Response to comments from reviewer 3

Manuscript: Coastal upwelling and tropical warm water intrusions are key drivers of interannual fog variability along the southwestern African coast # EGUSPHERE-2026-1208

We thank the reviewer for the assessment of the manuscript and for raising the question of code availability.
We share the reviewer's view that the code should be available alongside the publication. Our intention had been to release it once the review process was complete, so that the published version would correspond to the final manuscript and implementation rather than an intermediate state. We recognise that this does not help a reviewer trying to verify the results, and we have therefore made the current version of the code available now. It will remain accessible for the remainder of the review process and will be updated if the the manuscript is accepted.
We thank the four anonymous reviewers in the acknowledgments of the updated version of the manuscript. The comments and suggestions from Reviewer 3 are incorporated in italics and addressed by the authors in blue.

1 General

The manuscript would benefit greatly from more discussion at the end – more contextualization, more outlook, and the like. In other words, what's the point in all of this and how does this manuscript help the field?

We agree that the Conclusions and outlook section was narrow, mostly restating the main findings without connecting them to a broader motivation. We have revised the section in four ways.
First, we added an opening sentence stating why FLC variability matters here specifically: FLCs are the main source of moisture sustaining ecosystems across the hyper-arid Namib, so understanding what drives their variability is relevant to anticipating this moisture supply under future climate conditions.
Second, in revising this section we clarified the reanalysis discrepancy finding. We no note that the wind fields show the largest disagreement between ERA5 and MERRA-2, and that both reanalyses show a bias relative to station observation. This suggests a broader limitation of reanalysis products in remote regions with pronounced thermo-topographic wind regimes, such as the Namib, one of the most pronounced examples of this phenomenon globally. Rather than resolving the diurnal thermo-topographic cycle itself, reanalyses appear to represent mainly the synoptic variability.
Third, we added a paragraph after the findings stating what this study contributes more broadly: that a cloud-controlling factor framework, developed for marine low clouds, can be applied to a coastal fog system with physically interpretable results, and that extending the historical reconstruction to nearly four decades is what makes the link to regional climate modes possible in the first place.
Fourth, we revised the closing sentence to state a specific, motivated question for future work, namely whether the compensation between atmospheric stabilization and SST warming identified in this study persists or breaks down under stronger future forcing, rather than a general statement that the framework "could be used" with climate models.

Is it necessary to test the stationarity assumption around line 159?

The stationarity assumption cannot be directly validated for the 1982–2003 reconstruction period, since no satellite-based FLC observations exist prior to 2004 to evaluate the reconstruction against. One possible approach to assess temporal stability within the observed record would be to split the 2004–2019 period into two subsets and compare the sensitivities estimated from each half. This would, however, substantially reduce the observation-to-predictor ratio relative to the main analysis, requiring a different regularization strength and larger parameter uncertainty independent of any real change in the underlying sensitivities. Any difference between the two halves would therefore reflect this reduced sample size at least as much as genuine non-stationarity. Stationarity of the sensitivities remains a necessary, but not directly testable, assumption of our framework, a limitation common to CCF frameworks in general.

Can you comment on the co-variability (or absence thereof) between EIS and SST around line 275?

The EIS and SST anomaly fields used as predictors are moderately anti-correlated over the regression domain. Correlating the deseasonalized, detrended monthly anomalies gives a domain-mean Pearson correlation of $r = -0.52$ for the ERA5 configuration ($r = -0.49$ for MERRA-2), with grid-point correlations negative throughout the domain. This co-variability is physically consistent, as SST influences lower-tropospheric stability: inversion strength depends on the temperature contrast between the marine boundary layer and the free troposphere, so a warmer sea surface tends to weaken the capping inversion if free-tropospheric temperatures do not warm as much. This covariability does not, however, explain the opposing trend contributions discussed here. Over 1982–2019, both fields exhibit trends of the same sign, with SST warming and coastal EIS increasing (Fig. 5a,e,f). The opposing contributions shown in Fig. 4 therefore result from the opposite signs of the estimated sensitivities, rather than from opposing trends in the predictors themselves. Note that the anti-correlation refers to the detrended monthly anomalies used to fit the model, whereas the trends in Fig. 5 are computed from the non-detrended fields used for the historical reconstruction. The two are therefore not in conflict: at the monthly timescale, warmer SST anomalies are associated with weaker inversion anomalies, whereas over the full period both SST and EIS exhibit positive trends.

This is in a special issue on 'aerosol, fog, climate, and biogeochemistry in southern Africa' and yet this manuscript has nothing to do with aerosol as a CCF... is that common? Or are aerosols not important around this area?

Aerosols are not included as a cloud-controlling factor here. We investigated aerosol influences on Namib FLCs directly in Mass et al. (2025), using the same satellite FLC product and the same two regions, and found aerosol effects difficult to disentangle from meteorology: days with high aerosol loading are sampled under systematically different circulation and humidity conditions, and the resulting aerosol sensitivities were not robust. This does not reflect a view that aerosols are unimportant in the region.
Additionally, the special issue explicitly comprises papers with complementary goals in order to encourage exchange across activities in the region. This work is associated with AEROFOG, the central project that motivated the special issue, supporting its fit within the scope of the issue.
Added in 2.2: "Finally, based on Mass et al. (2025), it was found that biomass burning aerosols (BBA) may contribute to FLC persistence via semi-direct effects, but were found to be hard to separate from meteorological effects. As the analysis of Mass et al. (2025) suggests that semi-direct effects may be captured in the EIS fields already, we do not include BBA, or other aerosols, as CCFs here"

Can you better situate this manuscript against/along the findings of Li et al 2025?

