Response to reviewer 1

In this response letter, we repeat the reviewer's questions and answer them one by one. Our response is marked in blue letters.

Review of the paper "A meteorological overview of the ORACLES (ObseRvations of Aerosols above CLouds and their intEractionS) campaign over the southeast Atlantic during 2016-2018" by Ryoo et al. 2021, submitted to Atmospheric Chemistry and Physics Discussions.

General Comments

The paper describes the atmospheric conditions during three field campaigns over southeast Atlantic in 2016-2018. The manuscript is well written and clear, figures quality is good and captions are informative. The authors provide a comprehensive picture of the climatological conditions, seasonal anomalies and synoptic evolution during the deployments, and physical mechanisms are correctly described and widely detailed. The paper appears to be specifically addressed to the community studying the physics of the atmosphere in the southeast Atlantic and provides a valuable contribution to all the teams involved in field campaigns during 2016-2018 for putting their observations into a synoptic context.

However, in my opinion the paper remains almost exclusively descriptive and does not respond to any relevant scientific question. In the main text, several interesting scientific questions are highlighted, but the authors do not investigate any of them, only speculating on possible (always plausible) explanations and requiring further investigation in future papers. For instance, I found very interesting the anomaly (almost disappearance and/or lifting) of the AEJ-S in August 2017, which would be worth to be investigated, in terms of both local and remote drivers.

This is my main and only concern. Despite the overall good quality of the paper itself, I am not sure it fits with the scope of ACP and/or the special issue. I list below a few general and specific suggestions on what I believe could be improved.

=> Thank you for your valuable comments. This is a meteorological overview paper, so our main focus was to describe the overall meteorological conditions during the 2016-2018 ORACLES campaigns. While investigating underlying mechanism with more depth was important, this was not our focus in this overview paper.

Following your suggestions, we trimmed lots of text and kept the key figures. Furthermore, since 1) both reviewers pointed out the length of the manuscripts and 2) the other reviewer suggested that we separate the paper into two parts (***part 1***: climatology at monthly scales; ***part 2***: deployment month at daily to synoptic scales), we decided to separate the paper into two parts. In this revision, we will focus on the part 1 (climatology). We also examined further the possible environmental factors to cause the anomalous AEJ-S in August 2017 in this revision. We found a few distinctive characteristics during August 2017, but among them, we think upper-level disturbances play a large role in modulating the large-scale environment and thereby the AEJ-S. In order words, the position of the upper-level disturbance induces an anomalous circulation, thereby changing the cross-latitude temperature gradient over the land, ultimately reducing AEJ-S. We will describe this more in detail with figures in the response letter below as well as in the revised manuscript.

We have also further investigated a possible remote driver for weakening AEJ-S. Although beyond the scope of this paper to investigate further, previous other works indicates that wave activity supporting convection near the equator is weaker when the MJO is weak (Guo et al., 2014), and in turn the northern AEJ (AEJ-N), located to the north of the Congo, is also weakened (Ventrice and Thorncroft, 2013). Preliminary analysis showed that MJO convection was weak in August 2017 over Africa region. The impact of the MJO to the AEJ-S remains underexplored, however, and we can only speculate that a weaker MJO might also help weaken the AEJ-S. Furthermore, the mechanism of the magnitude of MJO influence on Southern Africa is still poorly characterized (Zaitchik, 2017).

More importantly, as mentioned earlier, this is an "overview" paper for better interpreting ORACLES measurements, so we want to keep focusing on this part, rather than investigating the detailed processes. Thus, we will briefly discuss this in the text without very detailed figures and explanations. Instead, we will put some results related to this remote driver in the supplementary materials.

The paper is very long, with many (25!) multi-panel figures. Figures are often the repetition of similar analysis, it is not easy for the reader to stay focused on the narrative of the paper. The authors could try select non-key information in text and figures and move it to the Supplement.

=> Both reviewers pointed this out, and we worked on this part. We trimmed the texts and tried to keep the main key parts. We also separate the paper into two parts following the second reviewer's suggestion.

In the Summary and Discussion, the authors speculate on the effect of meteorological conditions on the aerosol transport but no evidence of the actual effect is provided (while the cloud-circulation relationship is well described in the paper). I believe this is a key aspect which could provide added value to the paper. ECMWF CAMS reanalysis could be used to frame the aerosol patterns at the regional scale.

