Response to the reviewers: Synoptic and regional-scale meteorological controls of stratus altitude in the Namib Desert

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We thank the three anoymous reviewers for their careful reviews of the manuscript, their constructive criticism and overall positive evaluation. We have added the sentence: “We thank three anonymous reviewers for their valuable comments.” in the acknowledgements.

Reviewer 1

General comments

In their study, the authors explore the effect of regional- and synoptical-scale impacts on fog and low clouds in the Namib Desert. They show that while fog conditions are related to high pressure over continental southern Africa in combination with the regional-scale impact of thermotopographic circulation, low cloud situations are associated with a deeper marine boundary layer due to stronger heat fluxes over the Atlantic oceans, which stems from a stronger Atlantic High. The manuscript is logically structured, well-written, and fits into the scope of ACP . I have some remarks regarding the physical mechanism behind the mountain-plain wind and some recommendations on how one could better tease out the regional-scale and synoptical-scale signals. The manuscript merits publication once my concerns are addressed.

We thank the reviewer for the positive evaluation and respond to the individual points below. In particular the comments on scale separation, the Pdiff feature, and the physical mechanism of the mountain-plain wind, prompted related conceptual refinements that we believe substantially strengthen the manuscript and for which we are grateful. We now (i) distinguish more explicitly in the text between the two physically distinct easterly off-Escarpment flows: a shallow surface-based cold katabatic mountain-plain wind (depth ∼1000 m AGL, maximum at 250–500 m AGL at Gobabeb (Lindesay and Tyson, 1990) and a deeper, synoptically forced subsiding easterly that warms adiabatically (bergwind) under ridging continental high conditions, and (ii) make explicit that only the warm, dry synoptically forced flow can cap the marine boundary layer from above, while the colder katabatic drainage is expected to mix with the MBL near the surface rather than cap it (Seely and Henschel, 1998). The inversion-lowering signal in our composites is therefore most plausibly attributable to the strengthened Atlantic High and the resulting upstream surface heat fluxes that determine the MBL depth. The deep cap-from-above mechanism is demonstrated in the case study, where the bergwind is fully developed, but there is no evidence for this being a dominant process in the composites. These refinements appear in a new framing paragraph in the Introduction (Sec. 1), in the rewritten mechanism paragraph and the new "note on scale separation" paragraph in Sec. 4.1, in the case-study transition in Sec. 4.2, and in the third bullet of the Conclusions. Detailed responses to each individual comment follow in blue below.

Specific comments

As you stated in the manuscript, both synoptical- and regional-scale effects can influence the occurrence of low clouds/fog. To better distinguish between the two, one could analyze the geostrophic (synoptic, calculated from geopotential) and ageostrophic (regional, difference to modeled wind) wind components and relate them to the occurrence of low clouds/fog. One could then use them for the wind arrows in the vertical cross sections (e.g., Fig 4 a-f, Fig. 7).

We thank the reviewer for this thoughtful suggestion. We agree that separating the synoptic and regional contributions to the wind field would be valuable in principle. After careful consideration, however, we have concluded that the proposed geostrophic/ageostrophic decomposition is not the right tool in our setting, for three reasons. First, the geostrophic approximation (the assumption that the wind is set by a balance between the pressure-gradient force and the Coriolis force) does not hold well in the region of interest. At the horizontal scale of the Great Escarpment slope, the flow's inertia becomes as important as the Coriolis force, and friction near the surface would add a further departure from geostrophic balance. The difference between the actual wind and the geostrophic wind is therefore not a clean "thermotopographic residual": it also contains friction and flow blocking by the terrain, and these contributions cannot be disentangled. Second, ERA5's 0.25° horizontal resolution does not well resolve the Great Escarpment. Pressure gradients computed across only one or two grid points in this region are noisy, and that noise would carry directly into the geostrophic wind estimate. Third, the synoptic and thermotopographic contributions are not physically independent. The same continental high-pressure regime that creates the large-scale east–west pressure gradient also suppresses daytime convective cloud cover over the elevated interior, which allows stronger nocturnal radiative cooling and so strengthens the local katabatic drainage. The two scales are physically coupled, and a mathematical decomposition cannot reflect this, it would risk creating the misleading impression of a clean causal separation that the underlying physics does not support. Instead, we have substantially revised Section 4.1 to address the reviewer's underlying concern. We now distinguish more explicitly between (i) the synoptically forced subsiding easterly bergwind-type, and (ii) the surface-based cold katabatic drainage, locally enhanced by the same high-pressure regime. We believe this revised framing addresses the reviewer's concern more directly and more honestly than the formal decomposition would have. Concretely, Section 4.1 of the revised manuscript now contains: (i) a substantially rewritten mechanism paragraph that distinguishes between the warm, dry, adiabatically heated synoptically forced flow (which can cap the MBL from above) and the cold surface katabatic drainage component (which arrives colder than the MBL and is expected to mix with it rather than cap it (Seely and Henschel, 1998)), and (ii) a new dedicated 'note on scale separation' paragraph that summarises the physical coupling and explains why a cap-from-above inversion-lowering signal can only be produced by the synoptic component (even though it is not the dominant process in the composites).