Related comments from two other reviewers prompted us to expand the discussion of Li et al. (2025), and the revised manuscript now situates our results against that study in three places.
In the Introduction, we describe the mechanism they propose for the El Niño–fog link, namely increased moisture advection associated with more frequent northwesterly winds, and note that their result is based on observations from a single loca-

tion and is mainly evident during the most recent decade of the record. This motivates our investigation of ENSO-related FLC variability across the Namib desert.
In Sect. 3.2, we address the apparent disagreement over long-term trends. Li et al. (2025) report a decline in fog amount after 1996, whereas our reconstructions show near-zero trends. The two studies measure different quantities over different domains: they report fog water collected at a single inland station, whereas we report FLC cover averaged over the coastal strip. A decline in collected fog water can occur without a corresponding change in the frequency with which low cloud is present, their station lies outside our study domain, and they attribute much of the decline to air temperature and to local hydrological and vegetation processes that are not represented in our framework. Their record is also not a single continuous measurement, but a series assembled from successive fog collectors operating on different measurement principles, and the 1996 step coincides with one of these instrument transitions, which calls for care in interpreting the break point.

2 Specific

L 70: seems a bit defensive and giving up prematurely? More details?

We revised this sentence in response to a similar comment from another reviewer. The citation to Malik et al. (2026) was intended to acknowledge that separating fog from low stratus is possible in principle, so that grouping them here is a deliberate choice rather than an unavoidable limitation of the retrieval. We agree the original wording interrupted the flow and have shortened it.
-Modified in 2.1: "It should be noted, however, that this satellite-based method cannot on its own distinguish between fog and low stratus clouds, as it is not straightforward from a satellite perspective to determine whether the cloud base is in contact with the surface. Cloud base height can be estimated by combining the satellite product with additional near-surface relative humidity data (Malik et al., 2026), but such an estimate is not required for our analysis, and fog and low clouds are therefore grouped together throughout."

Fig 1: help the reader understand the fig with two color bars?

The two colour bars refer to fields plotted over separate areas of the map, using distinct colour scales: sea surface temperature over the ocean in purple to yellow shades, and FLC occurrence frequency over land in blue shades. They are shown on a single map to give an overview of the study area, covering the upwelling areas and the regions of frequent fog occurrence. We agree that the original caption did not make the correspondence between the colour bars and the fields explicit and have revised it accordingly.
-Modified in the caption of Fig. 1: "Spatial distribution of sea surface temperatures and relative frequency of fog and low cloud (FLC) occurrence along the southwestern African coast. SST [°C] is shown over the ocean in purple to yellow shades and FLC occurrence frequency [%] over land in blue shades, giving an overview of both the upwelling regions and the areas of frequent fog occurrence. The two regions are outlined in red: Angolan Namib (AN) and Central Namib (CN). The Cape Frio upwelling cell and the Lüderitz upwelling cell are marked with white circular arrows."

L 83: convection? Typo?

Thank you for catching this. It was a typo; the intended word is ”convention”, and the sentence has been corrected.

L 87: quantify skill improvement/degradation and how you judged it?

W700 was tested during model development. Model skill was evaluated with the coefficient of determination (R^2) on the held-out folds, the same criterion used throughout the study. We compared three configurations: the five-predictor model, the same model with W700 added, and a model using W700 alone. Adding W700 left the out-of-sample skill unchanged, and the W700-only model gave an R^2 of essentially zero, indicating no predictive information for FLC cover anomalies. We do not conclude from this that subsidence is physically unimportant for FLCs, only that the available reanalysis W700 field did not add usable information at the spatial and temporal scales considered here.
-Added in 2.2: ”Subsidence was tested as an additional predictor but was excluded from the final set, as it did not provide additional predictive information within our modelling framework. This does not imply that subsidence is physically unimportant for FLCs, but rather that it did not provide additional usable information for predicting FLC cover anomalies at the spatial and temporal scales considered here.”

Fig 2: maybe put the variable names on the fig itself?

Variable names have been added to Fig. 2 and to the corresponding figures in Appendix A.

L 373: It's rather difficult to verify the claims without taking a quick look at the code, so to me, it feels like I wasted my time reviewing this manuscript. Without code, anyone can make up all sorts of claims, etc. — I recommend a rejection here because it is impossible to support the claims made by the authors without at least a cursory look at the code.

The code used to produce the analyses in this manuscript has been deposited and is available at [DOI], and we have replaced the previous ”available upon reasonable request” statement accordingly.
-Modified in Code and data availability: ”Code for data processing is available at https://doi.org/10.5281/zenodo.21630065.”

References

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Malik, D., Andersen, H., Cermak, J., Vogt, R., and Adler, B.: Cloud base height determines fog occurrence patterns in the Namib Desert, Atmospheric Chemistry and Physics, 26, 681–701, https://doi.org/10.5194/acp-26-681-2026, 2026.

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