

Referee comments (questions) are in blue

Author comments (replies) are in black

Modifications made in the new manuscript by the authors are in red

Displacements made in the new manuscript are in green

Replies to Referee #1

We sincerely thank anonymous referee #1 for their review which really helped to improve the quality of the paper by suggesting a much clearer structure and strengthening the confidence in the results (uncertainties on the wind and cloud fractions, significance test). Please, find below our responses to their comments.

General comment :

The manuscript by Titus et al. investigates the relationship between thin clouds, deep convective clouds and upper tropospheric wind structures over the Indian monsoon region using a synergistic cloud and wind profile dataset from Aeolus during the summer monsoon 2020 time period. They conclude that a higher occurrence of thin clouds over the Arabian Sea is associated with stronger westerly winds combined with higher amounts of water vapor injected in the upper troposphere by deep convection over India and the Bay of Bengal.

They further show that the strength of the TEJ is mainly consistent with thermal-wind balance, but that the short-term regional variability of the TEJ is modulated by the outflow and divergence, vorticity perturbations caused by deep convection.

While Aeolus's repeat cycle is usually 7-days, the authors benefit from the orbit configuration to have 12 hours between an ascending and descending orbit with spatial distances of 1150 km, which provides them the opportunity to study advection processes. This is unique for a single observing system.

The dataset is valuable and the topic is very relevant. The study addresses several interesting scientific implications, which are explained in detail. My only concerns are regarding the structural clarify in sections 3-5 plus a few minor comments.

Detailed comments:

The manuscript is clear and well written, the abstract, introduction and conclusion read very well and communicate the motivation and relevance of the study. However, I have several concerns regarding the structure.

While section 2 (Dataset and method) is very descriptive and easy to follow, sections 3, 4 and 5 present a mixture of observational findings (what does the data show us directly without interpretation), interpretations from these findings and hypothesis.

Also, the subsection “method” re-appears in section 5 for the individual hypothesis about deep convection and the strength of the TEJ.

The Methodology subsection (Sec. 5.1) is now moved to the Dataset and Method section (Sec. 2). We created a new subsection (2.4 “Thermal wind over the Arabian Sea derived from ERA5 temperatures”)

This causes that the narrative reads as a sequence of results, observations, impacts and hypothesis with no clear logical hierarchy. I recommend separating the observational results, interpretations, discussion and conclusions, either globally, or if you like to keep the hypothesis-driven sectioning, also per section.

In Section 3, we now describe observational results on annual and diurnal variations of clouds and winds, without giving any interpretation. We therefore removed L.185, and L.190 to L.195 of the previous manuscript.

Section 4 “Drivers of thin clouds over the Arabian Sea” (instead of “Thin high clouds over the Arabian Sea are advected by the Tropical Easterly Jet”) has a new structure, to separate more clearly hypotheses and results:

4.1 Hypotheses

4.2 Wind results

4.3 Temperature results

4.4 Deep convection results

4.5 Respective roles of the wind and deep convection

4.6 Implications in a warming climate

At the beginning of Section 4, we formulate clearly our whole set of hypotheses (H1, H2 and H3) that are investigated using observations in subsections 4.2, 4.3, 4.4 and 4.5:

Observational results of Sect. 3 suggest that deep convection mainly develops over the Bay of Bengal and the Indian continent at 18 LT (Fig. 4a), providing a source of water in the upper troposphere, that is advected westwards during 12 hours by the TEJ, which has a maximum intensity over the Arabian Sea at 06 LT (Fig. 4h). As cirrus clouds found over the Arabian Sea are not always correlated with opaque clouds found at the same place and at the same time (Fig. 4i), we can thus make the hypothesis that the water injected at the Bay of Bengal and the Indian continent at 18 LT and advected by the TEJ during the night is at the origin of the cirrus clouds found at 06 LT over the Arabian Sea (Fig. 4e). In this Section we investigate this hypothesis (noted H1) by testing if Aeolus thin cloud cover increases with Aeolus wind over the Arabian Sea at 06 LT (Sect. 4.2 and 4.5).

However, this single result would not allow us to conclude on the role of the wind in the formation of cirrus over the Arabian Sea. Indeed we need to consider two other hypotheses: (H2) negative temperature anomalies could occur when westward winds are strong over the Arabian Sea, and thus the presence of cirrus could be more explained by the thermodynamic conditions than by advection; (H3) convective clouds could develop preferentially during strong westward winds over the Arabian Sea, and thus the higher occurrences of cirrus would be rather explained by a larger source of water (vapour or ice crystals) in the upper troposphere, than by strong winds. Hypothesis H2 is investigated in Sect. 4.3, while hypothesis H3 is investigated in Sect. 4.4 and 4.5. In Sect. 4.6, we study the implications of our results in a warmer climate.

We remove L.197 to L.199, but we refer to Fig. 4 in L.202.

The lines mentioning our two hypotheses in the previous Subsection 4.2 (L.237 to L.241) are now removed. But references to hypotheses H1, H2 and H3 have been added to Subsections 4.2, 4.3, 4.4 and 4.5.

We also restructured Section 5, in the same spirit as Section 4. We first clearly say our hypothesis (H4) in Subsection 5.1, then we present our results in Sections 5.2, 5.3 and 5.4. The plan is thus:

5. Drivers of the Tropical Easterly Jet variations over the Arabian Sea

5.1 Hypothesis

5.2 Thermal wind balance

5.3 Deep convection patterns

5.4 Quantification of the wind perturbations created by deep convective patterns

Subsection 5.1 is now:

The goal of this section is to examine if and how the convective patterns locations influence the temporal variation of the mean TEJ speed (u_{allsky} around 15 km of altitude) over the Arabian Sea during SASM. In contrast to the studies of Ye et al., (2023) and Liu et al., (2024), our study does not focus on the specific location of the TEJ core and its interannual variability, but instead on TEJ speed variation at a much shorter time scale (typically a few days).

To do so, we proceed in three steps. We first verify that at first order, the TEJ is in thermal wind balance during SASM of 2020 (Sect. 5.2). Then, we make the hypothesis that short time scale fluctuations of the TEJ speed around the wind expected from thermal wind balance can be explained by the effects of deep convection patterns that act as perturbators (H4) and we test this hypothesis against observations (Sect. 5.3). Finally, we quantify the wind speed perturbations created by deep convection patterns and propose an explanation of the mechanism in play over the Indian region (Sect. 5.4).

All along this section we make use of the two following variables estimated over the Arabian Sea at 06 LT: the projection of the thermal wind vector along Aeolus LOS, noted $\Delta u_{\text{thermal}}$, ERA5 $\approx u_g(15 \text{ km}) - u_g(7 \text{ km})$ (see Sect. 2.3) and the wind difference observed by Aeolus at the same altitudes as the thermal wind, noted $\Delta u_{\text{allsky}} = u_{\text{allsky}}(z = 15 \text{ km}) - u_{\text{allsky}}(z = 7 \text{ km})$. We consider that the fluctuations of Δu_{allsky} around $\Delta u_{\text{thermal}}$, ERA5 are mostly due to $u_{\text{allsky}}(z = 15 \text{ km})$ rather than $u_{\text{allsky}}(z = 7 \text{ km})$ because the wind divergence and vorticity associated with deep convection are strongest at the top of convective towers (Diongue et al., 2002). ~~We also consider that $u_{\text{allsky}}(z = 7 \text{ km}) \approx u_g(z = 7 \text{ km})$.~~ Indeed, we consider that $u_{\text{allsky}}(z = 7 \text{ km}) \approx u_g(z = 7 \text{ km})$, and that the wind divergence and vorticity associated with deep convection (ageostrophic wind) are strongest at the top of convective towers (Diongue et al., 2002). As a consequence, differences between Δu_{allsky} and $\Delta u_{\text{thermal}}$, ERA5 ~~are due to changes in $u_{\text{allsky}}(z = 15 \text{ km})$, the TEJ~~ are due to the fluctuations of $u_{\text{allsky}}(z = 15 \text{ km})$ (i.e. the TEJ) around the geostrophic wind at the same altitude.

We now refer to Hypothesis H4 later in Subsection 5.3: “To test H4, we consider in Fig. 11...”

Section 5: Deep convection strengthens ... This is more like a conclusion or hypothesis. Maybe rephrase title to “Impact of deep convection on TEJ strength over Arabian Sea”?

We changed the title of Section 5 that previously sounded as a conclusion. In order to keep a similarity with the title of Section 4, we chose : “5. Drivers of the Tropical Easterly Jet variations over the Arabian Sea”

Section 4: I actually like the idea to already provide the conclusions with the title, but later, with 4.2.1 you refer to the role of temperature and 4.2.2. the role of opaque clouds. Maybe better call section 4: “Drivers affecting the occurrence of thin high clouds over the Arabian Sea”

Then you go to 4.1. wind, 4.2. temperature and 4.3 opaque high clouds over India ? This is just a suggestion to make the order clearer.

Ok, this is clearer. We selected pieces of suggestion from referee #2 as well and changed the titles as follows:

The title of Section 4 is now : “Drivers of thin cloud cover over the Arabian Sea”

Subsections 4.2, 4.3 and 4.4 are now referred as : “Wind results”, “Temperature results” and “Deep convection results”

The abstract is clear and well written, but you have some room for further details. You mention the Aeolus dataset, some results, numbers and implications, which is a nice structure for the abstract. One sentence about the methodology (disentangling the effects) would make it rounder.

Right, we added:

“In this study, we focus on disentangling the role of the wind in the formation of cirrus clouds over the Arabian Sea during the South-Asian Summer Monsoon (SASM), from the roles of temperature and convective intensity variations.”

The Sub-grid scale variability below the native resolution of Aeolus is not resolved by the merged and re-gridded cloud-wind dataset. You should add some sentence that this high-resolution dataset nicely compares to the resolution of CALIOP, but does not provide higher level of details compared to the native Aeolus resolution, which brings some uncertainties, especially with respect to the cloud top levels.