=> Thank you for the comments. Following the reviewer's suggestion, we also looked at the ECMWF CAMS reanalysis data. Since we will focus on climatology in this paper, we observed the features from the monthly mean data.

The BC and CO have similar features as in August 2017; particularly the BC mixing ratio is "lower" in August 2017 than the climatological mean, especially off the coast and over the ocean, shown in Fig. R1. We think this is reasonable because a weaker AEJ-S leads to weaker advection of these burning trace constituents out over the SE Atlantic. BC mixing ratio in September 2016 are slightly higher than the climatological mean, especially at the jet entrance region over land following the ascent.

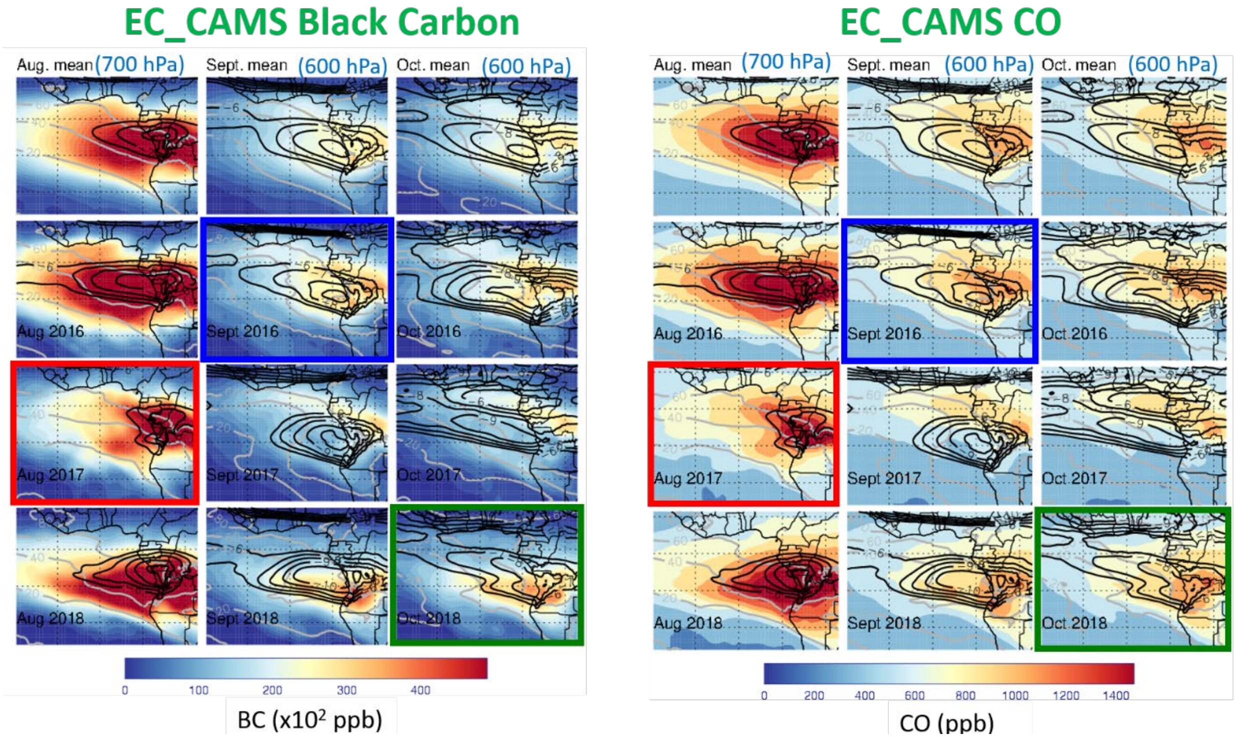

Figure R1. Map of monthly mean (left panel) black carbon mixing ratio (BC, shading, 100 x ppb) and (right panel) Carbon Monoxide mixing ratio (CO, shading, ppb) overlaid by AEJ-S and RH at 600 hPa for August and at 700 hPa for September and October for the (1st row) climatological mean and (2nd – 4th row) the deployment month. The climatology for the AEJ-S and RH is based on 2000-2018 from ERA5, and BC/CO is based on 2003-2020 from ECMWF CAMS due to data availability. The color boxes (red, blue, and green) indicate the month of ORACLES deployment.

