Response to comments from reviewer 1

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

First of all, we would like to thank the reviewer for the positive comments and for providing very helpful and constructive feedback. We thank the four anonymous reviewers in the acknowledgments of the updated version of the manuscript. The comments and suggestions from reviewer 1 are incorporated in italics and addressed by the authors in blue.

1 Major Comment

The CCF input features seem to be an order of magnitude greater than the number of monthly observations for the time-series (thousands of features versus hundreds of observations). The tuning of the lambda was done to maximize out of sample performance, and therefore reduce overfitting. While that can provide an acceptable model for the historical reconstruction analysis in 3.2, it can still lead to many spurious coefficients when working with such a high dimensional problem. Perhaps using a harsher penalty for the sensitivity analysis in 3.1, could yield a more reasonable number of important input features. Alternatively, the authors could also use a dimensionality reduction technique like PCA (i.e. apply PCA to an n x n matrix of spatial points to reduce the total number of input features while conserving spatial variance information). Another option could be to remove the input fields over land for RH at 700 hPa and U10 and V10. This could additionally reduce noise in ridge coefficients because the authors mention that the wind fields are not as reliable over the land as over the ocean. I don't feel super strongly about which approach is used, but I do think the CCF analysis would benefit from some dimensionality reduction or a harsher penalty to clarify which spatial fields and variables control FLC variability.

We agree that a high number of predictors relative to the number of observations can be a concern. The choice of region was made with this in mind, as described in Section 2.3 to balance the size of the region and, therefore, the number of predictors within it, while still capturing all the relevant synoptic-scale controls. Following the reviewer's recommendation, we masked the input fields over land for R700, U10, and V10. We chose this approach because, in addition to helping with dimensionality, it also removes the inland wind fields, which have been shown to not accurately capture the diurnal cycle of the thermo-topographic winds.
It is worth noting that this masking results in a slight decrease in performance for CN-ERA5 (R^2 from 0.32 to 0.30), AN-ERA5 (R^2 from 0.42 to 0.38), and CN-MERRA-2 (R^2 from 0.33 to 0.31), and a slight increase for AN-MERRA2- (R^2 from 0.35 to 0.36). This slight decrease in performance for most configurations likely reflects a trade-off between reducing noise and removing informative signal. Over land, both reanalyses show substantial discrepancies in the wind fields due to complex terrain and differences in model parameterizations, and neither dataset adequately captures the region's thermo-topographic wind circulation. While these inland wind fields are therefore not reliable in an absolute sense, they may still carry some spatially correlated signal related to onshore/offshore flow that the ridge regression exploits, even if imperfectly. Masking these fields removes this signal along with the noise. Overall, we consider this modest trade-off acceptable, since it improves the physical interpretability and robustness of the remaining predictors.
Additionally, Figures 3 and 4 of the manuscript are the most impacted by this change (see comparison in the Appendix of this document).. For Figure 3, the ERA5 and MERRA-2 trends move closer together in the AN, whereas in the CN they diverge slightly; however, in both regions the trends remain statistically not significant. For Figure 4, the wind trend contributions between the two reanalyses also align more closely overall; however, CN-ERA5 remains an outlier with the largest discrepancy, marked by a negative area in the ridge coefficients over the northern part of the region that is present in all four configurations but considerably stronger here. Reducing the domain does not seem like a suitable response, since the pattern is not specific to CN-ERA5. This area is also close to the AN region, and the domain needs to remain large enough around both regions to capture the non-local effects.

- Added in section 2.2 :"To reduce the dimensionality of the predictor set and to avoid relying on inland wind fields, which have been shown to be unreliable (see Fig. B2), the R700, U10, and V10 fields were masked over land, so that only oceanic grid points were retained as predictors for these three variables. This also ensures consistency across all predictors, as they now share the same spatial extent."
- All figures, tables, and their analyses, where necessary, have been updated in the manuscript to reflect this change in method.

2 Minor Comments

It is unclear what the spatial range of AN and CN are, the paper shows the central point, but I didn't see any description of the specific area of focus. I recommend indicating the spatial averaging box for FLC in Figure 1. Also, I recommend using an open circle to show the centers, so that the value for that grid cell is visible and that the color of the circle is not the same as the shades used in the color bar.

The regions are briefly described in the introduction, but we agree with the reviewer that it is necessary to show the actual spatial extent of the regions. The two regions are not outlined in Figure 1, whereas in the rest of the maps their centers are now indicated by open circles.

Ln 129-130: After removing trend and monthly seasonality, is there still autocorrelation in the residuals?

We thank the reviewer for raising this. We tested for residual autocorrelation in the deseasonalized, detrended target time series using the Ljung-Box test (n = 186 months per region), which is significant at lag 12 ($p < 0.001$, both regions). The Autocorrelation function (ACF) (Fig. 1 in this document) shows autocorrelation coefficients exceeding the 95% confidence band near lags 10–12 for both regions, with most shorter lags falling within the band. We do not have a definitive explanation for this pattern; it may reflect multi-month persistence in the large-scale climate drivers of FLC variability discussed in Section 3.3, an incomplete removal of inter-annual shifts in fog-season timing by the fixed monthly climatology, or some combination of factors. We note that our cross-validation uses contiguous, non-shuffled folds, which reduces the potential for train-test dependence relative to randomly shuffled folds, though given the residual autocorrelation at lag 12, some dependence within folds cannot be fully excluded."
- Added in section 2.3: "However, the residual autocorrelation remains in the deseasonalized, detrended monthly time series (Ljung-Box test, $p < 0.001$ at lag 12), concentrated near lags 10–12. It's potentaly explained by the multi-month persistence in the large-scale climate drivers discussed in Section 3.3 or to limitations of the fixed monthly climatology used for deseasonalizing. This may reduce the effective degrees of freedom in subsequent significance tests."

What is the standard deviation in out of sample performance across the cross validation?

Out-of-sample R^2 across the 6 folds (mean ± std) is 0.36 ± 0.09 (MERRA-2) and 0.38 ± 0.12 (ERA5) for AN, and 0.31 ± 0.26 (MERRA-2) and 0.30 ± 0.26 (ERA5) for CN. AN shows stable skill across both reanalyses, while CN shows substantially greater fold-to-fold variability in both reanalyses, driven by two folds (folds 3–4, covering July 2009–August 2014) with near-zero R^2 in both reanalyses. This period coincides with a period of reduced variance in the observed CN FLC anomalies. Using the CN-ERA5 configuration, we examined this further (Fig. 2 in this document) and found that folds 3–

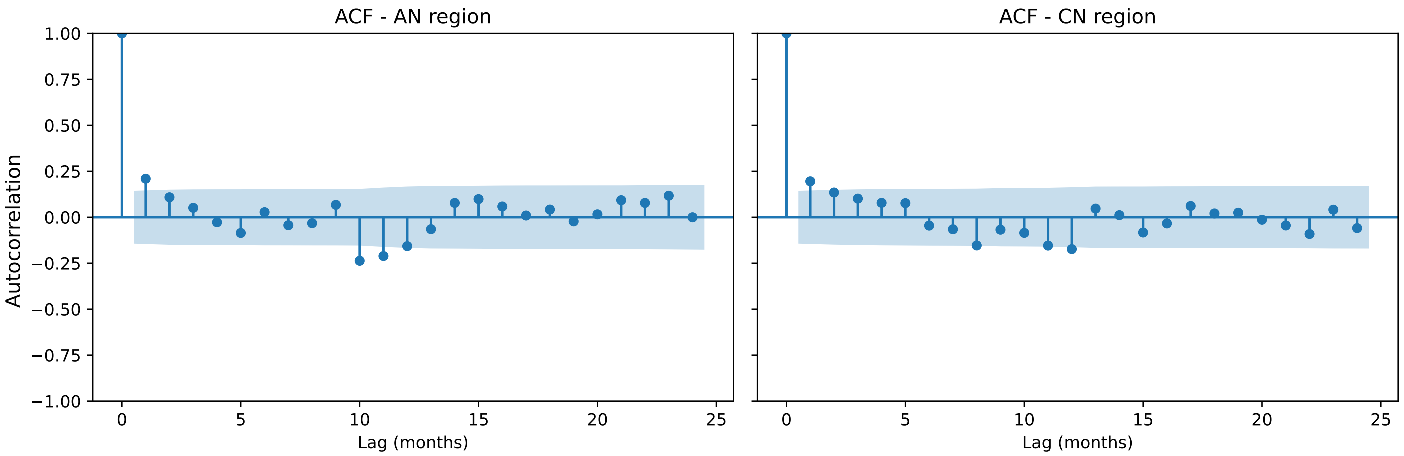

Figure 1: Autocorrelation function (ACF) of the deseasonalized, detrended monthly FLC anomaly time series for the (a) Angolan Namib (AN) and (b) Central Namib (CN) regions (n = 186 months, 2004–2019). Shaded regions indicate the 95% confidence interval under the null hypothesis of no autocorrelation.

4 have markedly lower target variance ($\approx 23\%^2$) than the remaining four folds ($43\%^2$–$58\%^2$). Notably, the model's mean squared error (MSE) in folds 3–4 ($23.5\%^2$–$24.3\%^2$) does not decrease proportionally with this reduced variance, remaining comparable to, or only slightly lower than, MSE values in folds with substantially higher target variance and R^2 (e.g., MSE $= 27.8\%^2$ for a fold with variance $= 43\%^2$ and $R^2 = 0.36$). This suggests that the model exhibits a relatively stable absolute error magnitude across periods with different target variability, resulting in reduced predictive skill for low-variance periods in addition to the mechanical effect of reduced variance on R^2.
- Standard deviation values added in Section 3.1.
- Added in 3.1: "While the AN model shows comparatively stable out-of-sample skill across folds, the CN model exhibits substantially greater fold-to-fold variability in both the ERA5 and MERRA-2 configurations. Two of the six folds, covering July 2009 to August 2014, consistently show near-zero predictive skill regardless of the reanalysis product, coinciding with a period of reduced variance in the observed CN FLC anomalies. The model's absolute error in these folds does not decrease proportionally with the reduced target variability, suggesting a reduced ability to resolve lower-amplitude fluctuations in FLC cover, in addition to the mechanical effect of reduced variance on R^2."

Ln 164: Why only show CN-ERA5? It could be worth discussing one of the AN results as well because some of the findings differ.

We acknowledge that differences exist between the AN and CN sensitivity patterns, largely consistent with the northward shift discussed in Section 3.1 (e.g., the SST sensitivity pattern shifting from the Lüderitz to the Cape Frio upwelling cell). However, these differences do not change the overall picture or conclusions of the study. We therefore prefer to present CN-ERA5 in the main text and keep the remaining configurations (AN-ERA5, CN-MERRA-2, AN-MERRA-2) in the Appendix, as we believe this improves the clarity and focus of the narrative.