We added the information about the cloud fraction comparison between CALIOP and Aeolus in [Appendix A1 “Clouds”](#). We also added that we do not gain a higher level of details with our dataset compared to the native Aeolus resolution dataset:

“Aeolus cloud fractions were evaluated against CALIPSO-GOCCP observations coarsened to Aeolus spatial resolution and corrected for the different flight times of the two satellites (Titus et al., 2026). Aeolus cloud fractions profiles are lower than CALIPSO-GOCCP cloud fractions by 2% in the Tropical upper troposphere during boreal summer 2020, and this difference is non-significant at the 95th percentile with a two-sided T-test. Note however that our dataset, in spite of its 480 m vertical resolution, does not provide higher level of details compared to the native Aeolus resolution dataset, which brings some uncertainties, especially with respect to the cloud top levels.”

The manuscript reports convective perturbation wind components (u_{conv}) on the order of -1.7 to $+1.3$ m/s at the end of Section 5. However, no uncertainty estimate is provided for these values, nor is their magnitude discussed relative to the wind measurement uncertainty and derived thermal-wind uncertainty described in Titus et al. (2025). Without this context, it is difficult to assess whether the reported perturbations are robust and statistically distinguishable from dataset uncertainty. I recommend providing uncertainty estimates or at least add a reference of typical random errors of Aeolus Mie-cloudy wind and to compare them to the observed amplitude.

The typical random (and systematic) errors of Mie (and Rayleigh) winds are now given in [Appendix A2 “Winds”](#) :

“The systematic error on individual Aeolus wind observations in the most recent (2B16) baseline is reported to be below 1 m/s for Rayleigh-clear winds and 0.5 m/s for Mie-cloudy winds, while the random errors are about 6 m/s for Rayleigh-clear winds and 4 m/s for Mie-cloudy winds (Aeolus, 2024). Given the short span of the study (2020) and the single baseline used, we assume that these properties remained constant.”

Furthermore we performed a 2-sided Student test to show where the amplitude of the wind difference is statistically significant in Fig. 13.

We now mention that the 3 m/s amplitude of the wind difference is statistically significant:

“...so the total amplitude variation of the wind induced by the deep convective activity is about -3 m/s on average over the Arabian Sea (statistically significant at a 90% confidence level), and reaches up to -5 m/s in its center...”

Because we average u_{allsky} in different ways throughout the paper, we also provided a paragraph with estimates of the standard error on our mean winds, also in [Appendix A2 “Winds”](#)

“The different spatial or temporal averages of the wind that we perform throughout the paper naturally mitigate the impact of the large random error on individual Aeolus wind observations. The standard error is a good metric to determine the uncertainty on our mean wind observations. The standard error on the mean wind is equal to the random error divided by \sqrt{N} , N being the number of individual wind observations, weighted by the spatial cover of each observation. While we do not show for each figure the exact uncertainty on the mean wind observations, we quantify for each type of figure the maximum possible standard error in perfectly clear sky (the minimum possible standard error in perfectly cloudy sky), knowing that the real standard error is between these two values.

Figure 7: The mean winds result from a spatial average. The standard error on a daily mean wind value at a single altitude level is obtained after an average of 13 independent Rayleigh-clear (80 independent Mie-cloudy) wind observations between 12°N and 24°N, which brings down the standard error to 1.6 (0.4) m/s.

Figure 2: At each altitude level, the mean u_{allsky} is calculated from two ascending and two descending orbits, so the standard error is equal to 0.8 (0.2) m/s.

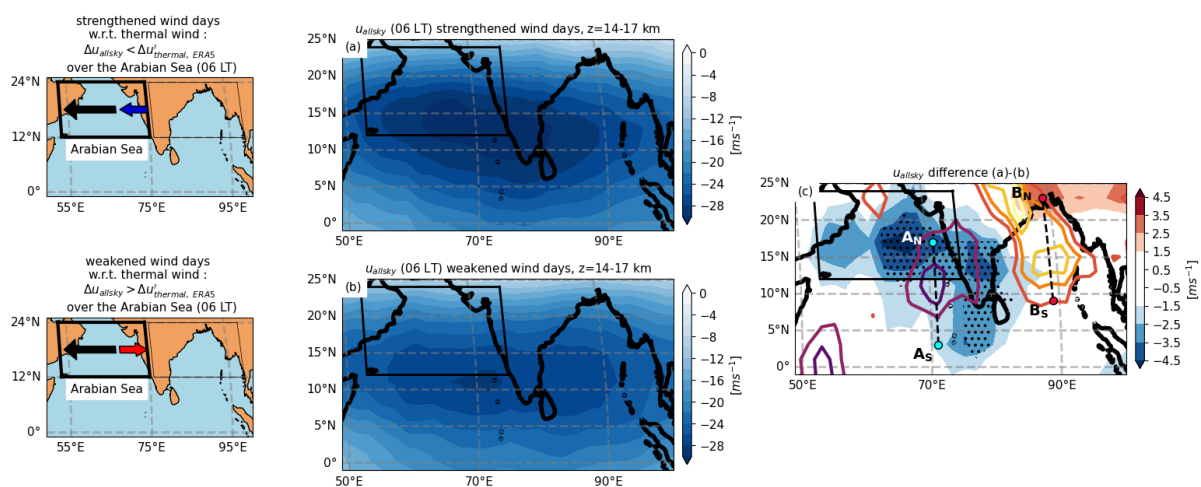
Figure 5: We perform an average along the vertical between 14 and 17 km of altitude. This brings down the maximum standard error to 0.9 (0.2) m/s.

Figure 10: Because we use the difference of two mean winds coming from two different altitudes, the standard error is 2.2 (0.6) m/s.

Figure 3: For maps, the mean wind results from a spatial average in each 2° x 2° grid-box followed by a temporal average. Thus the standard error is 0.9 (0.2) m/s.

Figure 11: The number of days used on each map is half that of Fig. 3. Thus the standard error is 1.2 (0.3) m/s.”

Additionally, we dotted in the new Fig. 13 the locations where the wind speed differences are significant at a 90% confidence level (two sided T-test), and the differences are statistically significant over the center and South-East Arabian Sea.



Minor technical comments:

- 1) L.24: “subject to change in a warming world” and “might affect the radiative effect of clouds”. Could you add some further details here? What is the hypothesis in the other papers? Is the variation between opaque and high clouds expected to warm or cool the planet?

Ok, we now provide more details. We removed the sentences “~~The balance between the size of the convective core and the anvil area in a deep convective cloud is subject to change in a warming world (Højgård-Olsen et al., 2022; McKim et al., 2024). This change should be evaluated as a variation in the ratio between opaque and thin high cloud covers might affect the radiative effect of these clouds (Hartmann and Berry, 2017; Gasparini et al., 2021).~~”

and replaced them with “**Within anvil clouds, the balance between the cover of the convective part of the cloud (good shortwave reflector) and the cover of the cirrus part of the cloud (longwave trap) leads to a neutral radiative effect (Ramanathan, 1989; Hartmann et al., 2017). However, observations revealed that with increasing sea surface temperatures, the opaque high cloud cover decreases, while the thin high cloud cover remains constant (Højgård-Olsen et al., 2022). Therefore, the anvil clouds could have a positive (warming) radiative effect in a warming world because of the imbalance between the opaque and thin high cloud covers (McKim et al., 2022).**”

- 2) L.37: “They concluded on the role...”. Please rephrase: The word role is not clear here. Does it mean that the advection of humidity is causing formation of cirrus? This is not what is currently written.

We reformulated: “~~Das et al., (2011) have noticed that during the period where both deep convection and TEJ are active, a larger cirrus cover is observed over the Indian region. They concluded on the role of advection of humidity by the TEJ through the cold temperatures of the upper troposphere in the formation of cirrus clouds over this region.~~”

to “**Das et al., (2011) have noticed a correlation between the TEJ at 150 hPa and the cirrus cloud cover during the SASM. While they observed that the TEJ spreads humidity over the Indian region, they do not explain if the cirrus clouds are simply detrained from deep convection or formed in-situ as cold temperature anomalies lead to the formation of ice crystals.**”

- 3) L.55: “TEJ is whether”. Do you mean “either”?

Yes! Corrected.

- 4) L.69: Here you mention the weakening of TEJ, similar to L.48.

As the weakening of the TEJ is previously mentioned L.48, we rewrite the sentence as :

“This is important considering the expected weakening of the TEJ in a warming climate (Rao et al. 2004; Huang et al. 2020), as well as the decrease in the convection over the Bay of Bengal (Rao et al. 2004).”

- 5) L.79: “is almost zonal”. Maybe rephrase that Aeolus is more favorable for measuring zonal winds instead of meridional due to the measurement geometry. A good reference for that is Krisch et al. 2022 (<https://doi.org/10.5194/amt-15-3465-2022>).

We did the rephrasing in the introduction and we added the reference to Krisch et al.

“Third, the TEJ is almost purely zonal (Mohan and Rao, 2016; Iqbal et al., 2017) and is thus well observed by Aeolus, as Aeolus is more favorable for measuring zonal winds instead of meridional winds due to the measurement geometry (Krisch et al. 2022).”

The expression “LOS is almost zonal” was also written in Sect. 2.2 when detailing the wind data of Aeolus”. We also made the change in this section, and specified that it is the horizontal projection of the wind along the LOS.

“However, ~~the LOS being almost zonal~~ at the latitudes considered in this paper, the angle between the **horizontal projection of the** LOS and the West-East direction is ~~comprised~~ between 8 and 10 deg. **Consequently**, Aeolus essentially retrieves the zonal component of the wind (Krisch et al., 2022). The relative difference between the zonal component of the wind and the **horizontal** projection of the wind along the LOS is about 1%”

and when deriving the thermal wind (Sect. 5.1 of the previous manuscript, and Sect. 2.4 of the new manuscript):

“and because ~~the horizontal projection is almost in the zonal direction~~ of the observation geometry...”