Besides the regional signal in the pressure gradient Pdiff, there could also be synoptical-scale pressure gradients between the ocean and the continent, which are superimposed on the Pdiff. Why not subtract the large-scale pressure gradient to isolate the pressure gradient caused by thermotopographic circulation?

We thank the reviewer for raising this important point. We fully agree that Pdiff as defined contains contributions from both a local thermotopographic gradient and a superimposed synoptic-scale gradient, and this was not stated clearly enough and discussed enough in the manuscript. After careful consideration we have nevertheless chosen not to subtract a synoptic component, for reasons that partly overlap with those given in our reply to Comment 1: the synoptic and thermotopographic contributions are physically coupled through the continental high-pressure regime, so a mathematical decomposition would not reflect a clean separation of distinct physical mechanisms (Sect. 4.1, new paragraph on scale coupling). Two further considerations apply specifically to Pdiff. First, any such decomposition requires an arbitrary choice of e.g. spatial averaging boxes for the "large-scale" gradient, and the resulting "regional residual" is the small difference of two large numbers, which amplifies noise in the resulting feature. Second, as a feature in the logistic regression, Pdiff captures the local east–west pressure gradient that physically causes the near-coastal flow and encapsulates both gradients originating from the synoptic high and from the regional thermal contrast, and removing one component would weaken the feature without making it more interpretable.

We do take the reviewer's concern seriously and made three important corresponding revisions. (i) The interpretation of Pdiff is now stated more carefully and precisely in Section 3.2 and the discussion: Pdiff is presented as a local pressure-gradient diagnostic, with the explicit caveat that its value cannot be uniquely attributed to a single scale of forcing. (ii) We have removed the feature-importance ranking from Sect. 4.3 and added a new Fig. 5 (with associated discussion at the end of Sect. 4.1; the figure is included as Fig. 1 in this response) that shows the relationships between Pdiff, the continental high CH, the 950 hPa zonal wind at CM, and the offshore boundary layer height, separately for fog and low-cloud events. The moderate negative correlation between CH and Pdiff (r = -0.43) directly illustrates the physical coupling between the synoptic and regional pressure signals. The strong correlation between Pdiff and the offshore boundary layer height (r = 0.62) connects the continental pressure pattern to the MBL depth that ultimately controls the stratus altitude. This new figure simultaneously addresses Reviewer 3's request for quantitative support of the feature relationships (Sect. 4.3) and Reviewer 2's request for a relational analysis (see also our replies there). (iii) Section 4.1 includes a new paragraph (see also our response to Comment 1) discussing the inherent coupling between synoptic and thermotopographic drivers, which makes clear to the reader why scale-clean features are intrinsically difficult to construct in this region.

L11, P235: The authors discuss that elevated continental pressure in fog situations prevents convective mixing in the Namib regions, which helps to establish a temperature gradient between the Great Escarpment and the plains. Firstly, I wonder whether it is the reduced mixing over the Namib or rather in the mountainous areas of the Great Escarpment and the high plains east of it where the cooling is actually taking place. Secondly, you are looking at the early morning, so convective mixing will only play a minor role during that time of day. I would rather argue that during the fog season, residual convective cloud cover over the Great Escarpment is minimal, as little convective clouds form during the day. This consequently leads to stronger radiative cooling during the night compared to the more convective austral summer, causing stronger katabatic/mountain-plain winds. We thank the author for this comment - we agree that this is a more physically consistent interpretation. In the updated version of the manuscript, we now state: "*Locally over the elevated interior, the higher continental pressure and stronger subsidence in fog cases is likely to suppress daytime convective cloud cover, which enhances overnight longwave radiative cooling of the elevated terrain, and thereby strengthens the shallow surface katabatic drainage component that can mix with, undercut, or block the marine air mass near the coast (Lindesay and Tyson, 1990; Seely and Henschel, 1998). Because this drainage flow is cool, it cannot cap the MBL from above and is therefore not expected to contribute meaningfully to the inversion-lowering signal of the fog days in the composites.*" In the same paragraph we now also distinguish explicitly between the warm, dry synoptically forced subsiding easterly (which can cap the MBL from above via adiabatic descent) and the cold surface katabatic drainage component (which arrives colder than the MBL and is expected to mix with it rather than cap it (Seely and Henschel, 1998). This refinement, together with the corrected radiative-cooling mechanism the reviewer suggested, makes the Sec. 4.1 interpretation internally consistent.

Specific comments

Fig. 4 g-l: Here, you use temperature and specific humidity at 950 hPa. Looking at the topography of southern Africa, surface pressure will be lower than 950 hPa in many regions due to the high surface elevation. This means that data at this pressure level is extrapolated to the 950 hPa level. I recommend masking out those regions and/or choosing a higher pressure level. We have removed the panels, as these were not adding much and were not discussed in the manuscript. The same issue was present in Figure 3, j–l (for Z850hPa), and we have implemented the suggested change there.

P5, L122: Missing reference.
Good catch, the reference is now included.