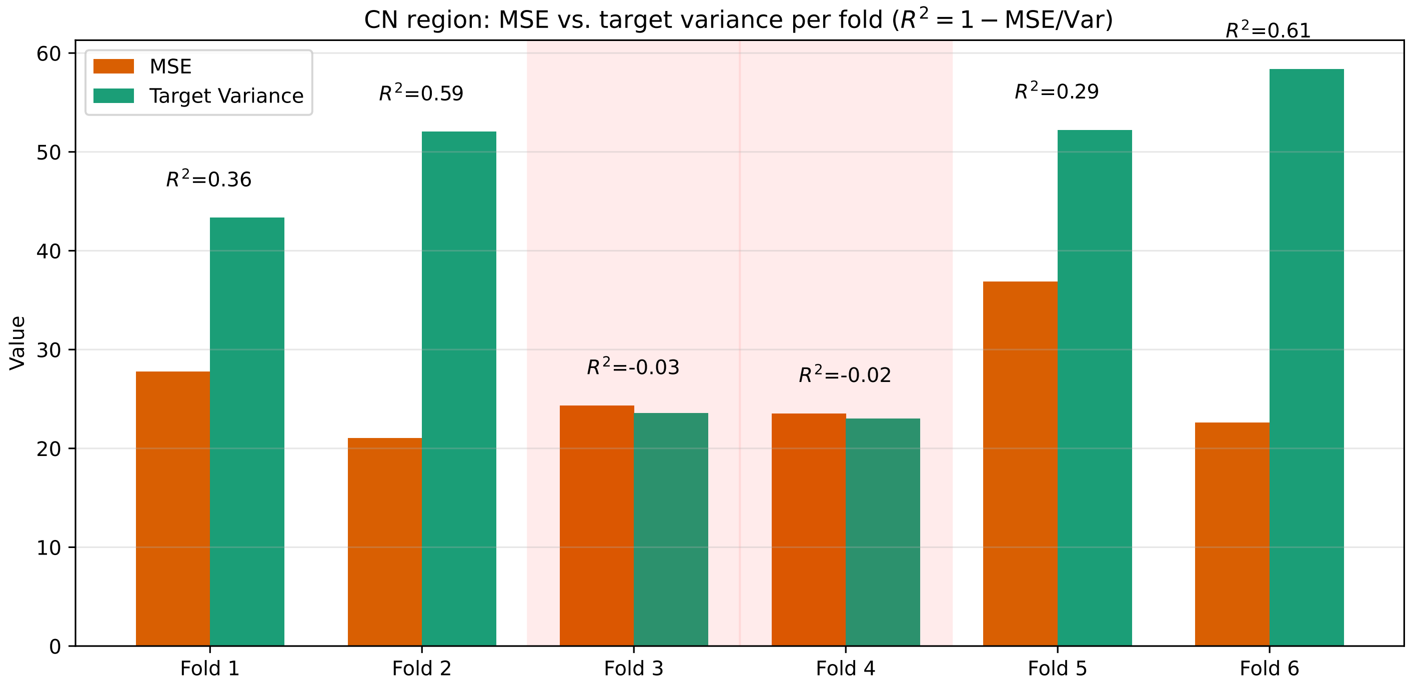

Figure 2: Mean squared error (MSE) and target variance of the observed CN FLC anomaly for each of the 6 cross-validation folds (CN-ERA5 configuration). Folds 3–4 (July 2009–August 2014, highlighted) show target variance roughly half that of the remaining folds, while MSE remains comparable across all folds. R^2 values for each fold are annotated above the corresponding bars.

Ln 190: The negative sensitivity may also be due to the LUCI located to the south of CN.

We considered whether the negative V10 sensitivity could reflect Lüderitz upwelling intensity, given that equatorward (southerly, positive V10) wind is the upwelling-favorable direction along this coast. However, this mechanism would predict a positive, not negative, V10 sensitivity: stronger equatorward wind would intensify upwelling, cool SST south of CN, and increase FLC (consistent with our SST sensitivity), implying a positive association between V10 and FLC through this pathway. Since we observe the opposite sign, we do not believe upwelling intensity via V10 is the primary driver of this sensitivity. This is further supported by the AN configuration, where a similarly negative V10 sensitivity is observed despite the Cape Frio upwelling cell lying much closer to the AN region than the Lüderitz cell lies to CN; if the negative V10 sensitivity were driven by distance-dependent upwelling intensity from a specific upwelling cell, we would expect this sensitivity to differ noticeably between the two regions given their different proximity to their respective upwelling cells. A more consistent explanation is that negative V10 over the region reflects weaker coast-parallel southerly flow, which Andersen et al. (2020) found to be characteristic of fog days in the CN, associated with synoptic-scale disturbances producing a northerly wind anomaly ahead of the trough that favours stratus formation and onshore advection.

Ln 220: How much is the skill improved by averaging annually post-prediction?

We thank the reviewer for prompting us to revisit this. Upon rechecking, we find that annual averaging does not improve skill, contrary to what was stated in the original manuscript (Ln 220); correlation coefficients in fact decrease slightly across all four configurations ($r = 0.66$–0.68 monthly vs. $r = 0.61$–0.64 annual). This discrepancy likely stems from an earlier version of the model used at the time the original text was written. We have therefore modified this sentence in the manuscript.
- Modified in section 3.2: "Averaging the monthly FLC cover anomalies to annual values slightly decreases the correlation with observations ($r = 0.66$–0.68 monthly vs. $r = 0.61$–0.64 annual, across regions and reanalyses)."

Ln 237: What statistical methods are being used to determine the trend? Have the authors considered non-linear trends?

Trends are estimated using ordinary least-squares linear regression (`scipy.stats.linregress`), with statistical significance assessed via the standard OLS p-value. Following this comment, we tested whether a quadratic fit better describes the reconstructed FLC anomaly time series (1982–2019). We compared the two models using the Akaike Information Criterion. For AN, the linear fit was preferred in both reanalyses. For CN, the quadratic fit was modestly favored in both reanalyses. However, a statistically improved quadratic fit does not necessarily correspond to a physically meaningful change in the underlying trend, and we therefore prefer to retain the simpler and consistent linear fit throughout the manuscript.
-Added in Section 2.3: "Trends in the reconstructed FLC anomalies are estimated using ordinary least-squares linear regression. As a robustness check, a quadratic fit to the reconstructed FLC anomaly time series was also tested. The linear fit was preferred for AN, while a quadratic model was slightly favored for CN. However, an improved statistical fit does not necessarily reflect a physically meaningful change in the trend, and therefore the linear fit is retained throughout the manuscript for consistency."

Ln 273: This does make me question why the wind components are in the reconstruction model, maybe justify why it is still important to include them.

Wind components (U10, V10) are retained in the reconstruction model for several reasons. Near-surface wind is a well-established cloud-controlling factor, capturing onshore advection of fog formed over the ocean as well as wind-driven mixing effects on the marine boundary layer (Klein et al., 2018). As noted in our response to the major comment, we have masked land-grid wind values (along with R700), addressing their reduced reliability over land while retaining them over the ocean, where they remain reliable. We find that including these ocean wind components measurably improves the model's predictive skill, supporting their retention as a physically meaningful and useful predictor.
-Added in 2.2: "Finally, U10 and V10 capture near-surface circulation, both the advection of FLCs toward the coast and inland, and boundary layer mixing (Klein et al., 2018)"

Ln 280-281: Another reason the trends are unreliable could be due to the lack of statistical significance as opposed to reanalysis differences.

Added : "The derived trends were found to be unreliable, notably because of discrepancies between ERA5 and MERRA-2, **as well as a general lack of statistical significance in the estimated trends**"

It's a little hard to differentiate the purple and blue on figures 6 and 7, perhaps make the anomalies green as was done in the appendix.

The original color scheme was intended to consistently distinguish ERA5 (purple) from MERRA-2 (green) across figures. We changed ERA5's color to brown throughout the manuscript.

Ln 326:328 I wonder how robust the finding of the correlation between the LUCI/BNI is with the respective AN/CN locations, due to SST anomalies being used to reconstruct the timeseries. I would be curious to see the correlation between any same-sized patch of SST anomalies with the AN/CN anomalies. If the correlation for the LUCI/BNI is significantly higher than any random SST anomaly patch, this could strengthen the findings.

We thank the reviewer for this suggestion. We tested the robustness of the BNI/LUCI–AN/CN correlations by systematically shifting each index's defining box in latitude (northward, southward) and longitude (offshore), and recomputing the correlation with the respective reconstructed AN/CN series at each shift (Figure 3 in this document). For BNI, the correlation remains stable for small shifts and only weakens appreciably once the shift becomes large, consistent with BNI being a spatially broad, literature-defined index (Shannon et al., 1986) situated within one of the strongest SST gradients in the region (Brandt et al., 2024). As BNI is an established index used elsewhere in the literature for comparability, and as even an offshore shift produces only a limited increase in correlation strength, we retain the original BNI definition rather than adjusting its location. For LUCI, the correlation weakens with shifts in most directions, though a slight increase is observed for a northward shift. We attribute this to the fact that upwelling is not confined to the Lüderitz cell: the coastal upwelling band extending north towards the CN region also contributes to FLC variability there, and a northward shift of the box captures it. As this slight increase reflects the contribution of the broader coastal upwelling signal rather than a stronger Lüderitz-specific relationship, and as no shift meaningfully improves on the original definition, we retain LUCI defined directly over the Lüderitz upwelling cell itself (Siddiqui et al., 2023), consistent with its intended physical interpretation
-Added in Section 3.3: "The robustness of the BNI and LUCI definitions to their exact spatial extent was assessed by testing the sensitivity of their correlation with the respective reconstructions to shifts in the defining box location (Appendix C, Figure C3). BNI shows stable correlation for small shifts, weakening only for large shifts, consistent with its broad, literature-defined extent within a strong regional SST gradient. LUCI shows a slight increase in correlation for a northward shift. The Lüderitz cell itself is fixed in location, but upwelling is not confined to it: the coastal upwelling band extending north towards the CN region also matters for FLC variability there, and a northward shift captures it. The increase therefore reflects the broader coastal upwelling signal rather than a stronger Lüderitz-specific relationship."

Also, it would be helpful to mention the periodicity of LUCI/BNI. Does it vary on short or long timescales?