- 6) L.93: Please add some more comments that the 3 km by 480 m dataset is result of re-gridded data, while the native resolution of Aeolus is lower. I had to look in the other Titus et al. 2025 paper to understand how this was done. Maybe to mention here at least that this is re-gridded and that this means that the spatial sub-grid scale variability is not better resolved than for Aeolus.

Right, we now specify that “The clouds are detected at a spatial resolution of 3 km along the orbit track horizontally and 480 m vertically **after a linear interpolation of the signal along the vertical (the native vertical resolution of Aeolus is typically 1 km in the Tropical upper troposphere)**... More details are given in Appendix A1.”

- 7) L.177 “non-local convection origin”: Might it be that cirrus clouds also have a longer life time compared to deep convection and therefore might still be present at 6 UTC from deep convection from the day before? I mean as additional source to the non-local advected cirrus clouds.

Yes this longer life time of cirrus could be an additional source to the cirrus clouds having a non-local convection origin. We removed L.185 from our manuscript and we added: “**This indicates that a part of cirrus clouds over the Arabian Sea are not directly correlated to deep convective clouds observed at this location at the same time.**”

- 8) L.195: “orbit properties”: Change to orbit configuration.

Done.

9) L.217: Remove Furthermore.

Done.

10) L.220: What does the intersect around -15 to -20 m/s tells us? Is it potentially a break point for the relationship between thin cirrus occurrence and wind speed?

This is an interesting point that you raise... We investigated but did not find an explanation as to why all the PDFs intersect between -15 and -20 m/s.

11) L.230: In your two hypotheses, you consider temperature anomaly and water vapor availability. But what about sea surface temperature anomalies due to changing ocean currents – could this cause the available humidity?

We now reformulated our hypotheses (H1-H3) but did not evaluate the sensitivity of cloudiness to temperature anomalies due to changing ocean currents. While it would be a good thing to quantify, it may be out of the scope of this paper which focuses more on the sensitivity to atmospheric conditions in the upper troposphere.

12) L.250: “or India 12 hours before blue” > Something is wrong here. Please correct.

Yes, the word “blue” has been removed.

13) L.373: “Lemburg further showed”: Suggestion to change to: “showed that the wind perturbation lasted up to two days after DCS development”

The sentence L.373 has been changed following this suggestion.

14) L.407: “Thin high cloud fraction profiles...” This sentence just repeats the sentence before in other words. Thin clouds over Arabian sea increase when west wind increases...

The statement : “Thin high cloud fraction profiles larger than 8 % are almost never observed over the Arabian Sea for westward winds slower than 10 m/s“, is not a repetition of the previous sentence and adds new information. However, “[thin high cloud fraction profiles] are observed more frequently when the westward wind speed increases” is indeed a repetition.

We therefore rephrased the previous sentence (L.405) as :

“We noted that thin high cloud fraction profiles larger than 8 % are almost never observed over the Arabian Sea for westward winds slower than 10 ms⁻¹, and that the thin high cloud cover spatially averaged over the Arabian Sea at 06 LT increases from 6 % to 14 % when the westward wind speed over the Arabian Sea doubles (from 13.8 ms⁻¹ to 29.9 ms⁻¹).”

15) L.430ff: The conclusion references SSP scenarios (e.g., 5–8.5), but these are not introduced earlier in the manuscript. A brief explanatory sentence would help readers unfamiliar with the scenario framework. Although you are already referring to Huang et al. 2020, some further details about this scenario would help.

The references to SSP scenarios are now removed from the manuscript.

Replies to Referee #2

We sincerely thank anonymous referee #2 for their highly detailed review which really helped to improve the quality of the paper by suggesting a clearer structure, strengthening the confidence in the results (uncertainties on the wind and cloud fractions), and suggesting a few interesting additional references. Anonymous referee #2 also noticed incoherences in the number of days without Aeolus data which are now corrected. Please, find below our responses to their comments.

General comment :

This paper uses a dataset (introduced in a previous paper) that has co-located cloud and wind profiles. The lidar retrievals reveal a relationship between the Tropical Easterly Jet, convection, and cirrus clouds over the Arabian sea during the South-Asian Summer Monsoon (June-October 2020). Cirrus clouds are twice as likely to occur on mornings following a strong TEJ the evening before. The authors hypothesize a few mechanisms for the strengthening of the TEJ, concluding that deep convection is a major source of divergence from the thermal winds derived from theory. The time lag between events suggests a causal relationship on the frequency of occurrence of cirrus clouds.

I think there are good results here and I particularly appreciate the schematics for describing the complicated relationships and scenarios. However, the paper could be more concise and clearer with a different organizational structure. I like the proposed hypotheses in Section 4, but they are mentioned in Section 3 as well (but without context).

In Section 3, we now describe observational findings on annual and diurnal variations of clouds and winds, without giving any interpretation. We therefore removed L.185, and L.190 to L.195. This also contributes to making the paper more concise.

The authors can reorganize Sections 3 and 4 to lay out the theory (hypothesis) first then relate it to the evidence from Aeolus.

Section 3 is now dedicated to a description of observations (annual and diurnal variations of clouds and winds).

Section 4 “**Drivers of thin clouds over the Arabian Sea**” (instead of “~~Thin high clouds over the Arabian Sea are advected by the Tropical Easterly Jet~~”) has a new structure, to separate more clearly hypotheses and results:

4.1 Hypotheses

4.2 Wind results

4.3 Temperature results

4.4 Deep convection results

4.5 Respective roles of the wind and deep convection

4.6 Implications in a warming climate

At the beginning of Section 4, we formulate clearly our whole set of hypotheses (H1, H2 and H3) that are investigated using observations in subsections 4.2, 4.3 and 4.4:

Observational results of Sect. 3 suggest that deep convection mainly develops over the Bay of Bengal and the Indian continent at 18 LT (Fig. 4a), providing a source of water in the upper troposphere, that is advected westwards during 12 hours by the TEJ, which has a maximum intensity over the Arabian Sea at 06 LT (Fig. 4h). As cirrus clouds found over the Arabian Sea are not always correlated with opaque clouds found at the same place and at the same time (Fig. 4i), we can thus make the hypothesis that the water injected at the Bay of Bengal and the Indian continent at 18 LT and advected by the TEJ during the night is at the origin of the cirrus clouds found at 06 LT over the Arabian Sea (Fig. 4e). In this Section we investigate this hypothesis (noted H1) by testing if Aeolus thin cloud cover increases with Aeolus wind over the Arabian Sea at 06 LT (Sect. 4.2 and 4.5).

However, this single result would not allow us to conclude on the role of the wind in the formation of cirrus over the Arabian Sea. Indeed we need to consider two other hypotheses: (H2) negative temperature anomalies could occur when westward winds are strong over the Arabian Sea, and thus the presence of cirrus could be more explained by the thermodynamic conditions than by advection; (H3) convective clouds could develop preferentially during strong westward winds over the Arabian Sea, and thus the higher occurrences of cirrus would be rather explained by a larger source of water (vapour or ice crystals) in the upper troposphere, than by strong winds. Hypothesis H2 is investigated in Sect. 4.3, while hypothesis H3 is investigated in Sect. 4.4 and 4.5. In Sect. 4.6, we study the implications of our results in a warmer climate.

We remove L.197 to L.199, but we refer to Fig. 4 in L.202.

The lines mentioning our two hypotheses in previous Subsection 4.2 (L.237 to L.241) are now removed.

Section 5 also needs restructuring (moving the Methods subsection (Sec. 5.1) to the Methods and Dataset section (Sec. 2)).

The Methodology subsection of Sect. 5 (Sect. 5.1) is now moved to the Dataset and Method section (Sect. 2). We created a new subsection (2.4 “Thermal wind over the Arabian Sea derived from ERA5 temperatures”)

Then we copy below the previous lines L.304-L.336.

In Section 5, between L.302 and L.338, we add the sentence:

“From temperature reanalyses, we derive $\Delta u_{\text{thermal, ERA5}}(t) = u_g(15 \text{ km}) - u_g(7 \text{ km})$, defined as the projection along the LOS of the thermal wind vector, estimated between 7 km and 15 km over the Arabian Sea (see Sect. 2.4).”

We also restructured Section 5, in the same spirit as Section 4. We first clearly say our hypothesis (H4) in Subsection 5.1, then we present our results in Sections 5.2, 5.3 and 5.4. The plan is thus:

5. Drivers of the Tropical Easterly Jet variations over the Arabian Sea

5.1 Hypothesis

5.2 Thermal wind balance

5.3 Deep convection patterns

5.4 Quantification of the wind perturbations created by deep convective patterns

Subsection 5.1 is now:

The goal of this section is to examine if and how the convective patterns locations influence the temporal variation of the mean TEJ speed (u_{allsky} around 15 km of altitude) over the Arabian Sea during SASM. In contrast to the studies of Ye et al., (2023) and Liu et al., (2024), our study does not focus on the specific location of the TEJ core and its interannual variability, but instead on TEJ speed variation at a much shorter time scale (typically a few days).

To do so, we proceed in three steps. We first verify that at first order, the TEJ is in thermal wind balance during SASM of 2020 (Sect. 5.2). Then, we make the hypothesis that short time scale fluctuations of the TEJ speed around the wind expected from thermal wind balance can be explained by the effects of deep convection patterns that act as perturbators (H4) and we test this hypothesis against observations (Sect. 5.3). Finally, we quantify the wind speed perturbations created by deep convection patterns and propose an explanation of the mechanism in play over the Indian region (Sect. 5.4).