Reviewer 2

General comments

This manuscript examines synoptic control on the formation of fog and low clouds over the western margin of the South African continent. The authors address this study by setting a spatial analysis to prove that the dipole high-pressure system between the Atlantic and the continent is the main mechanism controlling the land-to-sea wind circulation and boundary layer height. The manuscript is well written and well structured, with scientifically sound argumentation. The analysis, although descriptive, delves into synoptic causes of the difference between low stratus and land fog through a combination of reanalysis data and a case study. There are some minor comments in the supplement that need to be addressed before being considered for publication.

We thank the reviewer for the positive evaluation and the detailed review and respond to the individual points below.

Abstract

The abstract introduces the synoptic scale problem well. However, it lacks results, being difficult to evaluate.

We have now extended the abstract to include the BLH number from our findings. "*In fog situations, the marine boundary layer is shallower by up to* $130\,\mathrm{m}$ *along the entire coastline than in low-cloud situations.*"

Introduction

Line 26: ‘fog precipitation’ means fog and precipitation or fog deposition?

"Fog precipitation" refers to fog water collected by a fog collector (Juvik-type). In the FogNet stations, Juvik fog collectors are basically cylindrical screens mounted above a rain gauges into which the fog water deposited on the screen drips which is measured by a tipping bucket. We now shortly describe this in the introduction to give the reader an initial orientation, but then more detailed description of the measurements in the data section. In the introduction we now state: ”*Fog water collected by Juvik-type fog collectors (hereafter fog precipitation; see Sect. 2) peaks within a belt* $20-60\,\mathrm{km}$ *inland from the coast at around* $400-500\,\mathrm{m}$ *a.s.l. (Lancaster et al., 1984; Seely and Henschel, 1998).* ” The updated description in the data section is described below.

Line 28: the austral summer is from late December to late March, and September is even winter. September to March is the transition between late winter and late summer.

You are right, thanks for pointing this out. We now state: ”*Directly at the coast and peaking during the warm half-year (Sep to Mar), advective fog is connected to the southwesterly sea breeze in the afternoon (Seely and Henschel, 1998).*”

Line 30: The northeasterly surface wind is related to fog if fog/low stratus comes from the southwest (Atlantic)?

In Seely and Henschel (1998) north-northeasterly winds are presented during fog, but newer analyses have shown that it is more north-northwesterly winds during fog (Spirig et al., 2019; Spirig, 2022). We now only state northerly and provide a short explanation in the updated introduction: ”*It has been speculated that the northerly near-surface winds during inland fog events may be decoupled from the cloud-level winds that transport the stratus and may arise from mixing of different air masses (Seely and Henschel, 1998).*”

Line 35: SON is spring, not summer. Summer is JFM. Please correct.

Thanks, you are right. We now state: ”*During austral spring and summer the inversion is elevated,...*”, as this is really true for SONDJF.

Line 55: This is an austral summer! Which contradicts the other one mentioned in lines 28 and 35.

Yes, but this time the statement is actually correct.

Figure 1. Please set the coordinates for the MODIS image to be easily compared to the ERA5 synoptic patterns

We have added longitude and latitude lines to the snapshot.

Data

Line 99-101: Does this algorithm filter by mid-clouds? Because I understand that fog with a cloud base of 200 m might be warmer than a cloud with a cloud base of 500 m.

In coastal Namibia true mid-level clouds with cloud tops above 2km do not typically occur and would be an excepionally rare event. Mid-level clouds can be found further north on the eastern edge of the stratocumulus field during winter (Adebiyi et al., 2020). It is correct that cloud base altitudes between 200 m and 400 m or 500 m are typical of the coastal stratus in Namibia (Andersen et al., 2019) and it is likely that cloud-top temperatures vary also with cloud height, among other factors. However, the algorithm is designed to capture all of the typically occurring stratus clouds in the region. This is achieved by using conservative IR (and IR-differences) thresholds together with a test that compares the structural similarity between a clear sky composite and the actual scene. This makes the algorithm robust against variations in low-level cloud-top temperatures.

Line 105-109: It is well-known that ERA5 products are not representative of observations in the southern hemisphere, especially in relation to surface fluxes, where observations are scarce. Do you have any idea of how valid surface fluxes from ERA5 are to be included in the analysis?

Yes, we agree that the quality of ERA5 products is not homogeneous for the variables used, and that in particular the surface fluxes have large uncertainties. In addition to this, a recent study has shown that also the thermotopographic winds start to occur later than in the observations, however, they are represented at the time of day considererd here (5 UTC (Mass et al., 2026)). In the updated version of the manuscript we now state: "*ERA5 uncertainties differ across variables, with surface fluxes being less tightly constrained than directly observed quantities such as sea surface temperature (Bentamy et al., 2017; Luo and Minnett, 2020; Martens et al., 2020; Pokhrel et al., 2020). Specifically for near-surface winds in the central Namib, Mass et al. (2026) report that the diurnal evolution of the thermotopographic mountain-plain wind in ERA5 is delayed relative to in situ observations, but the circulation is captured at the time of our composite analysis (05 UTC).*"

Line 122: there is a latex missing input,'?'

Thanks, we have corrected this.

Methods

Line 138: 'fog precipitacion measurements' means fog collection measurements? Through a standard fog collector or a cylindrical one? If so, how do you deal with potential dew that the fog collector can register? This is quite relevant, since low-cloud days might be misinterpreted as fog days if the collector is measuring dew rather than fog.