Regarding periodicity, major Benguela Niño events typically persist for six months or longer, with some events (e.g., 1963) lasting close to a full year (Shannon et al., 1986); documented cases include the 1934 event, where the warm water intrusion persisted for about six months, and the 1984 event, associated with a raised sea-level anomaly lasting almost seven months. We note that the Lüderitz upwelling cell relevant to the LUCI index persists year-round as a physical feature, with intensity that is generally weaker in winter and spring (Andrews and Hutchings, 1980; Hutchings et al., 2009); the LUCI index itself, however, represents a deseasonalized SST anomaly and therefore fluctuates around zero rather than reflecting the permanent presence of the underlying upwelling process.
-Added in Secion 3.3: "BNI and LUCI vary primarily on multi-month timescales. Major Benguela Niño events typically

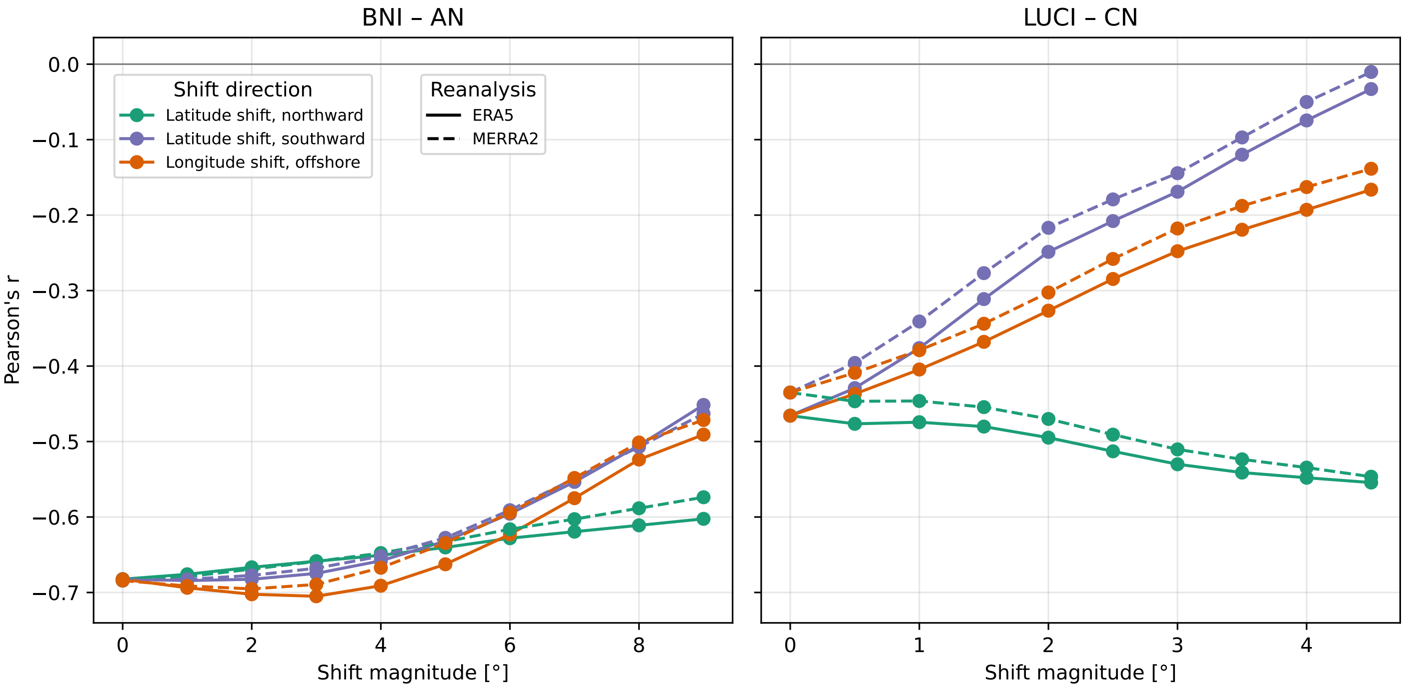

Figure 3: Sensitivity of the Pearson correlation between the BNI/LUCI indices and the reconstructed AN/CN FLC anomalies to shifts in the defining box location. The box is shifted northward, southward, and offshore (westward) from its original location (0° shift), in 1° increments for BNI and 0.5° increments for LUCI, for both the ERA5 (solid) and MERRA-2 (dashed) configurations. **(a)** BNI–AN. **(b)** LUCI–CN.

persist for six months or longer, with some events lasting close to a full year (Shannon et al., 1986). The Lüderitz upwelling cell relevant to the LUCI index persists year-round, with intensity generally weaker in winter and spring (Andrews and Hutchings, 1980; Hutchings et al., 2009). The robustness of the BNI and LUCI definitions to their exact spatial extent was assessed by testing the sensitivity of their correlation with the respective reconstructions to shifts in the defining box location (Fig. C3). BNI shows stable correlation for small shifts, weakening only for large shifts, consistent with its broad, literature-defined extent within a strong regional SST gradient. LUCI shows a slight increase in correlation for a northward shift, likely reflecting partial overlap with the BNI-related tropical warm-water intrusion signal rather than a stronger Lüderitz-specific relationship."
-Added figure C3

Appendix Figure B2

1. *Clouds are most frequent September to March (ln 29), some of these months are missing from the analysis, any reason?*
2. *Does this analysis just compare the closest grid cell to the Marble Koppie station?*

3. Why show the wind direction and not directly the u10 and v10 components?

1. Regarding the missing months: this is a good suggestion; we had not previously examined the diurnal wind direction cycle beyond DJF and JJA. The original focus on DJF and JJA was motivated by the contrasting summer and winter diurnal cycles described for the central Namib by Lindesay and Tyson (1990), which our comparison aimed to test the reanalyses against. We have added a second row to Fig. B2 (panels d–f) covering MAM and SON. The results show a similar bias between the station observations and the reanalyses as seen in DJF/JJA, though with a larger overlap in the interquartile ranges for MAM/SON compared to the clearer separation seen in DJF/JJA.
2. Regarding the grid cell used: yes, this analysis compares the nearest ERA5 and MERRA-2 grid cells to the Marble Koppie station. We have added the exact coordinates of the station and the two nearest grid cells to the figure.
3. Wind direction was originally shown as a simplified, easily interpretable summary of the wind field, without a wind speed dimension. We agree this does not fully convey the u10/v10 information and, in response, have added seasonal wind roses (Fig. B3) for the station and both reanalyses, showing the joint distribution of wind direction and speed. Together with the diurnal wind direction cycle (Fig. B2), this now provides a more complete picture of the wind field, including a strong bias between the station observations and the reanalyses that is consistent across both figures.

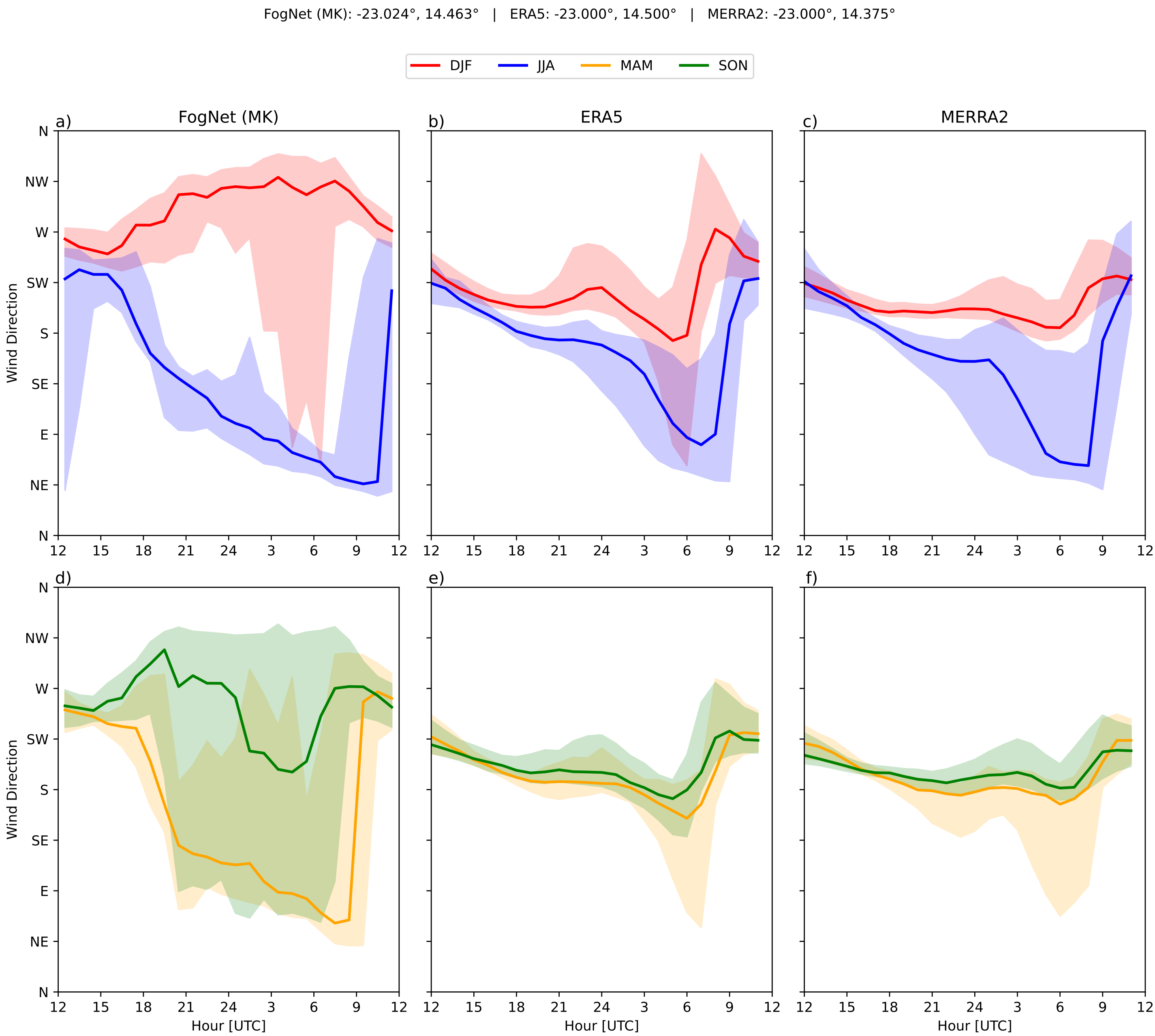

Figure 4: Seasonally averaged diurnal wind direction cycle for 2016 (red: DJF; blue: JJA; orange: MAM; green: SON). Shaded areas indicate the interquartile range (25th–75th percentile) for each hour. Top row: **(a)** FogNet station (Marble Koppie: 23.024°S, 14.463°E), **(b)** ERA5 (nearest grid cell), **(c)** MERRA-2 (nearest grid cell). Bottom row: **(d–f)** as in (a–c), for MAM and SON.