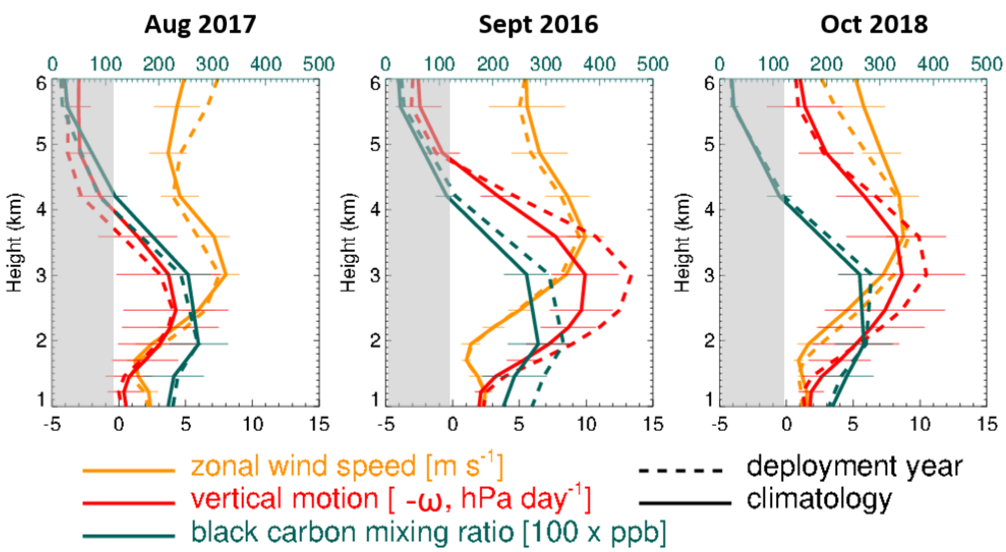

Figure R2. Vertical profiles of BC (green; averaged over 12-18E, 5-10S), vertical motion (red, omega multiplied by -1 to represent the positive (negative) value as the ascent (descent); averaged over 12-18E, 12-17S), and AEJ-S (orange; averaged over the jet exit region (12-18E, 5-10S) for August 2017, September 2016, and October 2018.

The thin horizontal line at each height level represents the 1.5 +/- standard deviation of the climatological mean. The gray shading refers to the descent regions. The gray shading represents the descent regions.

One interesting feature is that the peak mixing ratio occurs around 2-3 km, shown in Fig. R2 (green lines). The CAMS BC seems somewhat to follow the ascent motion (e.g. BC is higher when ascent is stronger in September 2016 than the climatological mean), but the overall pattern of them do not vary much during August - October. The vertical location of maximum values of BC do not change much over the ocean near the jet exit region as well (not shown). The original expectation was the BC/CO maximum will occur 3.5~4 km especially in September and October, around the level of AEJ-S. Shinozuka et al. (2020) and Doherty et al. (2021) both documented that the aerosols in models tend to have more of their mass located "lower" down than in the ORACLES observations, although their comparisons didn't include CAMS data. While more analysis needs to be done, this indicates the assimilation schemes in models still have room for improvement, in how they vertically allocate aerosol. We will include the summary of these plots along with further explanation in the part 1 of the revised manuscript.

Specific Comments

Abstract: in the introduction the authors state that "The goal of this study is to describe the meteorological factors that directly impact aerosols and low clouds, particularly stratocumulus decks during the ORACLES campaign". Therefore I believe that also the anomalies in the aerosol/cloud patterns originating different atmospheric conditions should be mentioned in the abstract.

=> Thank you for the suggestion. We agree with the reviewer. We will include the anomalous features of the low-cloud and aerosol compared to the climatology in the abstract of the part 1 of the revised manuscript.

Figures: anomalies should be presented along with assessment of statistical significance, qualitative assessment is not enough.

=> Thank you for pointing this out. We will describe the statistical assessment along with the plots as well in the revised manuscript.

L64: does the moisture gradient also play a role in the AEJ-S dynamics?