We agree that this needs to explained more thoroughly in the manuscript and have therefore expanded our discussion of the fog precipitation measurements in the updated version of the manuscript: "*Each Juvik-type collector comprises a louvered aluminium cylinder (12.7 cm in diameter, 40.6 cm tall) mounted above a tipping-bucket rain gauge that registers fog water dripping from the collector in discrete 2ml increments. The instrument design follows Juvik and Nullet (1995) and includes a protective rain shield that also reduces longwave radiative cooling of the mesh, thereby suppressing dew formation on the collector itself. The amount of fog water is scaled to a square meter: 1 mm of fog precipitation correspond to 20 ml of fog water. Via the collector's projected silhouette area of* $515.6\,\mathrm{cm}^2$*) the fog water amounts are comparable to other collector types. The resulting catch, hereafter referred to as fog precipitation (FP), is a semi-quantitative measure of fog deposition because the collection efficiency depends on the silhouette area exposed to the airflow and on the surface properties of the mesh (Spirig et al., 2019). Misclassification of dew-only nights, which also occur frequently in the Namib (Henschel and Seely, 2008), as fog events is therefore unlikely: beyond the suppression of dew on the collector by the rain shield, dewfall in the central Namib is expected primarily under clear-sky conditions, whereas our analysis is restricted to nights with low-cloud cover".*

I would expect a figure or table showing the simple logistic regression skill described in 3.2.

The results of the logistic regression are presented in a figure in section 4.3.

Results and discussion

The analysis is very descriptive, which is good when non-understood processes are found. However, overall, I missed some relational analysis that would have reinforced the results and supported the hypothesis. For example, the main hypothesis is that seasonal and interseasonal variability in the two marine and continental high-pressure systems influences the thermally driven land-sea circulation and MBL capping when fog and LC occur. I would expect, for example, to see how much (statistical metrics) pressure changes are related to wind circulation or MBL height, to evaluate if it is a mechanism (high correlation) that is controlling FLC or fog, or it is just a modulator (low correlation).

We thank the reviewer for this important suggestion. We have added a new Fig. 5 (included as Fig. 1 in this response) at the end of Sect. 4.1 that quantifies some of the key relationships between the continental synoptic high pressure, the regional pressure gradient, the near-surface easterly flow, and the offshore boundary layer height. The continental high and Pdiff show a moderate negative correlation (r = -0.43), Pdiff and the 900 hPa easterly wind close to CM a moderate positive correlation (r = 0.51), and Pdiff and the offshore BLH a strong correlation (0.62). More importantly, the triangular shape of the scatter cloud points to a physical bound that deep MBL cannot occur in the tail of the Pdiff distribution that would be associated with Bergwind conditions. The fog and low-cloud event clouds are visibly separated in all three panels, illustrating that these variables jointly carry information that discriminates between the two regimes. We deliberately report correlations rather than a clean attribution because the synoptic and regional contributions are physically coupled (Sect. 4.1, new "note on scale separation" paragraph), which we believe is more honest than statistical metrics that would over-state the cleanness of the attribution.

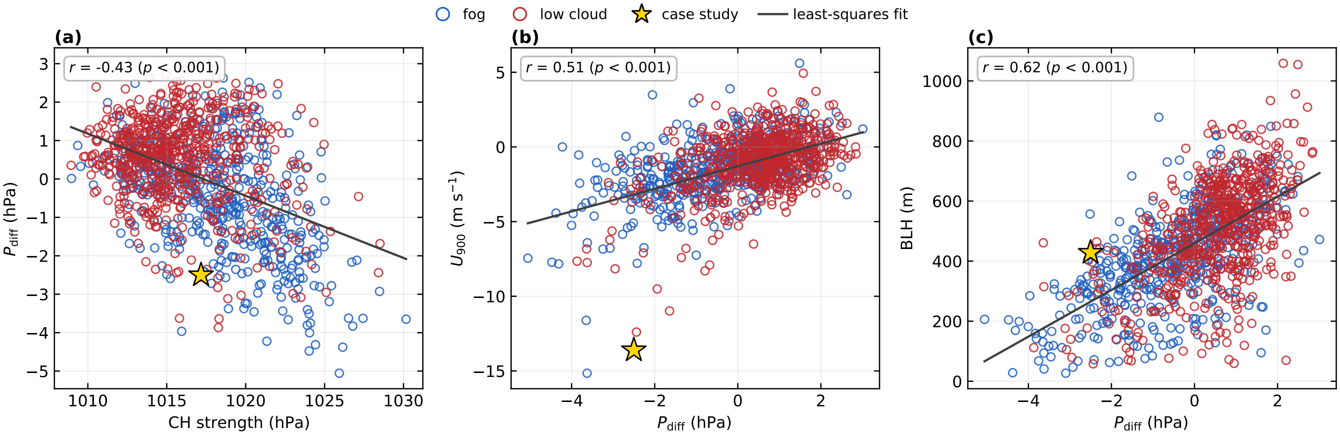

Figure 1. Quantitative relationships between the synoptic and regional pressure pattern, the near-surface zonal wind in the Central Namib, and the offshore boundary layer height, evaluated across all fog (red) and low-cloud (blue) events at CM. (a) Strength of the continental high (CH, Table 1) versus the regional pressure gradient P_{diff}. (b) P_{diff} versus the 900 hPa zonal wind component at 23°S and 15°E. (c) P_{diff} versus the offshore boundary layer height west of CM (23°S and 13°E). Pearson correlation coefficients are given in each panel. All quantities are computed from ERA5 at 05 UTC. The star marks the case event investigated in more detail as a case study in Sec. 4.2.