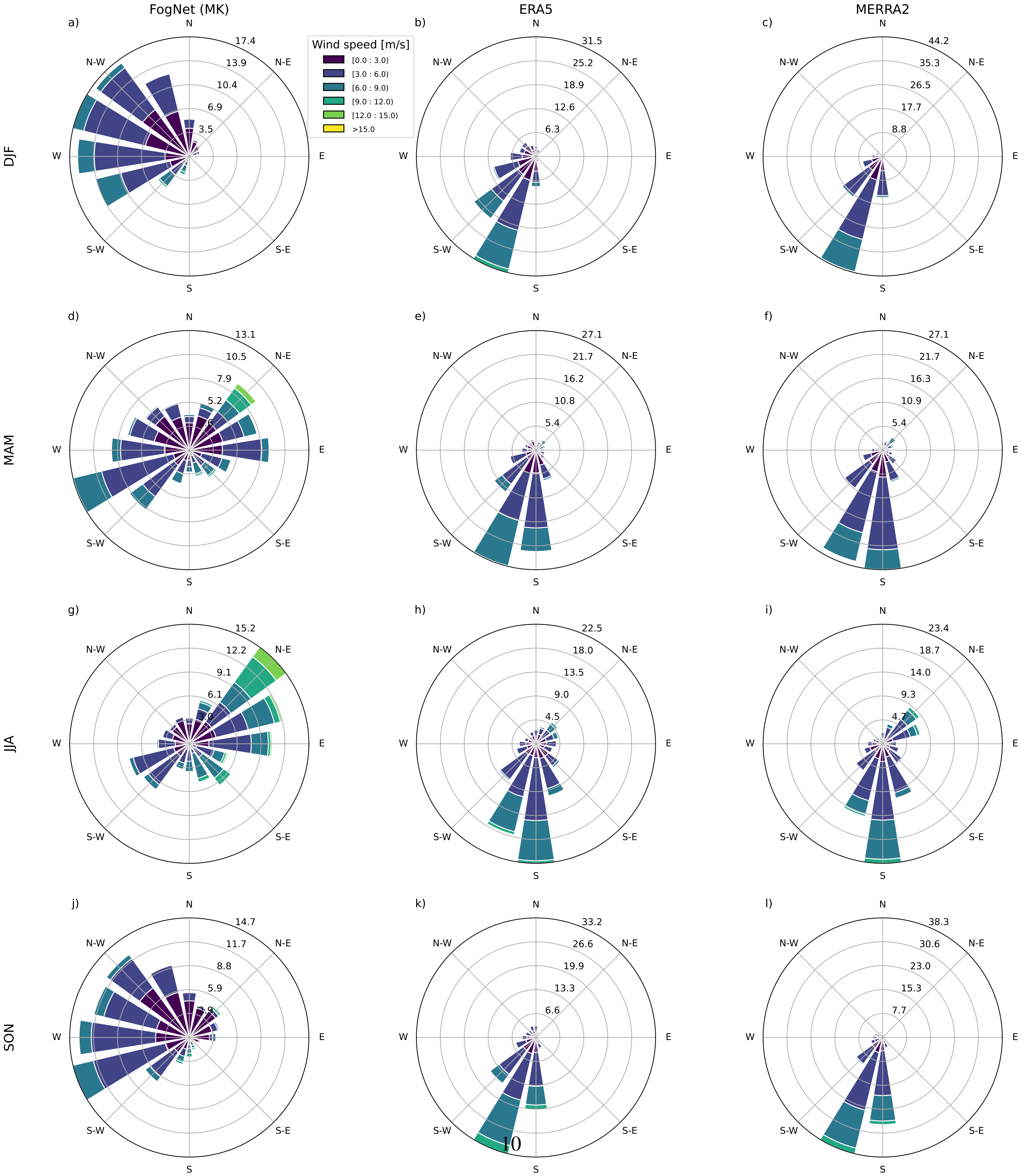

Figure 5: Wind roses showing the joint distribution of wind direction and speed for the FogNet station (Marble Koppie), ERA5, and MERRA-2, separated by season (rows: DJF, MAM, JJA, SON) for 2016. Bars indicate the frequency of occurrence (%) for each wind direction sector, colored by wind speed bin (m/s).

A Old vs New Figures 3 & 4

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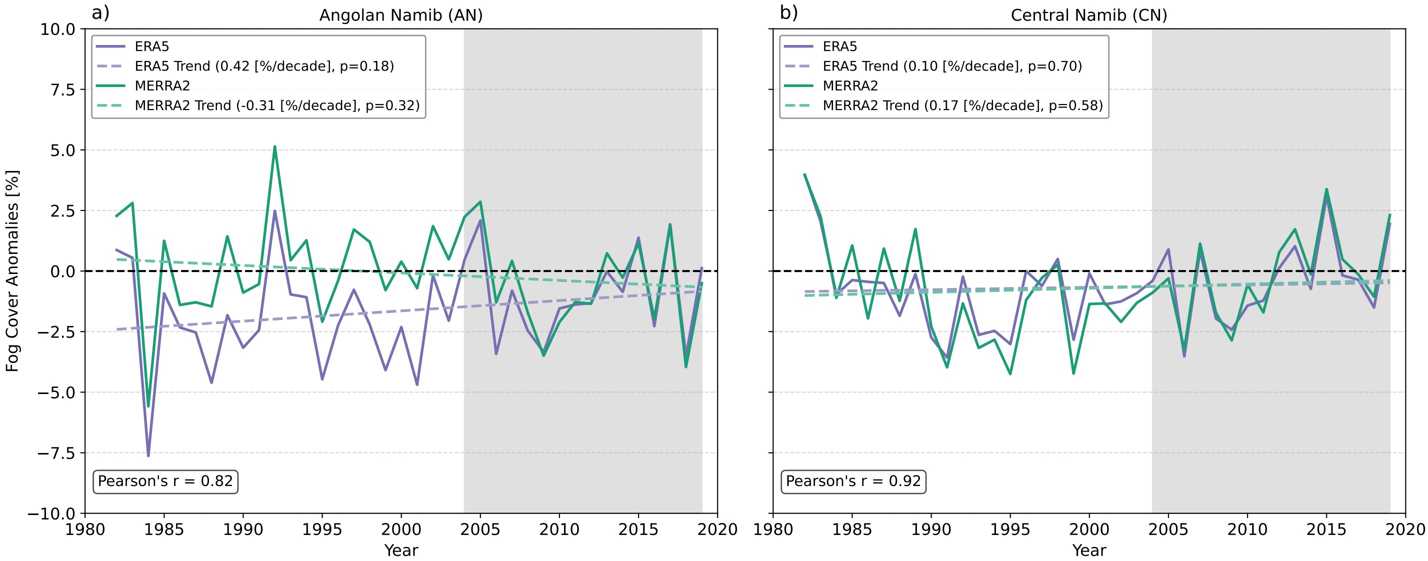

Figure 6: Old version of Figure 3 (with R700, V10 and V10 predictors field not masked over land).

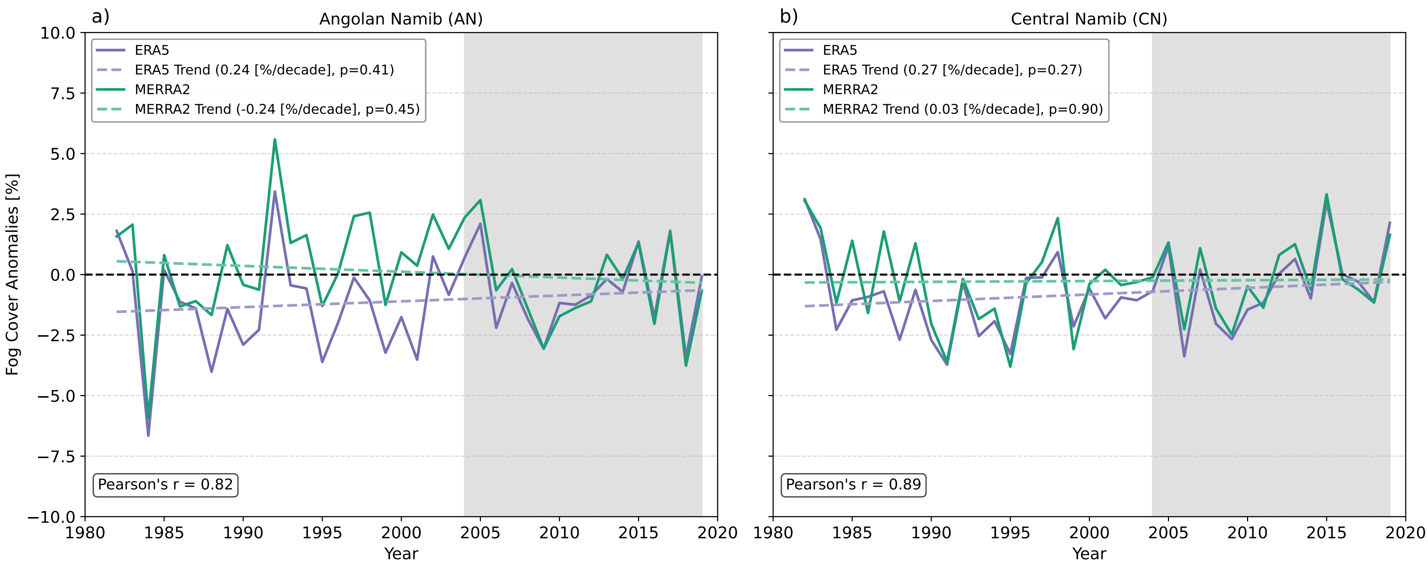

Figure 7: New version of Figure 3 (with R700, V10 and V10 predictors field masked over land).

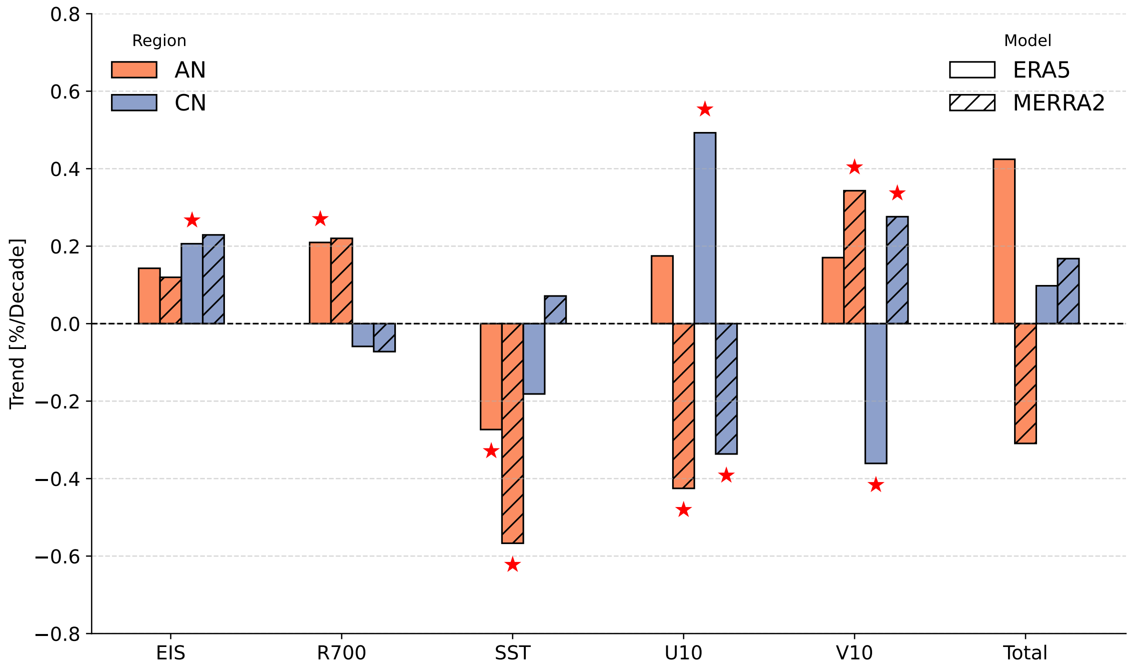

Figure 8: Old version of Figure 4 (with R700, V10 and V10 predictors field not masked over land).