=> AEJ-S is a basically thermally driven circulation. A large meridional (cross-latitudinal) temperature gradient between hot, dry Namibian dryland and cool, moist Congo basin is attributed to the development and maintenance of AEJ-S.

However, the correlation between AEJ-S and inland *q* is small, indicating that inland moisture itself may be less likely to affect the formation and maintenance of the AEJ-S. This can be understood in the same context as Jackson et al. (2009), in which the AEJ-S may enhance convection through enhancing the vertical ascent at the jet entrance region, with the additional advected moisture acting to reduce the thermal contrast (Adebiyi and Zuidema, 2016). Nonetheless, whether the AEJ itself may result from convection still remains unclear, because some studies claimed that latent heat release is a factor in the development of the AEJ-N (Thorncroft and Blackburn, 1999). "

L72: in Lamb and Peppler 1992 I cannot find an explicit reference to SST in the Benguela region as related to Sahel rainfall variability, they rather describe a basin-wide influence of North and South Atlantic. This sentence should be modified or removed.

=> The first author apologizes for the misinterpretation of the Lamb and Peppler (1992) paper. Although there is no mention of SST in the Benguela region in the paper, the figures in the paper cover this region, and the region also seems to be included in the tropical Atlantic regions and associated with sub-Saharan rainy season variability. However, considering that this paper focuses on sub-Saharan West Africa, it is less relevant to the regions we are interested in, this reference will be removed as suggested by reviewers.

L162: any reference for the choice of the threshold at 230K?

=> The 230K threshold was chosen because this threshold shows a good estimate of convective rainfall. For example, Zuidema (2003) used 235K threshold because of a documented fit between area- and time-averaged rainfall and brightness temperature (Arkin, 1979), which perform well for most regions. Ohsawa et al. (2001) also obtained 230K as a good estimate of rainfall when performing regression analysis of rainfall gauge data and satellite data.

However, we did not use the brightness temperature data for the climatological overview, we will remove this description from part 1 of the revised manuscript.

L178-179: how D values are determined?

=> D is the height that the maximum planetary boundary layer over both land and ocean. During the deployment period based on the reanalysis data and model data, the decoupled boundary layer is not higher than 3km over the ocean and 6km over the land (typically, D is not higher than the cloud top heights). Furthermore, the aerosol plume height and the airmass trajectory over the south Atlantic region that we are interested in is well under about 6 km. This is why we set D as 6 km over the land and 3km over the ocean.

Furthermore, D values are introduced for detecting the cloud-topped BLH including decoupled stratocumulus layer using 6 hourly data for detailed investigation during the deployment month. However, we used the ERA5 monthly mean BLH data (which is calculated based on the bulk Richardson number) in part 1 of the revised manuscript. Thus, the methodology of the cloud-topped BLH detection will be removed in the part 1 of the revised manuscript.

Figure 2: what do histograms in a-c exactly display? Why is pressure altitude reported in km on the y-axes?

=> The (a-c) represents the distributions of aerosol top height (red), cloud top height (blue), and the separation between clouds and overlying aerosols (yellow) as a function of longitude, between 10-22.5S using CALIOP version 3 aerosol profile product from 2006 and 2012. These figures are reproduced from Redemann et al. (2021). “Pressure altitude” is meant to refer to the height converted from the pressure level since variables in reanalysis data are reported in isobaric surfaces (pressure levels). That is why we named it pressure altitude. We will change it into “Height (km)” in the part 1 of the revised manuscript.

L223: the AEJ-S anomaly in August 2017 looks like a vertical shift of the jet. This is rather unusual, why not to explore possible local/remote mechanisms explaining this feature?

=> Thank you for your comments. This was certainly an interesting feature. While we perceived its unique features and found that very interesting, we wanted to focus on the meteorological and climatological overview in this paper. However, following your suggestion, we decided to pursue for investigating more possible local and remote drivers in the August 2017 AEJ-S feature. As we described earlier, we think that mid-latitude upper-level disturbances play a large role in modulating the large-scale environment and thereby the AEJ-S. In order words, the position of upper-level disturbance induces an anomalous circulation, thereby changing the cross-latitude temperature gradient over the land, ultimately reducing AEJ-S in August 2017. This disturbance also intensifies the anticyclonic circulation on the right side of low PV, assisting in the development of anomalously high large-scale subsidence. This can also affect AEJ-S because the AEJ-S weakens when subsidence is strong, because the lower-level updraft is blocked by the subsidence (Adebiyi and Zuidema, 2016). These features are also consistent with Kuete et al. (2020). We will describe this more in detail with figures in the response letter below as well as in part 1 of the revised manuscript.