Figure 4 shows the vertical structure of MBL over the study area where CM and GB stations are located. The cyan line shows the BLH. However, when I see the thermal or humidity structure shown in 4a, b,d and e, the BLH is not in agreement with the T and q vertical structure. What I mean is that maximum dT/dz or dq/dz is located, for example, at Fig 4a around 950 hPa (14.5°E, over the CM station), while the BLH line is at 1000. Same in figure 4b, where the BLH line decays over the CM station, where dT/dz is two levels higher. This is probably because BLH in ERA5 is calculated using the Rib (bulk Richardson number), which does not well represent BLH over the CM station. This might be leading to incorrect interpretations, even though max dT/dz or dq/dz shows that the MBL is lower under fog than under low cloud. My suggestion here is to recalculate the BLH using the maximum dtheta/dz (potential temperature) to have a consistent BLH line with the inversion observed in Fig 4a to d. Also, some vertical profiles of theta and q next to vertical cross section would be useful to quantify BLH and structure.

We thank the reviewer for this observation. The reviewer is correct that the ERA5 boundary layer height (BLH), diagnosed from the bulk Richardson number (Ri$_b$), does not consistently coincide with the thermodynamic inversion visible in the cross-sections of Fig. 4 of the manuscript. Since the Ri$_b$ method does not account for liquid water, it tends to approximate the cloud-base height rather than the true PBL height in marine cloudy boundary layers (Engeln and Teixeira, 2013), and can therefore be expected to be biased low in coastal Namibia. Following the reviewer's suggestion, we implemented an alternative diagnostic based on the height of maximum $\partial\theta_v/\partial z$. As shown in Fig. 2 in this response, for a representative offshore profile the $\partial\theta_v/\partial z$ diagnostic (magenta, 688 m a.s.l.) places the BLH directly at the θ_v inversion, while the ERA5 Ri$_b$ BLH (cyan dashed, 596 m a.g.l.) falls ∼90 m below it. However, with ERA5 pressure-level spacing of 25 hPa (≈200–250 m per level near 900–950 hPa), the $\partial\theta_v/\partial z$ diagnostic can only resolve the inversion at a small number of discrete heights, producing staircase

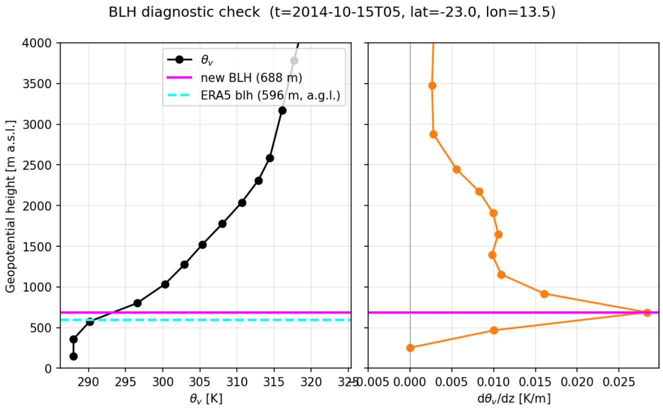

Figure 2. BLH diagnostic check, single profile

artefacts in composite figures. We therefore removed the BLH line from Fig. 4 and added a note to Sect. 2.2: *“The ERA5 bulk Richardson number-based BLH does not account for liquid water and tends to approximate the cloud-base height rather than the true PBL height in marine cloudy boundary layers (Engeln and Teixeira, 2013). It can therefore be expected to be biased low in coastal Namibia.*

We also added a local T and Q profile at CM as a supplementary figure (shown here as Fig. 3 in this response) and include this description in the manuscript: ”*Local temperature and moisture profiles at CM are shown in supplementary Fig. 1, and highlight that differences between fog and low cloud cases peak at 950 hPa.* ”