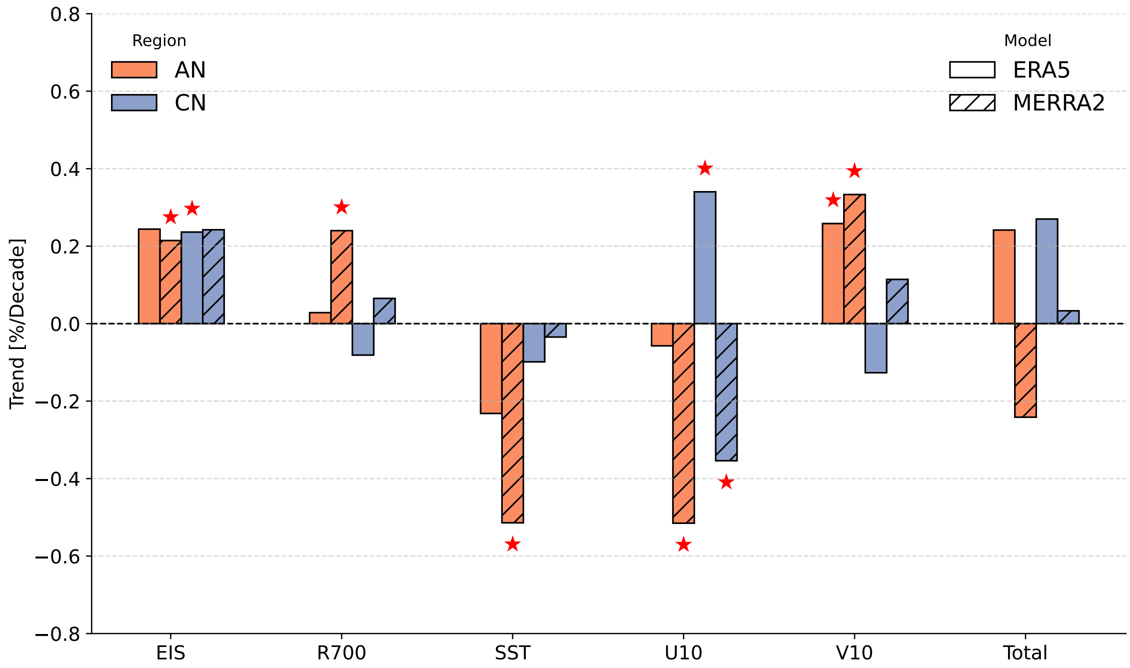

Figure 9: New version of Figure 4 (with R700, V10 and V10 predictors field masked over land).

Response to comments from reviewer 2

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

First of all, we would like to thank the reviewer for the positive comments and for providing very helpful and constructive feedback. We thank the four anontymous reviewers in the acknowledgments of the updated version of the manuscript. The comments and suggestions from reviewer 2 are incorporated in italics and addressed by the authors in blue.

1 Specific Comments

Methods –The differences between the winds is an interesting result! Figure B2 highlights the lack of the diurnal cycle tracking for DJF, but it seems that previous papers indicate log-fog season in April-June and high-fog season September-November? Any reason the cycle isn't grouped by fog-season?

We thank the reviewer for this comment. The motivation for this figure was to check whether the reanalyses reproduce the diurnal cycle of the wind field and its pronounced summer/winter differences, which are described for the central Namib by Lindesay and Tyson (1990). In response to a related comment from Reviewer 1, we have already expanded Fig. B2 to include all four seasons (DJF, MAM, JJA, SON; Fig. B2 panels a–f) and added seasonal wind roses (Fig. B3) showing the joint distribution of wind direction and speed. We also examined the diurnal wind direction cycle grouped specifically by fog season rather than by calendar season, and found the results to be really similar to those obtained with the calendar-season grouping already shown. We therefore retain the standard calendar-season grouping (DJF/MAM/JJA/SON) throughout, as this is more general and allows for direct, consistent comparison across seasons rather than being tied to a fog-specific definition.

From Figure 4, also related to the winds– Since the bars are the contribution from each component, and SST data is the same, is the MERRA2 AN linear model therefore more sensitive to SST than its ERA5 counterpart? Could this mean that when constructing historical interannual variability, that ERA5 is more sensitive to low-level winds, and therefore has a higher correlation to El Niño if the primary mechanism is indeed strengthened northwesterly winds (sort of apparent in higher r values in line 289). Would it be possible to make the weight of the SST sensitivity the same between ERA5 and MERRA2, and might that indicate which reanalysis wind is more sensitive to the significant land warming (`https://wires.onlinelibrary.wiley.com/doi/full/10.1002/wcc.70025`), or would that excessively disrupt the construction of the model? Are there any previous cases from higher-temporal scales (diurnal, synoptic) that point to more fidelity in a particular reanalysis?

The SST data (NOAA OI SST) is indeed identical between the ERA5 and MERRA-2 configurations. The SST bar in Fig. 4 shows the linear trend of the partial reconstruction obtained by applying the estimated SST sensitivities to the SST anomaly field; since that anomaly field is identical in both configurations, any difference in the resulting trend contribution must originate in the estimated sensitivities themselves. However, we do not think this reflects a genuinely stronger physical sensitivity to SST in the AN-MERRA-2 model. In ridge regression, each predictor's coefficient is estimated jointly with all others under a shared regularization penalty, so its value depends on the collinearity structure among them. Because the SST predictor is identical in both configurations, differences in its attributed contribution arise from differences in its covariance with the other predictors, which do differ between the ERA5 and MERRA-2 configurations (e.g., winds, EIS, R700).

Regarding the proposed experiment: while equalizing the SST weight between the two configurations could in principle be performed, its interpretation would be difficult. The remaining coefficients would still depend on the differing covariance structures among predictors in the two reanalysis configurations, so the "leftover" wind coefficient would not isolate a clean, physically-interpretable wind sensitivity. We have therefore not pursued this experiment, though we recognize it as a interesting idea in principle.

Regarding the possible ENSO–wind–FLC link: as noted in the manuscript (Sect. 3.3), we consider this connection suggestive but not firmly established. Rouault and Tomety (2022) report weak, positive correlations between ENSO and coastal upwelling, attributed to weaker-than-normal upwelling-favorable southeasterly winds during ENSO events, and Li et al. (2025) find that more frequent, stronger northwesterly winds during ENSO years intensified fog over the past decade, though at annual rather than monthly resolution. We agree the higher ENSO correlation in the ERA5 configuration is plausibly related to differences in simulated wind sensitivity between the reanalyses, but given the current evidence we would rather flag this as an open question than overstate what the current evidence supports.

Regarding reanalysis fidelity for winds at higher temporal resolution: we are not aware of a study that directly evaluates diurnal- or synoptic-scale wind fidelity between ERA5 and MERRA-2 specifically for the Benguela/Namib coastal region. More broadly, Gruber et al. (2022) report that ERA5-based wind power simulations correlate better with observed generation than MERRA-2 across several countries, including South Africa, though the difference in error (RMSE) between the two reanalyses was not statistically significant for South Africa specifically. Patricola and Chang (2017) further show that coarse-resolution reanalyses generally struggle to represent the structure of the Benguela low-level coastal jet itself, missing its characteristic near-shore velocity maxima. Taken together, we do not think the existing literature supports a clean, region-specific conclusion that one reanalysis's winds are simply "more faithful" for this setting.

-Added in Section 3.2: "Note that the relative magnitude of each predictor's contribution shown in Fig. 4 depends on the ridge-regression sensitivities, which are estimated jointly across all predictors under the shared l^2-norm regularization term and are therefore shaped by the collinearity structure among predictors. Consequently, a difference between reanalyses in a given predictor's contribution should not, on its own, be taken as evidence of a difference in that variable's underlying physical importance."

Section 3.2, line 220 – Any further information on the annual FLC anomalies would be a great addition for the reader. Such as, do annual anomalies follow the seasonal cycle for the AN, so a positive anomaly means more fog in the peak season, or a slightly longer season (to the extent it's tractable in monthly data)? Similarly for the CN, do any months consistently appear from the historical reconstruction in years with strong reductions?

We thank the reviewer for this suggestion. We address this using the satellite-observed FLC record (2004–2019) rather than the 1982–2019 model-based reconstructions, as the satellite observations provide a direct record and are not subject to uncertainties associated with the reconstruction model. To investigate this, we correlated each calendar month's FLC anomaly with the annual mean anomaly across all years. This quantifies the association between monthly variability and the overall annual signal across the full record (Fig. 1, in this document).

In AN, the fog season exhibits a strong and well-defined seasonal cycle. Significant positive correlations are found in several months, indicating that annual anomalies are associated with variability occurring during different parts of the year. Within the fog season, the strongest correlations occur in June and July, when the season is still developing toward its maximum, whereas August and September, despite having the highest climatological fog occurrence, show weaker relationships with the annual anomaly. This suggests that interannual variability in AN is primarily associated with changes during the early development of the fog season, which may reflect variations in the strength and/or timing of the seasonal build-up.

We further examined whether strong and weak anomaly years are associated with changes in season length and found only minor differences, suggesting that substantial changes in season duration do not dominate the annual anomalies.
In CN, fog occurs throughout the year with a weaker seasonal cycle. The annual anomaly is most strongly associated with variability outside the climatological peak season, particularly in January, March, and April. In contrast, the peak season shows little relationship with the annual anomaly, suggesting that interannual variability in CN is primarily linked to changes in fog occurrence during non-peak months rather than variability during the climatological maximum.
-Added in Section 3.2: "To better characterize these annual anomalies, we examine which months they are primarily associated with and whether they reflect changes in the intensity of the fog season or shifts in its timing or duration. Using the satellite-observed FLC record (2004–2019), we correlated each calendar month's FLC anomaly with the annual mean anomaly across all years to identify which months' variability is most closely linked to the overall annual signal (Fig.B2. In AN, significant positive correlations occur in several months, with the strongest relationships found in June and July, when the fog season is still developing toward its maximum. In contrast, August and September, despite having the highest climatological fog occurrence, show weaker relationships with the annual anomaly. This indicates that AN annual anomalies are primarily associated with variability during the early development of the fog season, potentially reflecting changes in the strength and/or timing of seasonal build-up rather than substantial changes in season duration. In CN, where fog occurs throughout the year with a weaker seasonal cycle, annual anomalies are most strongly associated with variability outside the climatological peak season, particularly in January, March, and April, while the peak period contributes comparatively little to interannual variability."
-Added figure B2

Section 3.3 – As a suggestion, I would restructure section 3.3 to lead with the BNI influence (lines 303-335) and Figure 7, as it ties into the regional climate more directly than ENSO (and reflects the title of the manuscript).

We thank the reviewer for this suggestion and appreciate the rationale for leading with the BNI influence, as it is more directly connected to the regional climate context and the focus of the manuscript. However, we prefer to retain the current structure and introduce the ONI influence first. This is because the BNI is defined in a similar way to the ONI, and the ONI is a more widely recognized climate index. Presenting the ONI first therefore provides a more accessible introduction and allows us to place our results within the literature before discussing the more regionally specific BNI influence. In addition, we find that this structure provides a more progressive narrative, introducing the established large-scale climate mode first and then concluding the section with the stronger and more regionally relevant BNI/LUCI results.

2 Technical Comments

Abstract, Line 12: I would start the conclusions of the interannual variability section with the last conclusion, "..half of the variability in Angolan Namib comes from Benguela Niño events..". I would also include the % increase/reduction in FLC occurrence during El Niño years for reader context.