All along this section we make use of the two following variables estimated over the Arabian Sea at 06 LT: the projection of the thermal wind vector along Aeolus LOS, noted $\Delta u_{\text{thermal}}$, $\text{ERA5} \approx u_g(15 \text{ km}) - u_g(7 \text{ km})$ (see Sect. 2.3) and the wind difference observed by Aeolus at the same altitudes as the thermal wind, noted $\Delta u_{\text{allsky}} = u_{\text{allsky}}(z = 15 \text{ km}) - u_{\text{allsky}}(z = 7 \text{ km})$. We consider that the fluctuations of Δu_{allsky} around $\Delta u_{\text{thermal}}$, ERA5 are mostly due to $u_{\text{allsky}}(z = 15 \text{ km})$ rather than $u_{\text{allsky}}(z = 7 \text{ km})$ because the wind divergence and vorticity associated with deep convection are strongest at the top of convective towers (Diongue et al., 2002). ~~We also consider that $u_{\text{allsky}}(z = 7 \text{ km}) \approx u_g(z = 7 \text{ km})$.~~ Indeed, we consider that $u_{\text{allsky}}(z = 7 \text{ km}) \approx u_g(z = 7 \text{ km})$, and that the wind divergence and vorticity associated with deep convection (ageostrophic wind) are strongest at the top of convective towers (Diongue et al., 2002). As a consequence, differences between Δu_{allsky} and $\Delta u_{\text{thermal}}$, ERA5 are due to changes in $u_{\text{allsky}}(z = 15 \text{ km})$, the TEJ, are due to the fluctuations of $u_{\text{allsky}}(z = 15 \text{ km})$ (i.e. the TEJ) around the geostrophic wind at the same altitude.

We now refer to Hypothesis H4 later in Subsection 5.3: “To test H4, we consider in Fig. 11...”

There is a new idea introduced in the conclusions section (Sec. 6) on the impact of changing climate on this work, which should either be introduced as a theoretical implication of this work as TEJ is expected to decrease with warming or not be included in this paper at all (saved for future work).

We now dedicate the new Sect. 4.6 “Implications in a warming climate” to discuss the effect of the TEJ in a warming climate as recommended in your detailed comment on the conclusion:

“The TEJ shows a decreasing trend in the reanalyses during the period 1958–1998 (Rao et al., 2004) and the central TEJ intensity is expected to weaken by 18% over the Indian Ocean by the end of the 21st century \sout{under the shared socioeconomic pathway 5–8.5, according to CMIP simulations} (Huang et al., 2020). As the cirrus cover over the Arabian Sea mostly results from the advection of water vapour caused by a strong TEJ, one can thus expect a decrease of the cirrus cloud cover in this region in the future. ~~Assuming that the shape of the westward wind distribution over the Arabian Sea is conserved, but that its mean is reduced by 18%, the number of days with a westward wind slower than 10 m s^{-1} would increase from 17 (present) to 28 (end of century). If all other parameters (temperature, deep convection intensity and patterns) remained constant, this would then increase similarly the number of cirrus free days over the Arabian Sea. The hypothesis of other constant parameters seem nevertheless very unrealistic. Indeed,~~ In addition, (Rao et al., 2004) noticed over 40 years a decrease in the number of tropical cyclones over the Bay of Bengal, and by the end of our century, the CMIP simulations project a suppressed rainfall over the tropical eastern Indian Ocean (Huang et al., 2020). This trend in convection could thus amplify the decrease of the cirrus cloud cover over the Arabian Sea, because the deep convective sources of water vapour would be diminished.”

Introduction: Again, this section needs to be reorganized – introduce the TEJ and do literature review then state the purpose of this paper as it adds to the literature. There is already a wealth of literature on this, so you really need to be clear in what the novelty of this study is. The novelty is that it focuses on shorter timescales (intraseasonal) but that is also a limitation of this study (cannot generalize or create a climatology). The novelty is also in the use of the instrument data (though it is introduced in a previous paper) and the ability to show a time lag between deep convection over the Bay of Bengal with cirrus cloud fraction over the Arabian Sea 12 hours later.

We detail that the novelty is the use of instrumental data, whose advantages are spatial coverage, and reduced bias.

In this study, the novelty is the use of profiles of horizontal winds observed by ALADIN/Aeolus spaceborne Doppler Wind Lidar. The satellite covers the Arabian Sea, India, and the Bay of Bengal, allowing us to study wind-cloud observations beyond regions usually observed with radiosondes (Roja Raman et al., 2009; Mehta, 2024). Aeolus observations have the advantage of a lower wind bias compared to models or reanalysis in the Tropical upper troposphere, especially near regions with deep convection (Rennie et al., 2021). We make use of an Aeolus spaceborne Doppler Wind LIDAR dataset (Titus et al., 2026), in which the profiles of horizontal wind are co-located with profiles of thin and opaque clouds, which displays perfectly co-located observed profiles of clouds and profiles of horizontal winds.

We detail the advantage of the time lag between observations:

Second, observations occurring 12 hours later are shifted westward by 1150 km., and thus offer the opportunity to observe the cirrus that are potentially detrained from convective sources by the westward TEJ. This unique configuration is adequate to study processes with a time lag such as the advection of water vapour and cirrus clouds from the Bay of Bengal to the Arabian Sea, or the strengthening of the TEJ over the Arabian Sea following deep convection anomalies above India and the Bay of Bengal.

We detail that we focus on intraseasonal scale:

Furthermore, we evaluate how different deep convection patterns above the Indian region weaken or strengthen the zonal wind through divergence and vorticity of the convective outflow **on intraseasonal time scale**.

Statistics of the data: The scarcity of data is a bit concerning for drawing conclusions based on one year of data over a small area. I'm not sure that the conclusions can be generalized to multi-year or climatology or conjecture for changes with warming.

You are right, our conclusions on the extra 8 "cirrus-free" days on the Arabian Sea in case of a 18% weakening of the TEJ only stand true if the wind resembles that of 2020 but uniformly shifted in order to decrease its mean by 18%. We are unlikely to find this exact configuration and we cannot generalize it to a climatology which may have completely different winds. Therefore, we removed this quantitative analysis. We dedicated a section to the future climate: **Sect. 4.6 "Implications in a warming climate"**.

Additionally, in lines 343-344, the total number of days does not match the breakdown of fast/slow days even considering the days for which there are no Aeolus measurements; this discrepancy needs to be addressed.

The days were miscounted. There are 68 (not 69) days with strengthened wind, 69 days with weakened wind, and 16 (not 7) days without Aeolus data. $68+69+16$ corresponds well to the total of 153 days during the considered period extending from 2020-06-01 to 2020-10-31. You will find a more detailed response below the detailed comment.

There are some interesting results throughout the paper, but the presentation was a bit confusing. It felt like a data paper and then a theory paper, but it should be combined so that the data supports the theory. In some instances, the authors are present clear evidence and explanation of the phenomena, but other times, the purpose of a certain figure/paragraph is not clear until much later and is distracting from the main results that cirrus clouds over the Arabian sea are influenced by both the TEJ and deep convection to the east.

We keep one section dedicated to the thin clouds variation over the Arabian Sea (Sect. 4), and another section for the strengthening / weakening of the TEJ induced by deep convection (Sect. 5). However, we now explicitly separate in both of these sections the hypotheses from the results. In the same way, the data and methods are now all gathered in Section 2 and Section 3 only describes annual and diurnal variations of clouds and winds, without giving any interpretation.

Detailed comments:

Terminology:

- I think you should define cirrus = thin clouds and anvil clouds = thick clouds from Aeolus, then stick to using “cirrus” and “anvil cloud” respectively. This will make everything more easily interpretable.

Right, we made this change all along the manuscript. In the beginning, we still mention the equivalence deep convective cloud <-> opaque cloud and cirrus cloud <-> thin cloud.

“At its top, the cloud spreads out horizontally away from the convective core (optically opaque), to form an anvil cloud (Betts, 1973; Ackerman et al., 1988). The edge of the anvil cloud (optically thin), can be detrained...”

Note that Fig. 2 which previously covered the altitude range from the surface to 18 km is now limited from 8 km to 18 km so that all the thin clouds can be considered as cirrus clouds.

Finally, we changed the title of the paper “Relationships between ~~Thin clouds, Opaque Clouds,~~ Deep Convective clouds, Cirrus clouds, and the Tropical Easterly Jet over the Indian Region observed with Aeolus Spaceborne Doppler Wind Lidar”.

- Similarly, you could move away from HCFopaque and HCCthin to deep convection/anvil cloud fraction and cirrus cloud cover.

Done

Formatting:

- Percent sign should come directly after the number (1% not 1 %)

Ok this is now corrected.

Lines 28-35: This paragraph should be moved later in the intro. Make it clear this is a result of your study (“We hypothesize...” etc.). Maybe combine with the paragraph starting with “The aim of this study is thus to investigate the relationship between...”

We slightly modified lines 28-35:

~~“In this study, we focus on the Indian region and~~ During the South Asian Summer Monsoon (SASM), the Indian region is subject to ~~characterized by a strong~~ deep convection over the Indian continent and the Bay of Bengal, and at the same time ~~characterized~~ by a fast westward wind in the upper troposphere reaching more than 30 m/s, the Tropical Easterly Jet (TEJ). ~~This region is the theatre of a subtle interplay between temperature, deep convection, the TEJ and the cirrus cover: the temperature contrast controls the TEJ speed via the thermal wind balance, and the TEJ speed is also controlled by, or controls, the occurrence and locations of deep convection. Deep convection acts as a source of water vapour in the upper troposphere which forms cirrus clouds that may extend over large distances depending on the speed of the TEJ and temperature conditions. In turn, the cirrus cloud cover may~~

affect temperatures of the upper troposphere by its radiative effect (Fu et al., 2018). Das et al., (2011)...”

and merged them with the paragraph:

“The aim of this study is thus to investigate the relationships between ~~thin clouds~~ **cirrus clouds**, ~~opaque clouds~~ **deep convective clouds** and the TEJ over the Indian region during the SASM. ~~This is particularly important as in a warming climate, a weakening of the TEJ is expected (Rao et al., 2004; Huang et al., 2020), as well as a decrease in the convection intensity over the Bay of Bengal (Rao et al., 2004).~~ **While** the temperature contrast controls the TEJ speed via the thermal wind balance, the TEJ speed is also controlled by, or controls, the occurrence and locations of deep convection. Deep convection **could act** as a source of water vapour in the upper troposphere which forms cirrus clouds that may extend over large distances depending on the speed of the TEJ and temperature conditions. In turn, the cirrus cloud cover may affect temperatures of the upper troposphere by its radiative effect (Fu et al 2018). These processes are important considering the expected weakening of the TEJ in a warming climate (Rao et al., 2004; Huang et al., 2020), as well as the decrease in the convection over the Bay of Bengal (Rao et al., 2004). These changes are likely to impact the cirrus cloud cover over the Indian region in the future.”