Figures 2 and 3: plotting individual year anomalies (with significance) would be also helpful.

=> We plotted the anomaly of zonal wind and RH at 700hPa for August and 600 hPa for September. We will also post part of this in part 1 of the revised manuscript and in the supplementary materials.

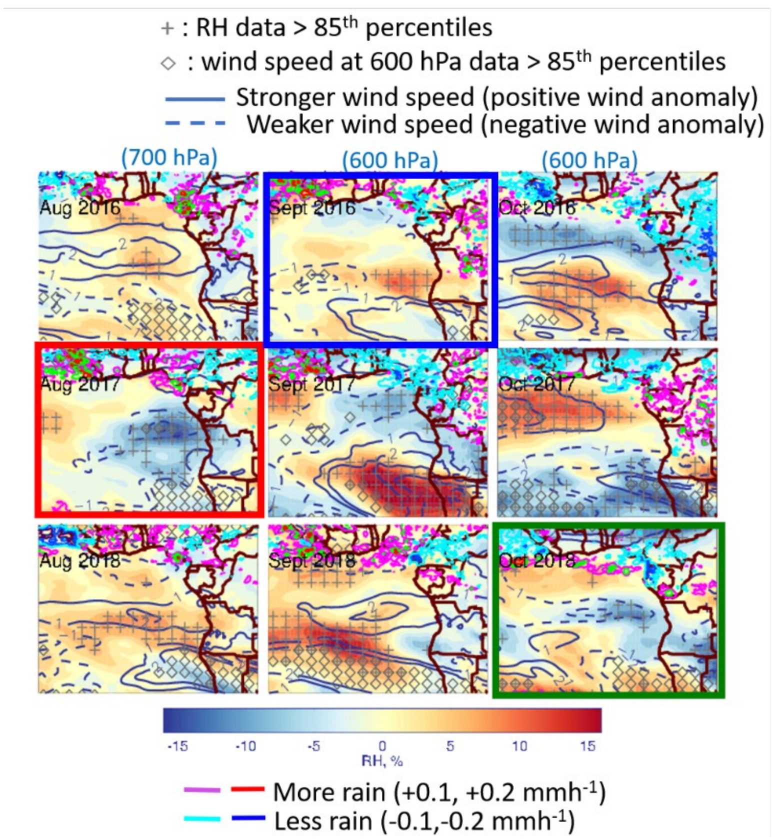

Figure R3. Map of the anomaly of horizontal wind speed, RH, and precipitation (contour, -0.2,-0.1,0.1,0.2 mm hr-1) at 700 hPa for August and at 600 hPa for September and October. The anomalies are computed by subtracting the monthly mean value from the climatological mean (2000–2018). The color boxes (red, blue, and green) indicate the month of ORACLES deployment. The gray diamond (cross) marks indicate the wind anomaly (RH anomaly) data is significant in 85% (90%) confidence level.

L251: how did you estimate the strength of recirculation?

=> We estimate the strength of recirculation by looking at the radius from the mid-level anticyclone driven by AEJ-S. The stronger the AEJ-S is, the larger the radius of the anticyclone is.

L254: the AEJ-S almost disappears in Aug 2017, the mechanism leading to this particular feature is worth to be further investigated.

=> As mentioned earlier, our primary focus in this paper is to provide the meteorological overview during the ORACLES deployment year. Thus, investigating the mechanism leading to the feature is NOT the primary focus. However, we agree that August 2017 has an interesting feature, and worthwhile to investigate it further. Thus, following your suggestion, we decided to investigate some possible local and remote drivers leading to the weak AEJ-S in August 2017. This is discussed on page 1 of this response, with more detail provided below.