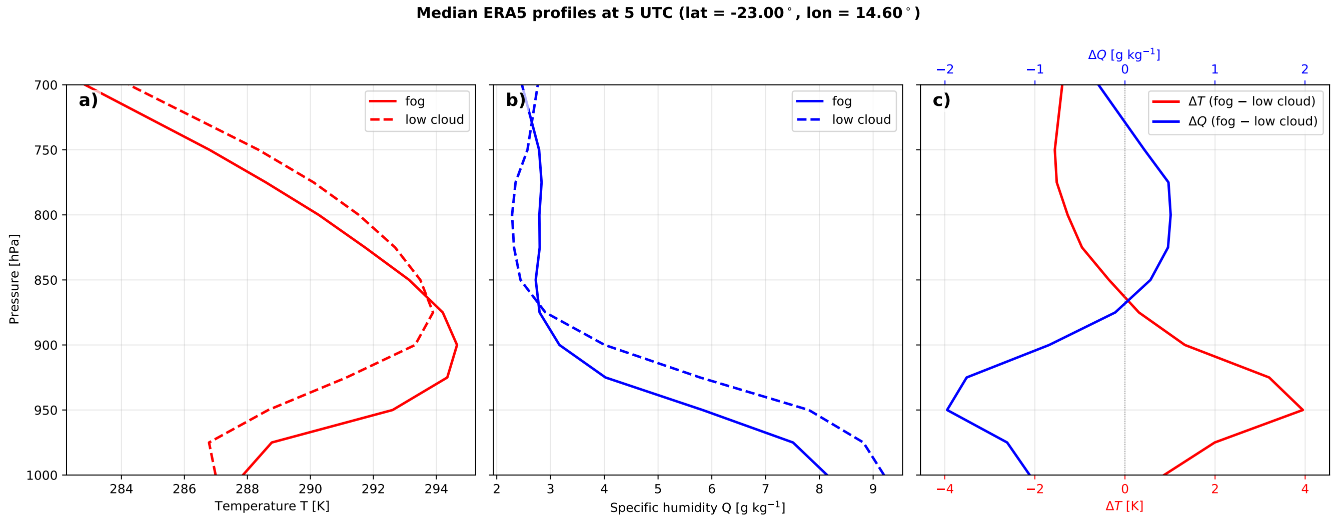

Figure 3. Median temperature (a) and moisture (b) profiles for fog cases (solid lines) and low cloud cases (dashed lines) and their differences (c) at CM.

Another element that could be included in the analysis is the inversion layer strength. For example, Espinoza et al. (2024) (https://doi.org/10.1016/j.atmosres.2024.107533) demonstrate that thermal inversion is a key synoptic mechanisms that control FLC formation, using the Low Tropospheric Stability (LTS) parameter to quantify thermal inversion. Using ERA5 data, it is easy to compute thermal differences between two levels to characterise, in time and space, the strength of the thermal inversion, which is likely higher under fog than in LC situations.

It is correct that the stability/inversion strength is one of the main mechanisms determining fog and low cloud variability, and we had looked into this before submission of the original manuscript. However, as the temperature profiles in Fig. 4 of the manuscript already show, there are no pronounced differences between fog and low cloud composites. Offshore, 1000hPa temperatures only differ marginally, and above 850hPa the same is true. We still calculated LTS850 and present it below. We argue that while the LTS differences suggest much more stable conditions during low clouds than during fog along the coastline (but strongest inland), this is not actually the case. The difference essentially reflects the fact that at these locations the surface is already in the inversion layer in many fog cases (therefore warmer and seemingly less stable). We therefore decided to stick with the profiles rather than the LTS. We added a statement on this in the manuscript: "*As the temperature cross sections show limited temperature differences at 1000 hPa over the ocean and above 850 hPa in the free troposphere, the overall stability of the lower troposphere, which is an important fog an low cloud control (Espinoza et al., 2024; Mass et al., 2026), is not found to differ substantially.*"

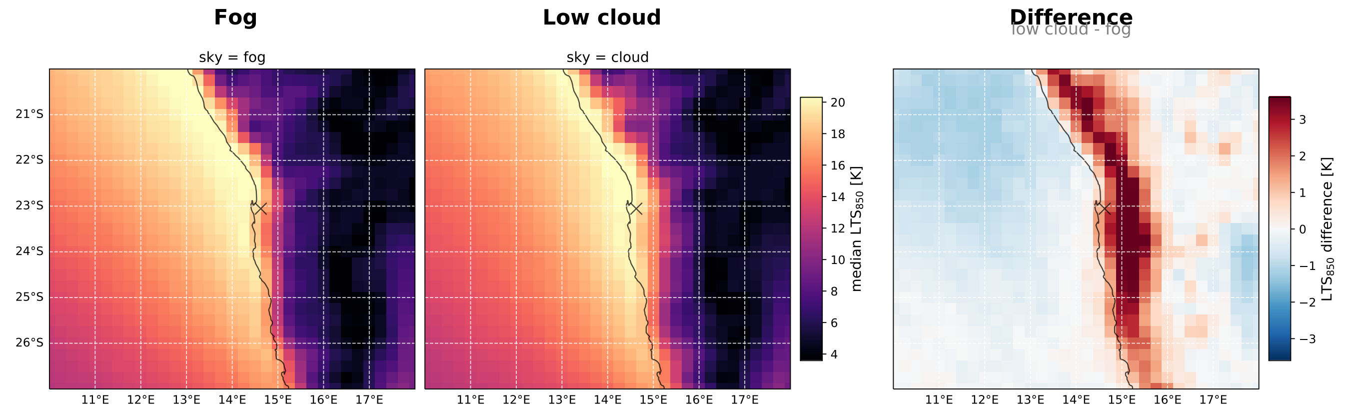

Figure 4. Composite of LTS850 between fog and low cloud groups.

Figures 4, 7: I suggest changing the colour palette used to represent temperature and specific humidity to a more nature-related colour. For example, ‘coolwarm’ to temperature and ‘Blues’ to specific humidity. ‘viridis’ is ok for pressure, but it is confusing to see viridis for every variable. In that way, it would be easy to see the black wind arrow that, for example, in Fig. 7h, is extremely hard to see.