We agree that Benguela Niño events represent the stronger and more regionally relevant control on interannual FLC variability and have therefore revised the abstract to highlight this result before discussing ENSO. Specifically, we now introduce Benguela Niño events first . We also clarified that the ENSO-related enhancement is associated with El Niño con-

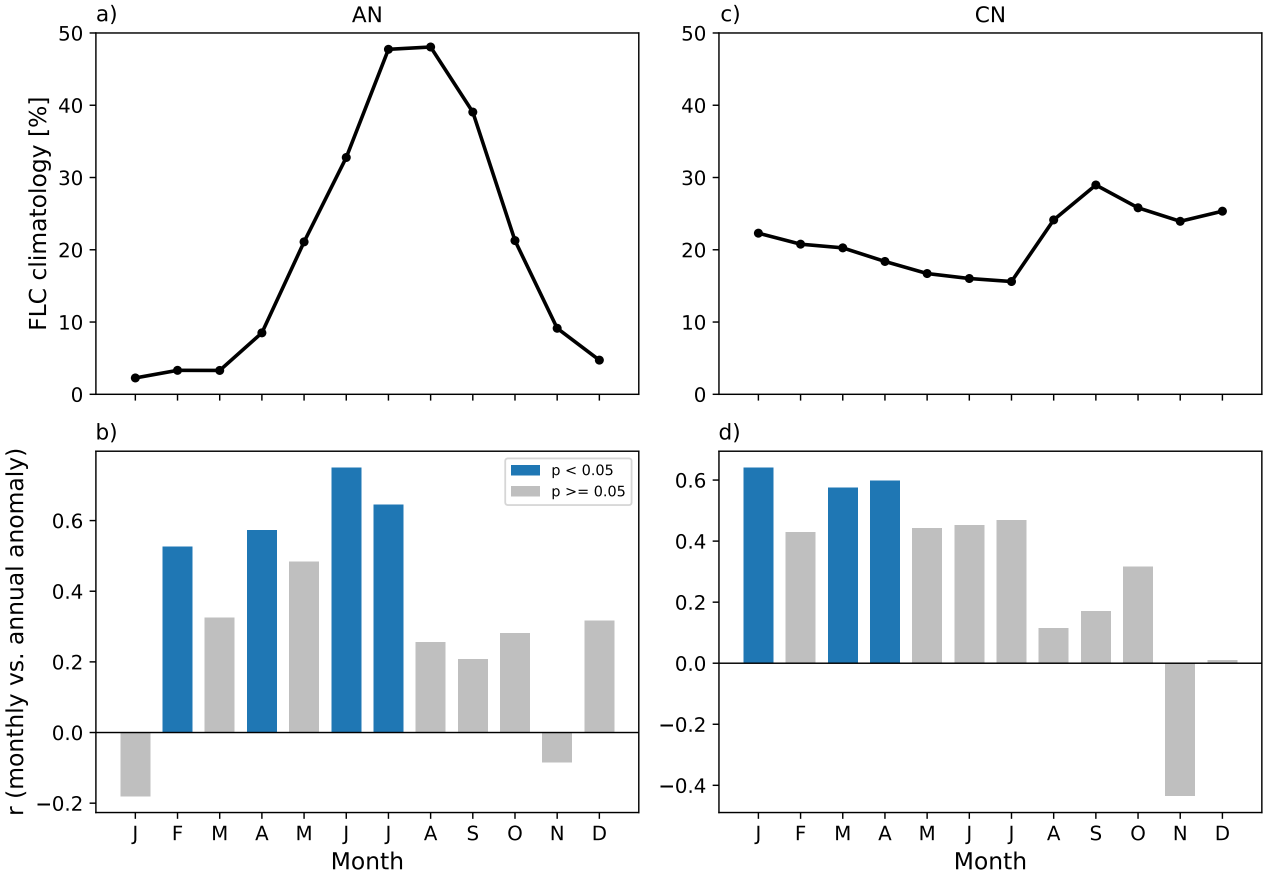

Figure 1: Based on the satellite-observed FLC record (2004–2019). (a, c) Average monthly cycle of FLC cover for AN and CN. (b, d) Correlation (r) between each month's FLC anomaly and the annual mean anomaly, for AN and CN; blue bars indicate $p < 0.05$.

ditions. We decided not to include the exact percentage change in FLC occurrence during El Niño years in the abstract because the ENSO influence is comparatively weak and not consistently significant across the different reconstruction datasets.

-Modified in the abstract: "Finally, the reconstructions are used to assess interannual variability and its links to climate modes. Benguela Niño events, associated with tropical water intrusion and SST warming, explain up to half of the interannual FLC variability in the Angolan Namib. El Niño conditions also enhance FLC occurrence, but their influence is much weaker."

Ln 190: Introduction, Line 50: It seems the Li (2025) reference sets up your study analyzing spatial drivers of interannual variability as a next step nicely (decrease in fog water since 1996, some discrepancies in El Nino periods before and after this period with in-situ observations, although only in one location). If desired, one could add another sentence here to *supply some of those motivations to the reader? Or maybe just the mechanism they supply that increases fog occurrence, and potential limitations? Some results later in this work reference the Li paper, so a bit more context would strengthen the impact of this study's results.*

We agree that additional context on the Li et al. (2025) study helps motivate the analysis of interannual variability presented here. We have added a sentence in the Introduction describing the proposed mechanisms linking El Niño to enhanced fog occurrence, namely increased moisture advection associated with stronger northwesterly winds and enhanced local moisture availability through vegetation transpiration. We also highlight the limitation that the reported El Niño signal is based on a single in-situ location and is mainly evident during the most recent decade of the record, motivating our spatially resolved analysis of FLC variability across the Namib region using satellite observations and reanalysis-based reconstructions.
-Added in the Introduction: "Additionally, teleconnections to El Niño-Southern Oscillation (ENSO) have been suggested to enhance fog occurrence in the Central Namib through increased moisture advection associated with more frequent northwesterly winds and enhanced local moisture availability via vegetation transpiration during El Niño conditions (Li et al., 2025). However, this relationship is based on observations from a single location and is mainly evident during the most recent decade of the record, motivating further investigation of the spatial extent and robustness of ENSO-related FLC variability across the Namib desert."

2.2, Line 102: Long term warming trend removed from BNI? Maybe mention the value in the case the reader is curious?

We agree that providing the magnitude of the removed long-term trend improves transparency. We have added that the linear trend removed from the BNI time series corresponds to $0.33°\mathrm{C} \cdot \mathrm{decade}^{-1}$.

3.1, Figure 2, Line 192: Add titles to subplots, even if small text, just makes looking at them together easier.

We have added predictor titles to Figure 2 and the corresponding figures in Appendix A.

3., Line 194: Add parentheses in Table 1 after SST (NOAA OI) so reader can't mistake reanalysis SST being used.

Done.

Figure 3 – Is there a time series of predicted vs observed FLC occurrence for 2003-2020 using the network? Maybe add in dots to both panels to show spread in observed fog anomalies. It would be nice to see observations next to what the network predicts. Any additional context to the 1996 divide posited by Li et al (2025) that you can add would be great, since the CN region includes Gobabeb research station, correct?

As noted in the manuscript, the final reconstruction (Fig. 3) uses fold-averaged sensitivities, in which each month from 2004–2019 contributes to the training data in all but one of the six cross-validation folds, making the resulting reconstruction non-independent. A fully out-of-sample reconstruction can instead be obtained by combining the predictions from all test folds, yielding correlations with the observations of $r = 0.64$ for AN-ERA5, $r = 0.62$ for AN-MERRA-2, $r = 0.61$ for CN-ERA5, and $r = 0.63$ for CN-MERRA-2. Using fold-averaged sensitivities ensures a continuous time series from 1982

to 2019, which allows the analysis of interannual variability and long-term trends, but as these fold-averaged values are not fully independent, we consider it potentially misleading to overlay the observations directly on Fig. 3.
To address the reviewer's request while keeping this distinction clear, we have added Fig. B1(Appendix), showing the observations against both the fully out-of-sample prediction ($\hat{y}$) and the fold-averaged reconstruction, at annual resolution, for both regions and reanalyses. As expected, the fold-averaged reconstruction correlates much more closely with the observations ($r = 0.88$–0.94) than the genuinely out-of-sample prediction does ($r = 0.61$–0.64).
Regarding the potential additional context related to the post-1996 changes reported by Li et al. (2025): although Gobabeb falls within the latitudinal band of our CN region, it lies some 60 km inland and therefore outside our study domain, which is restricted to the coastal strip within 24 km of the coastline. We have chosen not to directly compare our reconstruction with the specific pre-/post-1996 breakpoint identified in that study. As discussed in the manuscript, the reconstructed long-term trends are subject to substantial uncertainty, both due to differences between reanalysis products and the general lack of statistically significant trends. Even accounting for this uncertainty, we find no indication of a break point in our reconstructed time series corresponding to the one reported by Li et al. (2025), and we do not consider the reconstruction sufficiently robust for assessing a specific historical breakpoint on its own. Furthermore, the breakpoint identified by Li et al. (2025) is derived from a single long-term in-situ fog record at Gobabeb, whereas our CN reconstruction represents a regional-average FLC signal over the coastal domain; a change observed at an inland site outside this domain would not necessarily be expected to emerge in our reconstruction. Rather, our statistical framework is designed to quantify the sensitivities of FLC variability to large-scale cloud-controlling factors (SST, EIS, wind, and atmospheric moisture) and to investigate their role in interannual variability and long-term tendencies.
-Added Figure B1