Lines 38-39: This reminds me of a paper – Holton and Gettleman (2001): Horizontal transport and dehydration of the stratosphere.

Thank you for the reference, although lines 38-39 of the previous manuscript were revised, we added the reference to Holton and Gettelman (2001):

“**While they observed that the TEJ spreads humidity over the Indian region, they do not explain if the cirrus clouds are simply detrained from deep convection or formed in-situ as cold temperature anomalies lead to the formation of ice crystals (Holton and Gettelman, 2001).**”

Lines 53-54: Ye et al. (2023) focused on June, not July

We corrected “July” to “June”.

Line 91: What is the time period and spatial coverage of this dataset? You go into more detail in Section 4 (Figure 4), so maybe move section 4 (Lines 189-201) to this data and methods section.

We now mention the time and spatial coverage of this dataset earlier in the paper:

“In this study, we make use of a global wind and cloud profiles dataset observed with Aeolus (Titus et al., 2026) **which covers the period from 1 January 2020 to 31 December 2020. For each day of the year 2020, the Bay of Bengal and the Arabian Sea are overflowed by Aeolus at least once at 06 LT and once at 18 LT.**”

Line 91: Reference to Titus et al., 2025 should be updated to actual paper, not the preprint

We now reference the published version of this paper (Titus et al., 2026) instead of the preprint.

Line 103: Do you ever distinguish between profiles 2 and 3 or are they all binned together? It seems like profile #3 would be thin cirrus with underlying low clouds - perhaps no meaningful difference between these two profiles

No, we do not distinguish between profiles #2 and #3 which are both considered “thin”. Profile #3 is simply there to highlight that even though it contains fully attenuated bins, as there are also clear sky bins below the uppermost cloudy layer, the profile is considered thin.

Section 3.1 Title might be better described as “Properties of the TEJ and clouds in 2020”

Ok, title of Sect. 3.1 changed to “**Properties of the Tropical Easterly Jet and clouds in 2020**”

Fig 2: Is this a domain-average over the Indian region?

Yes, we now mention in the caption of Fig. 2 that this is a domain-average over the Indian Region:

“...**One profile corresponds to the daily average (of 06 and 18 LT orbits) and the domain-average of u_{allsky} over the Indian Region for each day of 2020...**”

Line 139-142: Do you see this anticyclonic circulation and upper tropospheric thermal gradient in SASM 2020?

We do not observe directly the anticyclonic circulation, and at this stage of the paper, we cannot state that our observations demonstrate that the TEJ is in thermal wind balance. We only know its speed. Therefore we removed lines 139-142. and we added : “**this is the TEJ**”. This also contributes to make Sect. 3 more concise and purely observational.

Section 3.2: This section could be more concise. The main take away of the section is that there is a strong diurnal contrast in deep convection, while the contrast in cirrus clouds is not as large. The key point here is the time lag between deep convection (18LT) and cirrus (6LT), which leads into your hypotheses in Section 4.

We made this section 3.2 more concise by removing the interpretation of the observations. We therefore removed L.185, and L.190 to L.195 of the previous manuscript.

At the beginning of Section 4, we sum-up our observation results of Section 3 that lead into our Hypotheses H1, H2 and H3:

“**Observational results of Sect. 3 suggest that deep convection mainly develops over the Bay of Bengal and the Indian continent at 18 LT (Fig. 3a), providing a source of water in the upper troposphere, that is advected westwards during 12 hours by the TEJ, which has a maximum intensity over the Arabian Sea at 06 LT (Fig. 3h). As cirrus clouds found over the Arabian Sea are not always correlated with opaque clouds found at the same place and at the same time (Fig. 3i), we can thus make the hypothesis that...**”

Line 158-162: This is a confusing statement. Kripalani et al., 2022 said that 2020 was an anomalous "erratic" monsoon year, yet you find that it is in "good agreement" with the typical diurnal cycle of convection? Can you really make this statement with the limited data that you have?

This sentence especially stresses that despite 2020 being an anomalous year, the diurnal contrast of deep convective clouds is similar to the one observed by Zuidema (2003). We rely on about 6 months of data with two passes of Aeolus at 06 LT and two passes at 18 LT over the entire Indian region while Zuidema's study relies on 2x5 months of geostationary data. We believe that we have enough Aeolus data to document the diurnal contrast of deep convection.

Line 162: The Arabian sea received less attention than the BoB considering deep convection in the literature? And what is your point? The rest of this paragraph is about the Arabian sea, not the Bay of Bengal which would have made more contextual sense after this sentence.

Ok, we removed the sentence "The Arabian Sea received less attention than the Bay of Bengal when it comes to deep convection."

Line 164-166: Do you see evidence of the diurnal cycle of SST driving deep convection in your results? You could look at SST in reanalysis during this time period.

Because it is a bit out of topic in this study, we did not go deeper into the analysis between the diurnal cycle of the SST and deep convection. Therefore, we removed the sentences which focused on the diurnal cycle of deep convection over specific parts of the Arabian Sea and added a reference to the paper of Chepfer et al., (2019):

Over the Arabian Sea, we observe a diurnal contrast of the **deep convective cloud cover**, which is about 5% larger at 18 LT than at 06 LT over the entire Arabian Sea (Fig. 3c), **consistent with Chepfer et al., (2019). However, this diurnal contrast is weak compared to the one observed over the Indian continent. This difference increases to about 12% on the South-East of the Arabian Sea (10°N, 65°E, Fig. 3c), most likely due to a large diurnal cycle of the sea surface temperature (Chen and Houze Jr, 1997. Indeed, the SST at this location can warm up by around 2 K during a single day between 04 LT and 16 LT between the months of June to September, as observed by Shenoi et al., (2009). Over the North-East Arabian Sea (20°N, 65°E), *HCCopaque* is the same (about 15%), at both 18 LT (Fig. 3a) and 06 LT (Fig. 3b).**

This makes the Sect. 3.2 more concise as requested by referee #1.

Line 174: Where is your evidence for this conversion from thick to thin clouds overnight?

We now mention the paper of **Feofilov & Stubenrauch (2019)**, which describes this phenomenon above India.

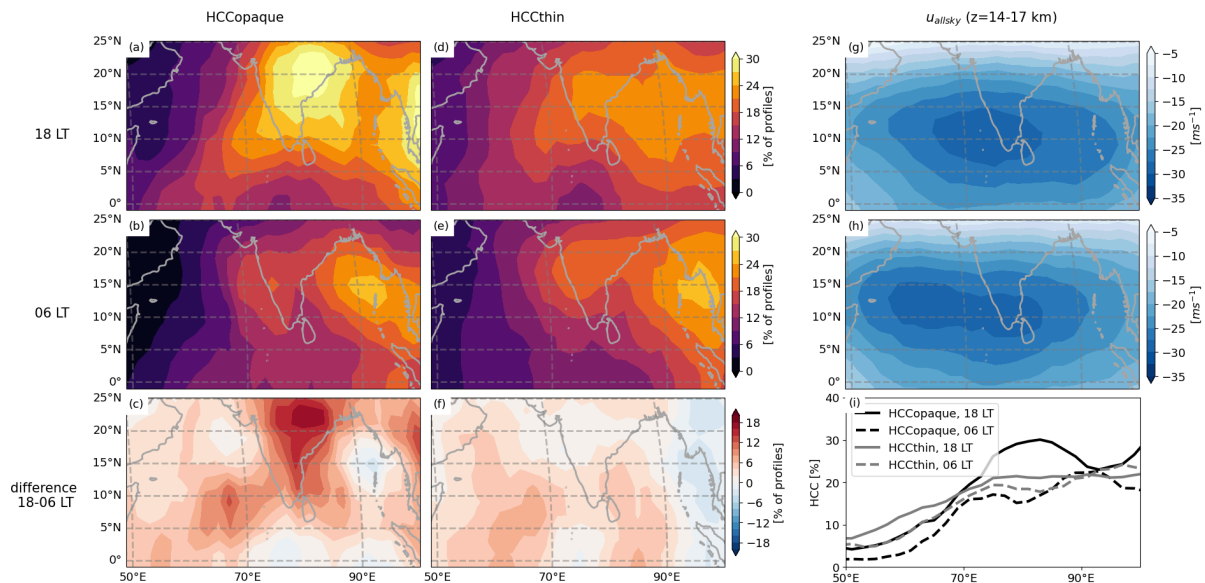
Line 175-77: How do you reconcile this non-local convective origin with the sentence above that thick cloud are converted to thin cloud during the night... isn't that still a local effect since they are in the same region? Or else, what do you mean by "non-local convective origin" - do you mean in time or space? This is an interesting concept (origin of cirrus clouds) but I don't think you can make this argument here without evidence. Instead, you could propose these two mechanisms for cirrus cloud formation and leave it to future work to quantify/prove which hypothesis is true.

