 m across the fourth cloud.

Line 250/251 '...perhaps suggest...' → shows

This has been corrected.

25) line 258: The authors state in section 4.1 the mature stage lasted until 1900 UTC.

This figure was used for both the mature and dissipating stages. Please note that another reviewer suggested to combine Figures 13-16 to form a single figure at a reduced frequency of one hour, which has been adopted. A new figure has been produced and replaced the figures. Figure numbers have been changed accordingly.

26) line 259: The gust fronts ... Is it the area highligted by the pink arrow? Please clarify.

The figure has been replotted as suggested by another reviewer and the interval has been reduced to 1 hour. It is now only one arrow in Figure 12a1.

27) line 262/263: '...one region of precipitation seen in the satellite observation...' This is speculative because satellite imagery shows re at cloud top. Furthermore, there is no Figure 7d9.

"There was only one region of precipitation seen in the satellite observations (Figure 7d9 for example)" has been changed to "There was only one region of effective radius greater than 40 μm (indicating the presence of precipitation) seen in the satellite observations (Figure 8d9) for example."

28) line 267: (Figure 13f) -> the pink arrow? Please clarify.

Please note that another reviewer suggested to combine Figures 13-16 to form a single figure at a reduced frequency of one hour, which has been adopted. The new figure uses red arrows. The text has changed to the new figure.

29) line 278: see comment line 258.

See response to Line 258.

30) line 279/280: ...did not change much... I realize a wobbling patch.

"did not change much" has been changed to "did not change significantly".

31) line 281: ...a few local maxima of about 50 ... And some patches > 50.

"than 30, with a few local maxima of about 50 appearing from time to time." has been changed to "than 30, with a few local maxima of about 50 and some patches > 50 occasionally"

32) line 285: ...the maximum values were about 45 μm ... decreasing to 45 μm…

", decreasing to 45 μm or less at 1600 UTC" has been deleted.

33) line 293: Is there an explanation why re is particularly high at the boundary of the FCS?

We have noticed the higher values of the effective radius at the edges of the flower. The possible reason is that overestimation in r_e increases for scattering angles greater than 140° (Painemal et al., 2021). We did not cite Painemal et al. (2021) because they evaluated the GOES-13 imager rather than the GOES-16 ABI product. Our focus is the organised region of large r_e in the central part of the flower which is unlikely affected by the large scattering angles as at the cloud edges.

34) lines 296-302: Move to methodology section. Figure 17 can be combined with Figure 11.

Lines 296-302 have been changed to "The HALO aircraft flew a clover pattern on 2 February (Konow et al. 2021). Those dropsondes allowed us to understand the temporal variation of the vertical stratification at certain locations. It took about 15 min for a dropsonde to fall from ~ 9 km into the boundary layer. We selected four dropsondes falling into or near the flower system. The temperature, relative humidity, and wind of the four dropsondes were examined in order to understand the temporal variations." It has been moved to the methodology section as the reviewer suggested.

Figure 17 has been deleted as another reviewer suggested. We have combined Figure 11 and Figure 12.

35) lines 309-310: Please clarify which channel and quantity (radiance, brightness temp.) is shown. Focus in Fig. 19 on the FCS, e.g. decrease lon/lat.

The caption has been changed to "Locations of the dropsondes imposed on radiance images of the GOES-16 ABI Band 6 for dropsonde W1-W4."

The domain has been reduced to focus on the flower system.

Please note that it is Figure 13 in the revised version.

36) lines 311/312: several times UTC is missing

UTC times have been added.

37) line 345 : there is no -(lon) W. Check also the subsequent sections.

This has been changed and all other occurrences have been checked and corrected.

38) line 372: Differences are usually given in K.

Figures 23, 25, and 26 have been removed. Figures 21,22, and 24 have been simplified and merged to a single figure. The text in Section 6 has been rewritten. Therefore, Lines 355 to 444 were deleted. As a results, there are not responses to comments below on Lines 355 – 444.

39) line 386: d -> d

See reply to Line 372.

40) line 403: The authors refer to cloud top height and pressure, but only the height is given. Please clarify.

See reply to Line 372.

41) line 410: ...during the developing and decaying stages... -> throughout the life cycle?

See reply to Line 372.

42) lines 412 : move to data section.

See reply to Line 372.

43) line 430: MODIS is already defined.

See reply to Line 372.

44) line 480: EUREC4A -> EUREC4A

It has been corrected.

45) line 481: (2021).. -> (2021)

It has been corrected.

46) line 485: ...propagation. Analysis…

This has been changed to “propagation analysis.”

47) References: Please check alphabetical order.

We have checked the order and made necessary changes.

48) page 22, Figure 1: Please indicate the time step of the surface weather analysis. To my knowledge the MODIS stripes are separated by about 100 minutes.

The caption of Figure 1 has been changed to “Figure 1: The MODIS Terra satellite image imposed with the surface pressure chart at 1200 UTC on 2 February 2020. The passover times of the satellite were approximately between 12:05 – 12:18, 13:45 – 13:57, and 15:23 – 15:36 UTC from east to west swaths, respectively.”

49) page 24, Figure 3: Shows the x-axis UTC or the time from evolution until decaying? Caption: describe the vertical lines.

The x-axis label has been changed to Time (UTC). The vertical lines have been removed.

50) page 25, Figure 4: Use the same tick marks on the x-axis as in Figure 3. Show the 3 stages in the life time of the FCS, e.g. vertical dotted line. Show colorbar label (unit).

The new Figure 4 has been changed using the same tick marks on the x-axis as in Figure 3. The colour bar unit has been added.

51) pages 26 : Please revise the captions. Check (missing) colorbars -> labels/units. Figure 12 -> which time (UTC, local time); velocity unit missing; LWC unit missing. Figure 20a -> check unit.

The x-axis label has been changed to Time (UTC). The units of w and LWC have been added. The Unit in Figure 20 has been changed.

52) Figures: be consistent with latitude and longitude. -59 = 59W; latitude 10 = 10N

Yes, all the latitude and longitude have been checked and they are in a consistent way.

References

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