References

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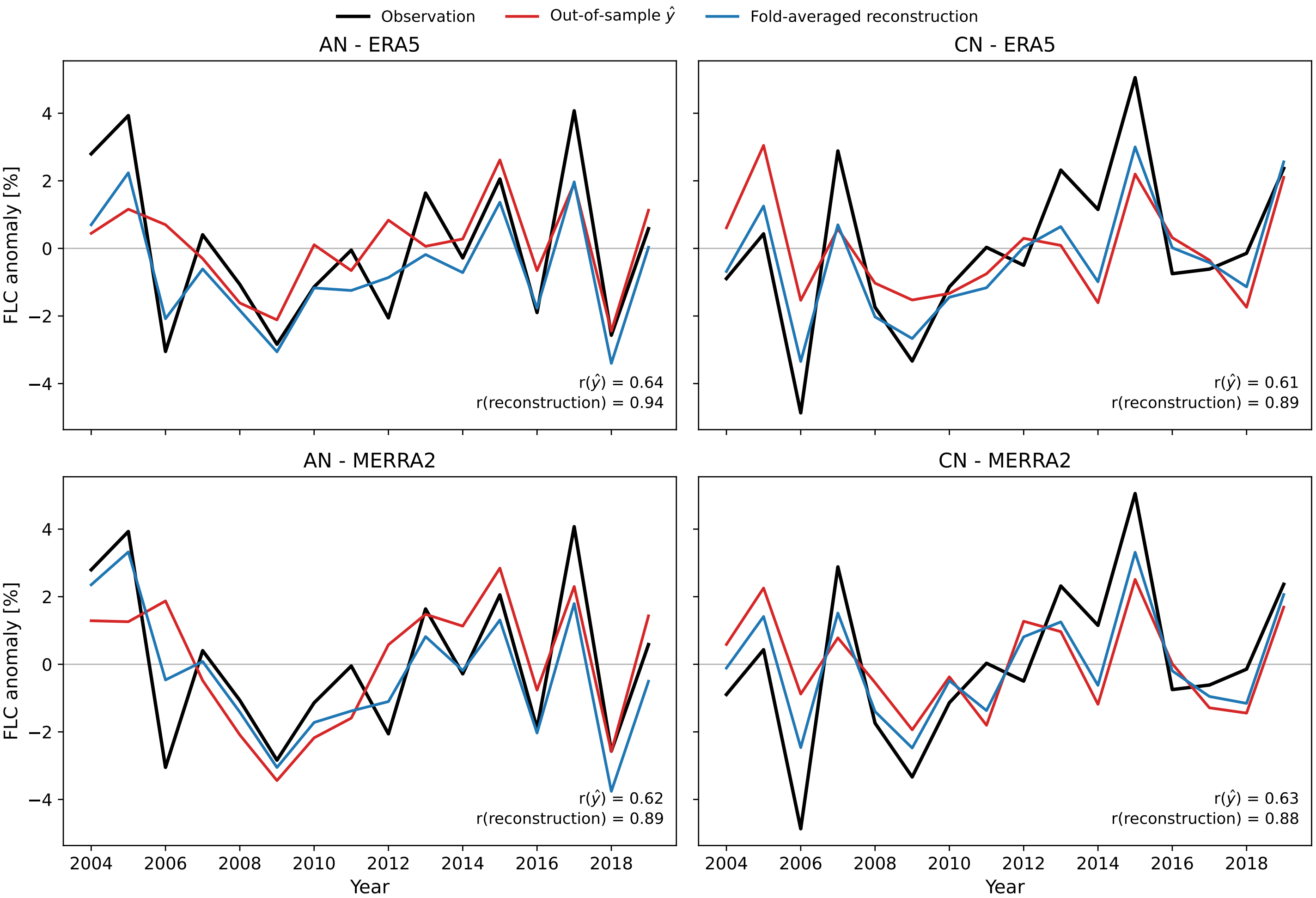

Figure 2: Comparison of observed annual FLC anomalies against the fully out-of-sample prediction ($\hat{y}$, red) and the fold-averaged reconstruction used in Fig. 3 (blue), for each region and reanalysis. The fold-averaged reconstruction is not independent of the observations and correlates more closely with them than the genuinely out-of-sample prediction, illustrating why Fig. 3 does not overlay observations directly.

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 covariability 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.”

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Response to comments from reviewer 4

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

First of all, we would like to thank the reviewer for the positive comments and for providing very helpful and constructive feedback. We thank the four anonymous reviewers in the acknowledgments of the updated version of the manuscript. The comments and suggestions from reviewer 4 are incorporated in italics and addressed by the authors in blue.

1 General Comments

Ln25-28: This paragraph uses several terms that seem to describe similar phenomena (e.g., "advection fog", and "advected marine stratus clouds"). Clarifying the distinctions between these terms, if any, and using consistent terminology would improve readability.

We thank the reviewer for this comment. We distinguish coastal advection fog, which forms through near-surface cooling of moist marine air, from inland "high fog," associated with advected marine stratus clouds whose base intersects the surface further inland; both are generally regarded as marine and advective phenomena but differ in where saturation occurs and in the atmospheric processes producing surface fog. We clarify in the revised text that these two types are grouped together as FLC both because they are predominantly marine in origin and because our satellite-based product cannot reliably distinguish between them.

Ln65-67: Validation against surface observations is reported to demonstrate good performance of the FLC detection algorithm; however, the specific surface observation datasets used are not specified. Please clarify which observations were used.

The validation referred to here uses nighttime surface net radiation measurements from the FogNet station network (Kaspar et al., 2015), part of the SASSCAL initiative, in the central Namib (2015–2017), as described in Andersen and Cermak (2018). Nighttime net radiation accurately captures the presence of low cloud, which corresponds to what the satellite product detects. We have revised the sentence to specify this dataset explicitly, rather than referring to "surface observations" without naming the source.

Ln75-77: It is unclear what is meant by "spatially and temporally averaged" FLC cover. Please clarify whether this corresponds to a domain-averaged time series and specify the spatial domain used for each zone.

"Spatially and temporally averaged" refers to a single domain-averaged monthly time series per region, derived in two steps from the daily, cell-based FLC cover (itself already a temporal average of the native 15-min classification, over each 24-hour period from 12:00 UTC). First, the daily cell-based values are spatially averaged over all valid grid cells within each region's domain (AN: 15–17 °S; CN: 22–24 °S, both restricted to grid cells within 24 km of the coastline), yielding one daily value per region. These daily regional values are then temporally averaged over each calendar month to produce the monthly FLC cover time series used as the predictand. We have revised the text to state this explicitly and to specify the spatial domain for each region.

2 Section 2.2 Cloud controlling factors

Ln82-89: The section introduces five controlling factors supported by literature, but these are also referred to as predictor variables (e.g. EIS, relative humidity at 700 hPa). It would be helpful clarify the physical processes represented by each variable and their relationship to FLC variability, rather than only listing them as predictors. In particular, linking these variables to processes such as cloud-top radiative cooling and entrainment, and how these processes may influence EIS, would improve the physical interpretation.

We thank the reviewer for this suggestion. We agree that the original text focused primarily on the predictor variables themselves rather than the physical processes they represent. We have therefore expanded Sect. 2.2 to briefly describe the physical interpretation of each cloud-controlling factor, including the role of EIS in regulating boundary-layer mixing and cloud persistence, the influence of free-tropospheric humidity on cloud-top radiative cooling and entrainment, the thermodynamic role of SST, and the representation of fog and low-cloud advection through the wind components, which additionally carry information on marine boundary layer dynamics.

Ln86-87: Subsidence is excluded from the final set of predictors because it does not improve model skill. However, this factor has been used in previous studies cited in the manuscript (e.g., Klein et al., 2018; Scott et al, 2020; Ceppi&Nowack,2021; Andersen et al., 2022). It is very important to provide an explanation for its exclusion beyond the model skill since subsidence is one of the main physical processes controlling marine low stratus formation in tropical oceans.

We agree that subsidence is an important component of the large-scale circulation. Our intention was therefore not to imply that subsidence is physically unimportant, and we have clarified this point in the revised manuscript. Rather, we tested whether the reanalysis-derived W700 field available to us provides additional predictive information for FLC cover anomalies beyond the five predictors already included.

In our experiments, it did not. We tested model skill both with and without W700 in the predictor set, and additionally trained a model using W700 alone. Adding W700 to the five-predictor model did not improve skill, and the W700-only model did not provide predictive skill. The ridge coefficient fields from the six-predictor model (Fig. 1 in this document), shown on the ERA5 grid, provide some insight into this result: EIS, R700, SST, U10, and V10 exhibit coherent coastal structures, whereas W700 coefficients are comparatively patchy and lack a clear physical interpretation. The reason for this is not established, though the patchiness of the W700 coefficients suggests the field may be less well constrained at the spatial scale considered here. A further possibility is that some aspects associated with subsidence are already represented indirectly by the other predictors, given their covariability.

We emphasize that the statistical model is an imperfect representation of the processes controlling FLC cover anomalies. W700 was excluded because the available reanalysis product did not provide useful information at the spatial and temporal scales investigated here, not because we conclude that subsidence is unimportant for FLCs.

-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."

Ln101: The use of the 1991-2020 period as the reference climatology for SST anomalies is not fully justified. Could you

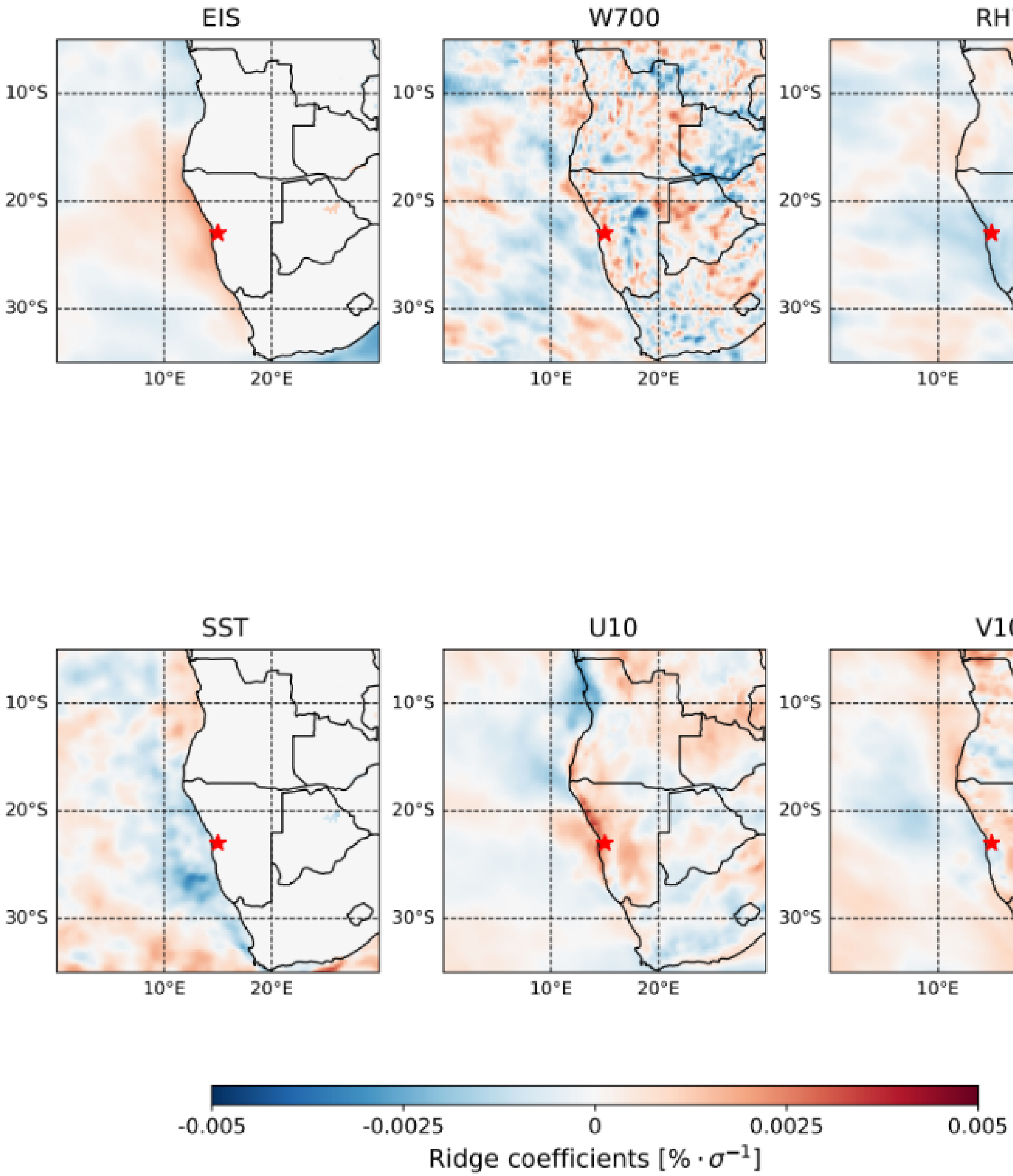

Figure 1: Ridge coefficients [% $\cdot\sigma^{-1}$] for the six-predictor model, including W700, for the CN region on the ERA5 grid. EIS, R700, SST, U10, and V10 show spatially coherent structures concentrated near the coast, whereas the W700 coefficients are comparatively patchy and lack a clear physical interpretation at this spatial scale. The red star marks the Central Namib (CN) region.

clarify whether this choice affects the results and why it was preferred over the 1982-2019 period?