The "non-local convective origin" sentence was confusing and is now removed. We meant non-local origin spatially. Instead of affirming that the thin clouds have a "non-local convective origin", we

simply comment on the correlations between thin and opaque high cloud covers in Sect. 3.2, but we try to explain them later in Sect. 4 after formulating hypotheses H1, H2, and H3.

Line 177-179: You have space for one more subplot in Fig 3 so you could show the zonal mean across longitude in this region for each type of cloud (4 lines on one plot). Then you don't have to list the specific values, it will be clear from the plot.

Yes, we added the (i) subplot on the new Fig. 3 and removed the suggested text.



Line 180: Yang et al. (2010) - Is this the right citation? I don't see any mention of longitudinal extent of cirrus in this paper on contrail cirrus...

No, this is not an appropriate citation, we removed it...

Line 180: Das et al (2001; Fig. 8) shows a peak in the cirrus cloud occurrence and zonal wind around 90E whereas you are looking at 70E.

... and we also removed the entire sentence “~~This longitudinal extension of thin clouds was also reported by Yang et al. (2010) and Das et al. (2011).~~”

Line 184: the ratio of HCcthin and HCCopaque is really hard to tell by eye from the plots. I suggest a supplemental/appendix figure that plots the ratio.

New subplot (i) on Fig. 3 is actually nice to directly show the ratio of cirrus to deep convective cloud cover at a given longitude. We added a reference to Fig. 3i in the following sentence: “**The ratio of cirrus to deep convective cloud covers both measured over the Arabian Sea is much larger at 06 LT than at 18 LT, especially eastwards of 65°E (Fig. 3i)**”.

Line 185-6: This hypothesis of the source of water vapor coming from deep convection then advecting westward, should be saved for later when you discuss the hypothesis in Section 4. I think some of this argument could just be moved to that section.

Ok, this paragraph was removed, and the hypotheses are now written in Sect. 4.

Section 4.1 & Figure 5 could be moved to an appendix/supplemental material to make the narrative clearer.

In this case, we preferred to keep these elements in the main sections of the manuscript. Pieces of Sect. 4.1 of the previous manuscript are in Sect 4.2 of the new manuscript, however, they contain some key results. In order to keep a consistency between Sect 4. and Sect. 5, we also preferred to keep the time series of Fig. 5 in Sect. 4.2, which shows when and for how long the winds are considered fast/slow.

Section 4.2 title could be “Drivers of cirrus cloud cover” instead, more concise

Section 4.2.1 title could be “Hypothesis 1: Temperature anomalies drive changes in cirrus clouds cover”

Section 4.2.2 title could be “Hypothesis 2: Deep convection drives changes in cirrus cloud cover”

We renamed Sect. 4 “**Drivers of thin clouds over the Arabian Sea**” (instead of “~~Thin high clouds over the Arabian Sea are advected by the Tropical Easterly Jet~~”). Section 4 has now a new structure, to separate more clearly hypotheses and results. We formulate all the hypotheses in Sect 4.1 and test them in Sect. 4.2, 4.3, 4.4 and 4.5:

4.1 Hypotheses

4.2 Wind results

4.3 Temperature results

4.4 Deep convection results

4.5 Respective roles of the wind and deep convection

4.6 Implications in a warming climate

Section 5.1 needs to be moved to the methods section introducing the ERA reanalysis dataset as well as describing the analysis involved here.

Sect. 5.1 was moved to the method section (Sect. 2.4 “**Thermal wind over the Arabian Sea derived from ERA5 temperatures**”) and we introduce the ERA5 reanalysis dataset at the beginning of Sect. 2.4:

“We compute the thermal wind at 06 LT along each Aeolus orbit in the Arabian Sea domain. To do so, **we use temperature profiles from ECMWF ERA5 reanalyses (Hersbach et al., 2020)**. We first compute...”

Lines 242-245: This could be a bit more complicated since the presence of thin clouds can actually feedback on the temperature and act to warm the TTL (Fu et al., 2018 - doi:10.3390/atmos9100377). So you might not actually see cirrus associated with lower temperatures in the data. Additionally,

temperature still has an important role in the formation of cirrus clouds in the TTL, temperature could also be impacted by gravity waves from convection. Finally, what are the uncertainties/variability in the temperature data over Aeolus track? And what are the uncertainties in cloud fraction/winds in Aeolus? This question of uncertainty should be addressed in the Methods and Data section.

You are right. Thank you for the reference. What we indeed see is that contrary to the wind, the temperatures averaged on an entire region show no correlation with the thin high (cirrus) cloud cover. However, the cirrus clouds may originate from gravity waves and/or warm their environment locally. Because both these phenomena are local, they could be masked when we perform the spatial average over one region. We adapted the new Sect. 4.3:

“This shows that large cirrus cloud fractions are not necessarily associated with low values of temperatures averaged above the Arabian Sea, and conversely low values of cirrus cloud fractions are not associated with particularly high values of temperatures. Therefore we do not observe a significant dependency of the cirrus cloud fraction on the mean temperatures experienced over the Arabian Sea between 14-17 km, and we can thus discard our hypothesis H2. Note however that these mean temperatures from the reanalyses could be affected by the presence of cirrus, that would have formed after having encountered colder temperatures. Indeed cirrus clouds can warm the upper troposphere locally by up to 4 K (Fu et al., 2018). In addition, these regionally averaged temperatures may hide small-scale processes. Cold temperature anomalies induced by gravity waves can form cirrus clouds (Corcos et al., 2023) but they are not captured in the regional mean temperatures.”

And we detailed in the caption of Fig. 7 that in the PDF, “One occurrence corresponds to the variable averaged along 12° of longitude between 12°N and 24°N (about 1330 km) along the track of Aeolus, at 480 m vertical resolution between 14 and 17 km of altitude, on a given day (6 values per day corresponding to the 6 vertical 480 m layers between 14 and 17 km of altitude).”

We added the information about the cloud fraction uncertainties in Aeolus in Appendix A1 “Clouds”:

“Aeolus cloud fractions were evaluated against CALIPSO-GOCCP observations coarsened to Aeolus spatial resolution and corrected for the different flight times of the two satellites (Titus et al., 2026). Aeolus cloud fractions profiles are lower than CALIPSO-GOCCP cloud fractions by 2% in the Tropical upper troposphere during boreal summer 2020, and this difference is non-significant at the 95th percentile with a two-sided T-test. Note however that our dataset, in spite of its 480 m vertical resolution, does not provide higher level of details compared to the native Aeolus resolution dataset, which brings some uncertainties, especially with respect to the cloud top levels.”

We added the information about the wind uncertainties in Appendix A2 “Winds”:

“The systematic error on individual Aeolus wind observations in the most recent (2B16) baseline is reported to be below 1 m/s for Rayleigh-clear winds and 0.5 m/s for Mie-cloudy winds, while the random errors are about 6 m/s for Rayleigh-clear winds and 4 m/s for Mie-cloudy winds (Aeolus, 2024). Given the short span of the study (2020) and the single baseline used, we assume that these properties remained constant.”

Because we average u_{allsky} in different ways throughout the paper, we also provided a paragraph with estimates of the standard on our mean winds, also in Appendix A2 “Winds”:

“The different spatial or temporal averages of the wind that we perform throughout the paper naturally mitigate the impact of the large random error on individual Aeolus wind observations. The standard error is a good metric to determine the uncertainty on our mean wind observations. The standard error on the mean wind is equal to the random error divided by \sqrt{N} , N being the number of individual wind observations, weighted by the spatial cover of each observation. While we do not show for each figure the exact uncertainty on the mean wind observations, we quantify for each type of figure the maximum possible standard error in perfectly clear sky (the minimum possible standard error in perfectly cloudy sky), knowing that the real standard error is between these two values.

Figure 7: The mean winds result from a spatial average. The standard error on a daily mean wind value at a single altitude level is obtained after an average of 13 independent Rayleigh-clear (80 independent Mie-cloudy) wind observations between 12°N and 24°N, which brings down the standard error to 1.6 (0.4) m/s.

Figure 2: At each altitude level, the mean u_{allsky} is calculated from two ascending and two descending orbits, so the standard error is equal to 0.8 (0.2) m/s.

Figure 5: We perform an average along the vertical between 14 and 17 km of altitude. This brings down the maximum standard error to 0.9 (0.2) m/s.

Figure 10: Because we use the difference of two mean winds coming from two different altitudes, the standard error is 2.2 (0.6) m/s.

Figure 3: For maps, the mean wind results from a spatial average in each 2° x 2° grid-box followed by a temporal average. Thus the standard error is 0.9 (0.2) m/s.

Figure 11: The number of days used on each map is half that of Fig. 3. Thus the standard error is 1.2 (0.3) m/s.”

Finally, we added information about the variability of ERA5 temperatures along the orbit track of Aeolus in the new [Appendix A3](#).

Line 252-253: I really like this calculation! Does this mean that this analysis comparing the BoB at 18 LT and AS at 6 LT is only valid for "fast wind" days since slow wind days would take longer than 12 hours to advect that distance?

Technically yes, comparing the BoB at 18 LT and AS at 6 LT is only valid for westward winds of about 25 m/s, in which case what is observed over the AS at 6 LT is related to what is observed over the BoB 12 hours before, at 18 LT.

Figure 8 gives a good overview of the situation above the BoB at 18 LT regardless of if the winds are “fast” or “slow”. Therefore, we just moved the calculation of Lines 252-253 before commenting Fig. 7d and refined our commentary of Fig. 7d in Sect. 4.5:

é... We conclude that higher occurrences of cirrus over the Arabian Sea may be explained by a combination of faster winds and larger sources of water vapour injected in the upper troposphere over India and the Bay of Bengal. **Note however that comparing the Arabian Sea at 06 LT and the Bay of**

Bengal 12 hours before, at 18 LT makes sense if the wind speed is sufficient to advect air parcels from the Bay of Bengal to the Arabian Sea.”