We have changed the colormap to "Reds" for temperature and "Blues" for specific and relative humidity in this figures, and also chose variable-specific ones in figure 3.

Figures 4, 7: it has to be mentioned that vertical wind has been exaggerated to represent the vector since horizontal wind (u,v) is usually one order of magnitude higher than the vertical one.

Thanks for the pointer, we now include this information in the relevant figure captions: ”*For illustration purposes, the w vector is multiplied by a factor of 10.*”

Conclusions

To reinforce conclusion bullet n° 3, the authors could include an analysis of the rate of change of MBL (dh/dt) relative to changes in the continental pressure system, which will likely show a stronger correlation under fog than in LC situations.

We thank the reviewer for this suggestion, we believe to have addressed strengthened the third conclusion bullet with our new relational analysis (see above).

Figure 10, which, to my understanding, summarises processes described in section 4.1, should be located there. It is odd to see a figure in the conclusions. Especially if it is a physical interpretation of processes described in section 4.1.

As the aspect of the synoptic strengthening of the easterlies is developed with the case study, we have decided to move this figure at the end of section 4.2. We have added a paragraph to describe this figure more thoroughly as well: ”*Taken together, the composite analysis (Sec. 4.1) and the case study presented above provide a consistent picture of the synoptic and regional-scale*

processes that modulate the stratus altitude in the central Namib which are schematically summarized in Fig. 10. Under typical low-cloud conditions, a strengthened Atlantic High drives enhanced coast-parallel southeasterly winds, increased latent and sensible heat fluxes upstream of the study region, and a correspondingly deeper marine boundary layer that supports an elevated stratus base. Under typical fog conditions these processes are less developed and the MBL is shallower, and elevated continental pressure favors the development of a nocturnal mountain-plain wind that descends the Great Escarpment, and can mix with, undercut or block the advection of marine air masses. The case study additionally shows that the regional katabatic flow can be completely overridden by a synoptically forced (here: ridging high-pressure system) easterly flow that is hot and dry and intensifies the inversion from above well beyond the composite mean and confines the stratus to a narrow coastal strip.
”

Reviewer 3

General comments

The manuscript investigates the meteorological controls on stratus cloud base height in the Namib Desert, considering both large-scale circulation and regional-scale processes that influence boundary-layer structure and cloud altitude. Using synoptic analyses, vertical cross-sections, and ground-based observations, the authors examine how pressure patterns and wind fields interact with boundary-layer dynamics to control low-cloud altitude. Focusing on an atypical fog event, the study explores how variations in cloud base height govern the transition from stratus to surface-reaching fog. A logistic regression model is also used to assess the predictive skill of key meteorological variables for fog occurrence.

Overall, the manuscript provides valuable insights into the coupling between large-scale dynamics and boundary-layer processes in a region where fog is climatically important. The manuscript is well structured and clearly written. However, several aspects of the methodology, interpretation, and figure presentation and discussion require further clarification. The manuscript requires major revision before it can be considered for publication.

We thank the reviewer for the generally positive evaluation and the detailed review and respond to the individual points raised by the reviewer below.

Specific comments

Line 3: Please clarify what is meant by “the relevant processes.”

Thanks, we agree that this was not well communicated. We have modified the sentence to state: ”*This study aims to develop a basic understanding of the processes that influence the stratus altitude and by extension the spatial fog occurrence.*”

Line 11: Please specify which two pressure systems are being referred to.

Done, the sentence now reads: ”*To assess the predictive power of the continental and Atlantic high-pressure systems and the regional pressure pattern, ...*”

Line 27: Above sea level (a.s.l.)

Adapted, thanks!

Line 68: change ‘the fog occurrence spatial patterns,’ to ‘the spatial patterns of fog occurrence’

Done.

Line 111-113: Please revise the sentence for clarity. ‘Both measure the standard meteorological variables at 1 min resolution…, and additionally measure fog water input…’

Done, this is better!

Line 113: ‘Juvik-type’

Done.

Line 122: A reference appears to be missing here.

Thanks, we have corrected this.

Line 135-142: Could the authors clarify what threshold of fog precipitation was used to define ‘any’? For example, was a minimum measurable amount (e.g., instrument detection limit or a specific cutoff value) applied?

The individual ticks of the tipping bucket is 2ml. Fog precipitation is then frequently normalized from the collector area (a 515.6 mm^2 surface area) to $1m^2$ and given in mm. Here though, no further threshold is used, but rather, fog nights are defined as a night where a 2ml tick was registered. We have substantially expanded our description of fog precipitation measuremnts in the updated version of the manuscript.

Figure 2: In the caption of Figure 2, please clarify the meaning of the dashed and solid lines, as well as the shaded regions. From my understanding, the solid lines and shaded regions appear to represent similar information.

We have clarified this in the caption: "*Time series of monthly counts of fog (red) and low-cloud (blue) events and their sum (dashed line) between 4 and 6 UTC at CM, according to combined SEVIRI-based low-cloud data and in situ fog precipitation. Bars at the top and dotted vertical lines indicate the months of peak seasons at CM of fog (red, April-May-June) and low clouds (blue, September-October-November).*"

Figure 3: In Figure 3, do the outlines indicate regions where the differences are consistent throughout the year (i.e., same sign, shown by black contours)? These are difficult to see in panels 3f and 3i. Additionally, line 195 states that the outlines are in blue. Please clarify this in the caption and consider using a brighter or more distinct color.