Previous studies have shown that the primary cause of AEJ-S is the cross-latitudinal temperature gradient between the heat low over the Namibian dryland and the equatorial Congo basin. Thus, we focus on the factors that "disturb" this temperature gradient at synoptic scales. The proposed mechanism of modulating AEJ-S is the upper-level disturbances associated with the mid-latitude synoptic activity. We used Potential Vorticity (PV) at 250 hPa data, as a proxy. The PV data at 200 hPa do not show much difference as 250 hPa (with PV 250 hPa more variability). We found the upper-level disturbance is closely linked to the change in thickness (which is proportional to layer mean temperature, heat low over the continent), shown in Fig. R4.

Furthermore, the negative PV anomalies developed over the southeastern Atlantic tend to generate cyclonic circulation on the left side of high PV, bringing the oceanic air mass to the land and this can reduce the cross-latitudinal temperature gradient, ending up causing the reduction of AEJ-S. This disturbance also intensifies the anticyclonic circulation on the right side of low PV, leading to anomalously high large-scale subsidence (see Fig. R5). This can affect AEJ-S because the AEJ-S can also get weak when subsidence is strong because the updraft is blocked by the subsidence. These patterns are well observed in August 2017, and the phase of upper-level waves are consistent with the features shown by Kuete et al. (2020).

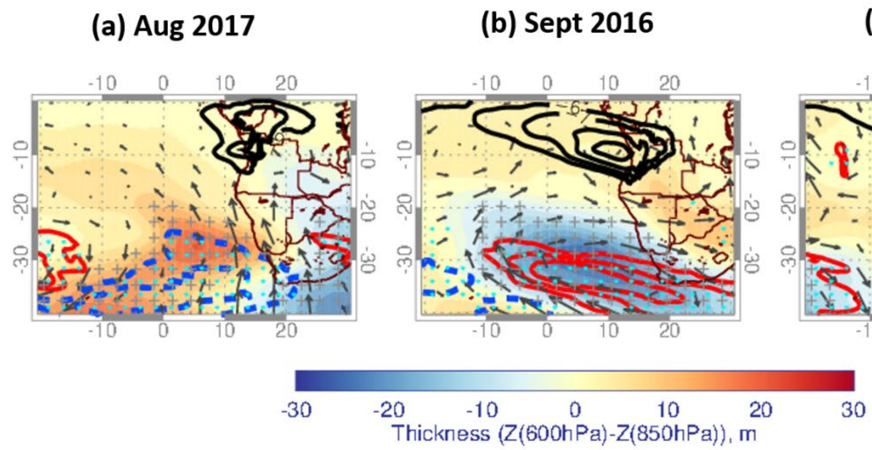

Figure R4. The PV anomaly at 250 hPa (blue dashed line: negative PV anomaly, red solid line: positive PV anomaly) and thickness anomaly (Z600hPa – Z850hPa, color shading) and the horizontal wind anomaly at 250 hPa (arrow) overlaid by the zonal wind at 600 hPa (black contour) during August 2017, September 2016, and October 2018. The cyan dot (gray cross) indicates data is significant in 85% confidence level for PV anomaly (thickness anomaly).