We refined our commentary of Fig. 7d in Sect. 4.5:

“...is ignored. If thin clouds are detrained from the deep convective opaque clouds over Bay of Bengal at 18 LT, and advected at about 25 m/s westward, they are observed with Aeolus 12 hours later over the Arabian Sea, which could explain why the mode of the PDF of $u_{\text{allsky}}(\text{AS}, 06 \text{ LT})$ for the largest quartile of α ($\alpha > 0.35$) is located at about -25 m/s (Fig. 7d). For slower westward $u_{\text{allsky}}(\text{AS}, 06 \text{ LT})$, thin clouds detrained from deep convection over the Bay of Bengal do not make it to the Arabian Sea before the pass of Aeolus, which could explain why $\alpha < 0.03$ is dominant, especially for westward $u_{\text{allsky}}(\text{AS}, 06 \text{ LT})$ slower than 20 m/s. In contrast, for westward $u_{\text{allsky}}(\text{AS}, 06 \text{ LT})$ faster than 25 m/s, if thin clouds leave a trail of ice crystals behind them, it is still possible to observe it with Aeolus over the Arabian Sea at 06 LT although part of the cloud has traveled westward further than the orbit track. This could explain why $\alpha > 0.03$ are more often observed for westward $u_{\text{allsky}}(\text{AS}, 06 \text{ LT})$ faster than 25 m/s than $\alpha < 0.03$. Therefore...”

The cirrus advected by slow winds from a deep convective cloud observed by Aeolus at 18 LT along the Bay of Bengal orbit track may exist over the Arabian Sea but are not observable by Aeolus (whose orbit is too far westward). Conversely, some deep convective clouds may exist at 18 LT over the Indian continent but are not observable by Aeolus because located westward of the Bay of Bengal orbit track. Those could generate cirrus that are still observed by Aeolus at 06 LT over the Arabian Sea, in case of a weak wind. However, observations show that the cirrus cover over the Arabian Sea is much smaller for weak wind days compared to strong wind days. We thus believe that the wind plays a determinant role in the presence of cirrus over the Arabian Sea.

Line 264: Yes, the source of water vapor in the upper troposphere from convection is important. Have you also considered that thin clouds are directly formed via outflow from convection and persist for 12 hours or more? Cirrus clouds may have long lifetimes in favorable conditions.

You raise a good point, in fact, because of low temporal resolution of Aeolus (06 LT and 18 LT) we cannot know if the extra cirrus that observe over the Arabian Sea are formed in-situ after the advection of humidity by the wind, or if the cloud itself is advected.

We now mention it: “Thus we cannot exclude that the higher occurrences of thin clouds during these fast wind days are partly due to an increase in water vapour sources in the upper troposphere, transported upward by convection as suggested in Das et al., (2011), or to a larger portion of anvil clouds originating from India or the Bay of Bengal and advected towards the Arabian Sea”

This was not detailed in Sect. 4.4 so we also clarified:

“We deduce that the thin clouds found over the Arabian Sea result from their advection ~~over large distances~~, or from the advection of water vapour towards the Arabian Sea.”

Line 284: “HCFthin(AS, 06 LT) with respect to HCFopaque(BoB, 18 LT)” a (you defined alpha for a reason, so use it to simplify your explanations)

Right, we did the following change: “This increase of $HCF_{thin}(AS, 06-LT)$ with respect to the $HCF_{opaque}(BoB, 18-LT)$ in α is...”

Figure 9: I like this schematic – could you somehow include the transport of water vapor as well as the simple convective outflow of clouds?

We now include these elements in the new Fig. 9 and extended the caption with:

“The blue shading highlights the supply of water vapour in the upper-troposphere fed by the deep convection while the black arrows denote the convective outflow.”

Line 334: the median value is -2.1 m/s but how sensitive are your results to this value – I assume it would be different for a different year of data. What is the standard deviation? mean? Maybe a scatter plot of u_{allsky} vs $u_{thermal}$ would be informative (or a joint histogram). The method for calculating this cloud also be moved to the methods section.

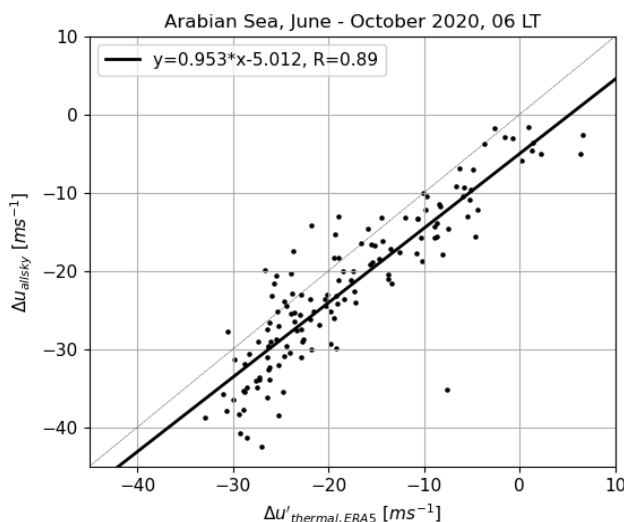
Indeed, for a different year of data, this median would probably be different.

We now specify that “During the SASM of 2020, the median...”

We tested the sensitivity of our results by computing the thermal wind $u_{thermal}$ over different domains (0-12°N and 24°N-36°N instead of 9°N-12°N and 24°N-27°N), which gave a larger median difference (-8.0 instead of -2.1 m/s) between u_{allsky} and $u_{thermal}$, but led to similar conclusions.

We give below the scatterplot of u_{allsky} vs $u_{thermal}$ with the best linear regression along with the mean difference of $u_{allsky} - u_{thermal}$ (-2.1 m/s), and the standard deviation of the differences (4.5 m/s) in the caption of the new Fig. B2. Note that the values displayed on this scatterplot are the same as within the time series (Fig. 10), therefore, the figure below is moved to the Appendix B2

“Complements relative to ERA5 temperatures and the thermal wind”:

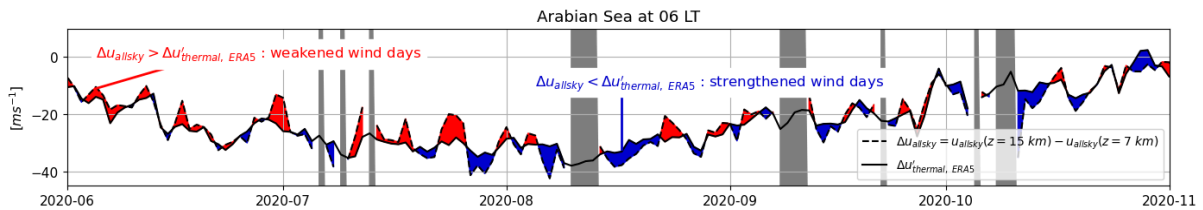


Lines 343-344: Something is not adding up... 153 days total - 7 days without Aeolus obs = 146 days with data - (69+69 days strengthen/weakened) = 8 days unaccounted for. Are these days the exact same value as $u_{thermal}$? What is the prevision with which you can determine equality?

Correct, the days were miscounted. The numbers are corrected in the paper. This corresponds well to the total of 153 days during the considered period extending from 2020-06-01 to 2020-10-31.

“From June to October 2020, there are exactly 6968 days where $\Delta u_{\text{allsky}} < \Delta u_{\text{thermal, ERA5}}$, 69 days where $\Delta u_{\text{allsky}} > \Delta u_{\text{thermal, ERA5}}$, and 716 days without Aeolus observations (Fig. 10)”

Days without data are now shaded in gray in the new Fig. 10.



Line 345-346: Is there previous work that supports your analysis that deviations from the thermal winds are due to deep convection? Please add some citations here.

The specific lines 345-346 in the previous manuscript were removed because they were affirmative and we provided no references:

~~“The fluctuations of Δu_{allsky} around $\Delta u_{\text{thermal}}$ can be explained by the effects of deep convection patterns on Δu_{allsky} , that act as perturbations on the first order approximation of Δu_{allsky} .”~~

We now formulate it as a hypothesis in Sect 5.1:

“we make the hypothesis that short time scale fluctuations of the TEJ speed around the wind expected from thermal wind balance can be explained by the effects of deep convection patterns that act as perturbators (H4) and we test this hypothesis against observations (Sect. 5.3).”

Further we write:

“Indeed, we consider that $u_{\text{allsky}}(z = 7 \text{ km}) \approx u_{\text{g}}(z = 7 \text{ km})$, and that the wind divergence and vorticity associated with deep convection (ageostrophic wind) are strongest at the top of convective towers (Diongue et al., 2002). As a consequence, differences between Δu_{allsky} and $\Delta u_{\text{thermal, ERA5}}$ are due to the fluctuations of $u_{\text{allsky}}(z = 15 \text{ km})$ (i.e. the TEJ) around the geostrophic wind at the same altitude.”

We did not find any reference about deviations from the thermal wind due to deep convection. However, according to the literature, the TEJ is at the first order in thermal wind balance, so we now formulate the hypothesis (Sect. 5.1) of strengthening/weakening by taking the thermal wind as a reference of the unperturbed wind difference between 7 and 15 km.

The circulations associated with deep convective patterns are divergent, thus non-geostrophic, whereas the thermal wind is a difference between geostrophic winds. Huang et al., (2020) suggest that the divergent/convergent patterns around areas with enhanced/suppressed convections explain a part of the weakening or strengthening of the TEJ. Chen (1982) (mentioned in the introduction)

suggested that the TEJ is energetically maintained by the release of available potential energy by the tropical divergent circulation associated with both the North-South (Hadley) and the East-West (Walker) circulations. Some studies like Lemburg et al., (2019) use one case study of a deep convective system and observe a zonal wind anomaly around it at t , $t+1$ day, $t+2$ days. Other studies like Liu et al., (2024) split their dataset in years with convection in North-West / South-East in order to evaluate the interannual variability of the TEJ.