The caption now states: "*Regions where the differences are consistent throughout the year (same sign) are highlighted with contours for the variables...*". The wording in L195 was poorly chosen and did not refer to the contours but the map itself, which is now corrected.

Line 275: In this sentence, ‘there contrasting’ appears to be a typographical error; it would be clearer as ‘thereby contrasting with’.

We agree, this sounded a bit clumsy. We now state: ”*The second characteristic is a southeasterly* $850\,\mathrm{hPa}$ *wind that originates from the AH and passes over the continent before reaching CM, in contrast to the calm local conditions observed on the nights before and after.* ”

Line 276-277: Is this interpretation correct? I do not clearly see a southeasterly signal on the day before the event.

Yes, this is what we intended to convey. We hope this is clearer now: ” *As a third characteristic, the coast-parallel southeasterly winds are weakly developed because of the position of the AH, which is also the case one day before the event.*”

Line 303-304: The phrase “there due to limited range only observed after 10 UTC” is unclear. It may be clearer to write: “where it is only observed after 10 UTC due to limited range.”

We have changed this for clarity and corrected another typo: ”*Ground-based SODAR measurements confirm the easterly wind component. At GB (station elevation* $405\,\mathrm{m}$ *a.s.l.), the easterly extends down into the lowest SODAR level at* $\sim 450\,\mathrm{m}$ *a.s.l. and thus effectively reaches the surface. At CM (*$94\,\mathrm{m}$ *a.s.l.), by contrast, the easterly is observed only at* $\sim 500\,\mathrm{m}$ *a.s.l. — several hundred metres above the surface — and only after* $10\,\mathrm{UTC}$ *due to the limited SODAR range (Fig. 8b).*”

Figure 9: Please clarify the ratio of training to testing data used in the analysis.

The cross validation strategy is described in section 2.3. We have extended this by stating: ”*Across iterations, this corresponds on average to a training-to-test ratio broadly equivalent to a standard 5-fold cross-validation split.*” The training-to-test ratio is on average approximately 84:16 (5.4:1). Of the 1372 FLC events in the dataset (Aug 2014–Dec 2020), roughly 214 events fall within any given 365-day test window, with the remaining 1150 events — minus a 14-day buffer (7 days on each side of the test window) — used for training.

Additionally, provide a more detailed explanation of the figure, including the meaning of the F1-score and the representation of dots and lines (their frequencies).

We thank the reviewer for highlighting that the figure was insufficiently self-explanatory. The caption of the figure has been substantially expanded and a complementary definition of accuracy and F1-score has been added to Sect. 3.2.

Line 327-328: Is this ranking of importance statistically supported? For example, are there confidence intervals, significance tests, or standardized coefficients that demonstrate the relative importance of these predictors? I did not find any figure or quantitative analysis that clearly supports this statement. Please clarify how feature importance is assessed and consider providing additional evidence or visualization.

We agree and have removed the feature-importance ranking from Sect. 4.3 entirely and replaced it with a new Fig. 5 (Sect. 4.1; included as Fig. 1 in this response) that shows the relationships between some of the three features and the offshore boundary layer height, separated by fog and low-cloud events. The new figure illustrates that all three features carry information distinguishing the two regimes, without claiming a clean physical ranking, which the coupling between the features (Sect. 4.1) would in any case make difficult to interpret. The combined skill of the three features against the climatological baseline (Fig. 11) is now reported as the headline classification result.

In Figure 9, the accuracy of the logistic regression appears to decrease toward the end of certain years (e.g., 2015, 2018, 2020). Could the authors comment on the potential reasons for this behavior?

We view the large deviation in model performance in late 2019/early 2020 as the main signal here, on which we have expanded our discussion. The updated version of the manuscript now states: "*The reduced classification skill during the 2019 and 2020 test windows (Fig. 11) coincides with the Benguela Niño of October 2019–January 2020, which produced coastal SST anomalies exceeding* $2\,^{\circ}\mathrm{C}$ *and was accompanied by anomalous near-surface winds and surface heat fluxes (Imbol Koungue et al., 2021). Under these anomalous oceanic conditions, the climatological coupling between the synoptic pressure pattern and the marine boundary layer state is weakened. Warmer SSTs enhance latent and sensible heat fluxes into the MBL and deepen it, so that synoptic conditions that would normally produce fog at CM may instead produce elevated low clouds. Because the model uses only pressure-based features (CH, AH,* P_{diff}*) and does not carry direct information on SST or surface heat fluxes, this oceanically driven shift in the MBL response is not represented in the feature space. The simultaneous drop in baseline skill during the same period confirms that even the climatological seasonality is shifted, so that the lower model skill in 2019/2020 reflects a genuine departure from the climatological expectation rather than a methodological shortcoming. This is consistent with the findings of Mass et al. (2026) that Benguela Niños are a relevant driver of fog and low-cloud variability in the central Namib, and suggests that including SST patterns or MBL information could improve model robustness to anomalous oceanic states in future work.*"

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