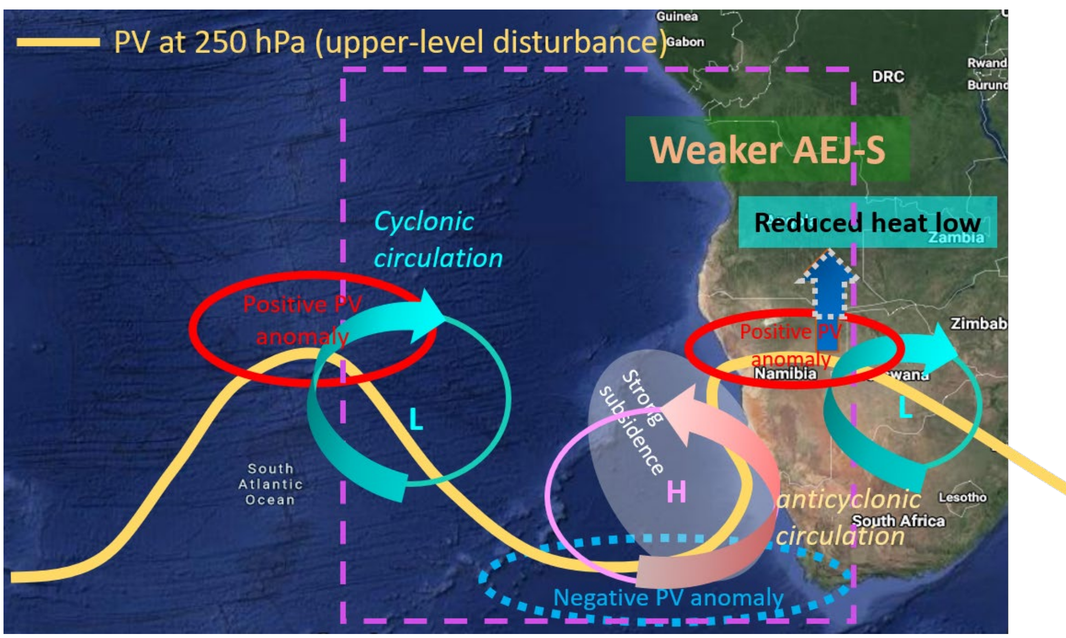

Figure R5. Schematics of the impact of the upper-level disturbance on the heat low and AEJ-S. The map shows the PV anomaly at 250 hPa (blue dashed line is negative PV anomaly and red solid line is positive PV anomaly) associated upper-level disturbance, leading to the change in local circulation, heat-low, and AEJ-S over the land. The cyclonic (anticyclonic) circulation represents the winds turning clockwise (anticlockwise) in SH associated with the phase of the upper-level PV.

We also found distinctive features in August 2017 based on the monthly mean data, such as 1) The large-scale subsidence is large compared to the climatological mean. 2) The temperature and moisture advection off the coast of 5-10S and 0-10E is weaker compared to the climatological mean, consistent with a weaker-than-average AEJ-S.

As a remote driver, we also found a weakening of the MJO convection over Africa in August 2017. Although more speculative, work focused on the AEJ-N (Ventrice and Thorncroft, 2013) suggests this too may weaken the AEJ-S.

However, both local and remote drivers for modulating AEJ-S have not much studied and still poorly understood, so further investigation on this using modeling and in-depth data analyses is needed that is beyond the scope of the current study.

L278-300: in discussing Fig. 4, please specify when you discuss Aug 2017, Sep 2016 and Oct 2018.

=> We apologize that we do not clearly specify which month when we discuss the individual month. We will carefully specify them in the revised manuscript.

L360-361: "correlation" word should not be used if correlation is not computed (Fig. 6 does not show correlations).

=> Sorry for the incorrect use of the wording. We did perform correlation analysis using 6 hourly data in the deployment month, which will be the part 2 of the manuscript, so the wording might come from that. We will carefully reword the paragraph related to Fig. 6.

L437: no "robust correlation" is shown in Fig. 8, see comment above.

=> This is a good point. Yes, we performed the correlation analysis, but we got high correlation for some deployment and very low correlation for some deployment, so we could not find the clear correlation during the deployment months.

L568: in Fig. 13, I cannot see the midlatitude weather system on 8 Sep 2016.

=> The mid-latitude (around 30 S) cyclone seems to be developed around September 24, 2016. And the large-scale anticyclone is dominant on 8 September 2016. We will correct that part. However, since we will focus on the climatological part in this revision, Fig. 13 will not be included in part 1 of the revised manuscript.

L753: Meridional wind is not displayed in Fig. 19, so how can we see "southeasterly winds"?

=> Thanks for catching this. The statement was based on both Fig 19d and Fig. 20(e, f). We will correct that part. However, since we will focus on the climatological part in this revision, Fig. 19 will not be included in part 1 of the revised manuscript.

Figure 24: is the seasonal cycle removed before computing daily correlations?

=> The monthly mean value is removed from the data before computing daily correlations. However, Fig. 24 will not be included in part 1 of the revised manuscript.

Technical corrections

L81: West African monsoon.

=> We corrected it.

L161: typo, brightness.

=> We corrected it.

L240: magenta dashed box region is not displayed in Fig. 3.

=> Sorry for the missing box region. However, since we do not include the precipitation histogram in the revised manuscript, the magenta dashed box will not be displayed. We will carefully work on the figure caption appropriately in part 1 of the revised manuscript.

L535: check punctuation.

=> Thanks. We will check it thoroughly in the revised manuscript.

Reference

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