Therefore, making the hypothesis that deviations from the thermal wind are due to deep convection seems reasonable to us, although we cannot exclude that waves and/or the ageostrophic wind created by TEJ acceleration and distortion can also cause perturbations of the thermal wind. The results displayed in Section 5.3 support our hypothesis.

Figure 12: There is a small difference in the u_{thermal} for each scenario – is this difference significant? Does this suggest that the deep convection is feeding back on surface temperatures which impacts the thermal winds (an indirect effect of convection)?

Correct, there is a small difference between the u_{thermal} averaged on strengthened (-19.6 m/s) vs weakened (-20.5) wind days. Using a two-sided T-test, we showed that this 0.9 m/s difference is non-significant (p-value = 0.51). We added to the caption of Fig. 12:

“The 0.9 m/s difference in the mean u_{thermal} between weakened and strengthened days is not significant at a 90% confidence level.”

Section 6 (Conclusions):

This feels like it is just a set up for your next paper which will use these results as a baseline for your CMIP model results. If that is the case, this paper could be reduced to the relevant figures and kept as a separate paper, or it could be modified to suit your needs for the next paper.

However, I do feel like the schematics in particular could be a nice contribution to literature from this paper and that the observations support these "hypotheses" that you've presented.

Line 409-410: I would reframe this; cirrus clouds act to warm their local environment which may complicate these results...

You are right, we now clarify:

We do not observe a significant dependency of thin high cloud fractions on the mean temperatures over the Arabian sea, therefore the thermodynamic conditions ~~are not the~~ do not seem to be a limiting factor controlling the formation of cirrus over the Arabian Sea during SASM, although the mean temperature over the Arabian Sea may be affected by the upper tropopause warming effect of cirrus clouds.

Lines 412-414: Earlier you also mentioned water vapor brought to the UTLS by convection then advected (forming cirrus later) which is different than cirrus detrained directly from convection and advecting hundreds of kilometers. This separation is not clear here.

Yes, actually, it is impossible, using simply this dataset, to know if the cloud is actually advected or formed in-situ, after the humidity is advected. We now say:

“We conclude that the thin clouds found over the Arabian Sea result from their detrainment and advection over large distances, **or from the large-scale advection of water vapour towards the Arabian Sea as suggested in Das et al., (2011).**”

Line 424: Lemburg et al. (2019) and Diongue et al. (2002), right?

Correct, we now cite Diongue et al., (2002) along with Lemburg et al., (2019).

Lines 424-425: Did you actually show that $u_{\text{allsky}}(\text{BoB } 18 \text{ LT, } 12 \text{ hours before})$ did not have an anomaly associated the TEJ at 18 LT? Like Fig 11 but for $u_{\text{allsky}}(\text{BoB } 18 \text{ LT, } 12 \text{ hours before})$ rather than $\text{HCCopaque}(\text{BoB } 18 \text{ LT, } 12 \text{ hours before})$.

In the literature, some studies find that anomalies in the TEJ intensity follow anomalies in deep convection drive with a time lag (Lemburg et al., 2019) while other studies find that the deep convection follows specific wind conditions (Rao et al., 2004, verifier). These are opposite cause-consequences relationships. In our case we want to highlight that while we were able to verify the first one, we cannot conclude anything about the second. We now mention:

“**This conclusion is not incompatible with the existence of a reversal process involving the increase of deep convection caused by the increase in the TEJ intensity or the wind shear, via baroclinic instability (Rao et al., 2024). However this process was not investigated in our study.**”

Lines 430-445: This whole paragraph should have its own section in the results "Implications for future warming". This is a nice back of the envelope calculation but it should not be only introduced and discussed in the conclusion section. This is a huge pivot from the focus of the rest of the paper, so it should be introduced earlier.

You are right, we moved these lines into a new **Sect. 4.6 “Implications in a warming climate”**, and removed the quantitative analysis which was a bit of a stretch.

The TEJ shows a decreasing trend in the reanalyses during the period 1958–1998 (Rao et al., 2004) and the central TEJ intensity is expected to weaken by 18% over the Indian Ocean by the end of the 21st century ~~under the shared socioeconomic pathway 5–8.5, according to CMIP simulations (Huang et al., 2020)~~. As the cirrus cover over the Arabian Sea mostly results from the advection of water caused by a strong TEJ, one can thus expect a decrease of the cirrus cloud cover in this region in the future. ~~Assuming that the shape of the westward wind distribution over the Arabian Sea is conserved, but that its mean is reduced by 18%, the number of days with a westward wind slower than 10 m/s would increase from 17 (present) to 28 (end of century). If all other parameters (temperature, deep convection intensity and patterns) remained constant, this would then increase similarly the number of cirrus-free days over the Arabian Sea. The hypothesis of other constant parameters seem nevertheless very unrealistic. Indeed,~~ **In addition**, Rao et al., (2004) noticed over 40 years a decrease in the number of tropical cyclones over the Bay of Bengal, and by the end of our century, the CMIP simulations project a suppressed rainfall over the tropical eastern Indian Ocean (Huang et al., 2020).

This trend in convection could thus amplify the decrease of the cirrus cloud cover over the Arabian Sea, because the deep convective sources of water would be diminished.

Lines 446-448: This would be a cool paper but this and the previous paragraph could go in the intro to that paper, but the conclusion section to this paper should focus on your actual results, relevance/novelty to existing literature, and future work that could build on your work or support your hypothesis. For example, it could be relevant to note that a dataset with higher temporal resolution could better show the evolution in time from triggering of deep convection over the BoB as it moves towards the Arabian sea.

True, we now removed Lines 430-445 from the conclusion. but now limit the conclusion to actual results. We still kept two sentences at the end of the conclusion to motivate the study in a warming climate.

We detail that having additional observations in between 18 LT and 06 LT would be relevant to follow the evolution of deep convection from the Bay of Bengal towards the Arabian Sea:

“Because Aeolus overflies the Arabian Sea at 06 LT and at 18 LT only, we cannot conclude on the advected water phase (ice crystals or water vapour). This would require additional observations in-between these 06 LT and 18 LT to properly track the evolution of deep convection.”

Minor technical comments:

Lines 42, 44, etc.: Capitalize all instances of East-West/North-South to be consistent. Check throughout the paper.

East-West and North-South capitalized.

Lines 54-55: This sentence doesn't make sense grammatically. Suggested edit:

Liu et al (2024) further showed that this longitudinal variability also has a latitudinal component, particularly in July, that changes with convective activity in the Arabian sea. They demonstrated that a strengthening of deep convection in the Arabian Sea and a suppression of convection in the Bay of Bengal drive the northwest shift in the core of the TEJ.

We accepted this suggestion and added this to the introduction

Line 75: delete “perfectly” as it is an extraneous adjective

Done.

Line 81: replace “shortly” with “briefly”

Done.

Line 90: delete comma after winds

Done.

Line 103: delete “entire single” – extraneous

“Entire” was deleted in line 103 and in line 105 but “single” was kept to stay coherent with the expression “single profile”.

Line 109: “resp” Change this and next few paragraphs to use this layout:

gridded opaque (thin) cloud fraction profiles, CFopaque (CFthin).... among "opaque" ("thin") profiles... etc. Keep order of thick (thin) consistent throughout.

We adopted this layout here, and also for the high cloud fraction, cloud covers, and removed “resp.” when detailing that positive (negative) winds are eastward (westward) in Sect. 2.2.

Line 130: delete “comprised”

Done.

Line 170: “about” “above”

“About” changed to “above”

Line 184: Would make more sense as "Indeed, strong winds push further into the Arabian sea at 6 LT (Fig. 3g & h)"

Suggestion accepted.

Figure 7d: the caption says alpha but the subplot legend says f. Keep it consistent or explain the difference between alpha and f

Correct, the subplot legend should be indeed α , not f. In the new Fig. 7, we corrected to α .

Line 261: It is “associated with” not “associated to”

We now say “associated with”.

Line 262: Add reference to Fig. 7c.

Done.

Lines 278-280: You don’t need to explain this since it is evident from the equation.

We left these sentences for clarity purposes.

Line 285: “and then seems to form a plateau” “and alpha seems to plateau”

Corrected.

Line 366: I think you are refer to panel c in Fig. 11. Change reference to Fig. 11c

Indeed, reference changed from Fig. 11 to 11c

Fig. 11: I think the schematics to the left of panel a and b should also be explained in Figure caption. They are nice intuition for the concepts here.

Ok, we added in the caption of Fig. 11: “The insets on the left of the figure denote the situation where the thermal wind (black arrow) is strengthened (blue arrow) or weakened (red arrow).”

Figure 381: “counter acted” counteracted. One word.

Corrected.

Line 396: typo in number format. Should be period, not comma: -1.7 m/s.

Comma replaced by period.

Line 419: “the temperatures, and these days were associated to” “their temperature gradients, which were associated with”

Suggestion accepted.

Figure 12 caption:

(1) Add “proposed” before mechanisms in the first line.

Done.

(2) the “average” situation might be called something else instead. If most days have u_{thermal} not equal to u_{allsky} (69 days strengthened and 69 weakened), then this “average” state is not so average. Perhaps this scenario is "in thermal wind balance"?

(3) The shading is gray, not brown.

Corrected brown to gray.

Lines 389-390: Maybe A_N , A_S and B_N , B_S would make more sense here (points A and B or Arabian sea and Bay of Bengal are easier to remember than P and Q).

Correct, we replaced P and Q with A and B respectively.

Line 426-end of paragraph: Rephrase from “Additionally...” “Liu et al. (2024) showed that this dipole in deep convection explained the interannual longitudinal variability of the TEJ core; however, here we show that this dipole modulates the TEJ on much shorter time scales (on the order of days).”

We accepted this suggestion.