The 1991–2020 period was chosen to follow the BNI definition of McPhaden et al. (2024), which we adopt here, and because it corresponds to the base period used by NOAA for the ONI, so that the BNI, LUCI, and ONI indices are all defined relative to a common climatology and remain directly comparable. The choice of base period shifts the monthly anomalies by a near-constant offset and therefore has little effect on interannual variability. Recomputing both indices relative to a 1982–2019 climatology yields time series that are almost identical to the original ones ($r = 0.998$ for BNI, $r = 0.997$ for LUCI), and leaves their correlations with the reconstructed FLC anomalies essentially unchanged (BNI–AN: $r = -0.68$ in both cases; LUCI–CN: $r = -0.47$ vs. $r = -0.46$). The choice of reference period therefore does not affect our conclusions.

Ln149: In relation to the comment on Ln101, why is the 2004-2019 period used as the reference for calculating the anomalies? It would be preferable to use a consistent reference period throughout the analysis to ensure comparability.

The two reference periods serve different purposes. The 2004–2019 period is set by the availability of the satellite FLC record: as the statistical model is trained on this period, both the predictand and the predictors are deseasonalized over the same window to ensure they are defined consistently. The 1991–2020 period applies only to the SST-based indices (ONI, BNI, LUCI), which are not model predictors but are used to interpret the reconstructed variability; here we follow the definitions used in the literature (McPhaden et al. (2024) for the BNI, and the NOAA base period for the ONI) so that our indices remain comparable to published values. A single period could not be used for both, as no FLC observations exist before 2004, while defining the SST indices over 2004–2019 would depart from their standard definitions.
-Added in 2.3: "This reference period is set by the availability of the satellite FLC record, and applying it to both predictand and predictors ensures they are defined consistently over the training period."

Ln232-233: Li et al. (2025) is cited multiple times. Could the authors clarify the reason for the difference in the reported decline in fog amount after 1996? In particular, is this related to differences between low clouds and fog, or could it be attributed to methodological choices or other factors?

The two studies measure different quantities over different domains, which we think largely accounts for the apparent discrepancy. Li et al. (2025) report the annual amount of fog water collected at a single station (Gobabeb, some 60 km inland), whereas our reconstruction describes FLC cover anomalies, averaged over the coastal strip within 24 km of the coastline. A decline in collected fog water can result from fewer, shorter, or less productive fog events without a corresponding change in the frequency with which low cloud is present, and, as noted in Sect. 2, our satellite-based product cannot distinguish fog from low stratus that does not reach the surface. Gobabeb is furthermore located well beyond our study domain, at the inland margin of the fog gradient, where occurrence is lower and the marine layer only intermittently reaches the surface, making it particularly sensitive to changes in inland penetration that a coastal-strip average would not resolve. Finally, Li et al. (2025) attribute much of their post-1996 decline to air temperature and to local hydrological and vegetation processes (groundwater availability, plant transpiration), which are not represented in our cloud-controlling-factor framework. We have added a sentence to the manuscript noting these differences. It should also be noted that the record used by Li et al. (2025) is not homogeneous: it combines measurements from three different fog collectors, with the pre-1996 data subject to two successive conversions to make them comparable to the later instruments. The 1996 breakpoint coincides with a transition between measurement devices, which suggests the timing of the breakpoint would warrant further analysis before

being interpreted as a purely climatic signal.
-Added in 3.2: "However, this contrasts with Li et al. (2025), where in-situ fog water measurements show a decline in fog amount after 1996. These measurements are from Gobabeb, which, although within the latitudinal band of the CN region, lies some 60 km inland and therefore outside the study domain, which is restricted to the coastal strip within 24 km of the coastline. No corresponding break point is found in the reconstructed FLC cover time series, though the reconstruction carries substantial uncertainty. A change observed at Gobabeb, an inland site outside the study domain, would not necessarily be expected to emerge in this regional-average coastal reconstruction. Their record is also not a single continuous measurement, but a series assembled from successive fog collectors operating on different measurement principles. The 1996 step coincides with one of these instrument transitions, which calls for care in interpreting the break point."

Ln236 - Fig 4.: I would question whether Fig. 4 is needed, as it is only mentioned twice in the text. If retained, it should be used more effectively to summarize the contribution of each cloud-controlling factor and their general behavior.

Figure 4 is central to the study, as the decomposition of the reconstructed trends into individual predictor contributions is what allows the underlying processes to be identified; in particular, it shows that the near-zero total trends result from opposing contributions of increased stability and SST warming, which is one of the main conclusions of this work. We agree, however, that this was not adequately reflected in the text: while the entire discussion of the individual predictor trend contributions (EIS, R700, SST, U10, V10) is based on this figure, it was explicitly referenced only in a few places. We have added references to Fig. 4 throughout the corresponding paragraphs, and a sentence when the figure is introduced stating what the decomposition is intended to show.

Fig.6c. Ln.287. Please provide a clearer description of how the Pearson correlation coefficients are calculated as a function of monthly time lag, including how the lag is applied. If it is necessary, this information should be included in the Methods section.

The lagged correlations are computed by shifting the index series relative to the FLC anomaly series in one-month increments: for a lag of k months, the FLC anomalies are correlated with the index values from k months earlier, so that positive lags indicate the index leading the FLC anomalies. The correlation is evaluated over the overlapping portion of the two series at each lag. We have added a description of this procedure to the Methods section.
-Added in 2.2: "The relationship between the SST-based indices and the reconstructed FLC anomalies at different time lags is quantified using lagged Pearson correlations. Prior to this analysis, all series are smoothed with a 3-month centred running mean, consistent with the definition of the ONI. For a lag of k months, the FLC anomaly series is correlated with the index series shifted k months earlier, such that positive lags correspond to the index leading the FLC cover anomalies."

Fig 6 and 7. The correlation between FCL and ONI es low and scattered and the correlation between FLC and BNI is stronger. Even though anomalies time series are a nice way to represent these findings, adding a inset or small scatter plot between those variables highlighting cool and warm phases might improve the visualization.

We have added scatter plots of the monthly FLC anomalies against the respective index alongside each time series (panels b and d in Figs. 6 and 7), with points coloured by warm, cool, and neutral phase. These make the contrast between the two relationships directly visible: the ONI scatter is diffuse, with warm and cool phases overlapping substantially in FLC anomaly and only a weak slope, whereas the BNI and LUCI scatters show a clearer relationship with less overlap between phases. The corresponding figures for the MERRA-2 configuration in the Appendix have been updated in the same way.

Ln12, 49-50, 292-294 and 295-296: Please clarify which ENSO phase is associated with the enhancement of FLC. ENSO is typically characterized by warm (El Nino) and cold (La Nina) phases, and it would be helpful to specify the phase-dependent behavior in this context.

The relationship is positive: El Niño conditions are associated with enhanced FLC cover and La Niña conditions with reduced FLC cover, as is now visible in the scatter plots added to Figs. 6 and 7. We have revised the text at the locations noted to refer explicitly to El Niño conditions rather than to ENSO events or years, and have stated the phase-dependent behaviour explicitly in Sect. 3.3 and in the Conclusions.

3 Specific comments

Ln12-14: The statement on interannual FLC variability would benefit from clarification of whether the SST-FLC relationship, i.e., whether SST anomalies are positively or negatively associated with FLC occurrence.

The relationship is negative: warmer SST anomalies are associated with reduced FLC cover. We have made this explicit in the sentence in question.
-Modified in the abstract: "Benguela Niño events, associated with tropical water intrusion and SST warming, explain up to half of the interannual FLC variability in the Angolan Namib, with warmer SSTs reducing FLC cover."

Ln18: The word "harsh" may be perceived as subjective. A more specific or objective term would improve clarity.

We have replaced the subjective term with a more specific one.
-Modified in the introduction: "Despite these hyper-arid conditions, it is home to a large number of plant and animal species, several of which are endemic."

Ln29: The term "these clouds" is ambiguous. Please clarify whether it refers to advection fog, advected marine stratus clouds, fog, or high fog.

The sentence refers to the advected marine stratus clouds described in the preceding sentence. We have made this explicit.
-Modified in the introduction: "High fog is most frequent between September and March and extends up to 100 km inland."

Ln38-48: The paragraph initially introduces the fog hotspots, but the description subsequently shifts to upwelling and the intrusion of warm tropical waters. Explaining the relationship between these processes and fog occurrence would improve the coherence of the paragraph.

We agree that the connection between the upwelling regime and fog occurrence was left implicit. We have added a sentence introducing this link before the description of the two upwelling cells.

-Added in the introduction: ”Upwelling is a key component of fog formation in the Namib Desert, as marine air advected over cold upwelled water cools towards saturation (Olivier and Stockton, 1989).”

Ln53: The term “non-local effects” is somewhat vague. Please clarify which processes are included under this definition and provide representative examples.

By ”non-local effects” we mean the influence on FLC cover of conditions occurring outside the AN and CN regions themselves, which the use of spatial predictor fields allows us to capture. Examples include SST anomalies associated with upwelling cells located upwind of these regions, such as the Lüderitz cell relative to the CN, and large-scale circulation patterns extending across the domain. We have clarified this in the text.
-Modified in the introduction: ”We extend the traditional CCF framework to account for advection patterns and non-local effects, i.e. the influence of conditions occurring outside the AN and CN regions themselves, such as SST anomalies associated with upwelling cells located upwind of them, by using spatial predictor fields (Andersen et al., 2020; Ceppi and Nowack, 2021; Ceppi et al., 2026).”

Ln56: This further highlights the need for consistent terminology as mentioned in the comment on Ln38-48. Please ensure that “marine low clouds” is either clearly defined or aligned with the previously used terminology, and adjust accordingly throughout the manuscript.

We use ”marine low clouds” when referring to the broader class of low clouds over the ocean addressed by the cloud-controlling factor literature, and FLC for the fog and low clouds analysed in this study, as defined in Sect. 1. The distinction is intentional here: the hypothesis being tested is precisely whether the sensitivities we derive for FLC match those established for marine low clouds, which would support a marine, advective origin for the FLC observed in the Namib. We have checked the manuscript and confirm that ”marine low clouds” is used only in reference to the broader literature, and FLC throughout when referring to our own analysis.

Ln69-71: The reference to Malik et al. (2026) appears somewhat out of context in this paragraph. It introduces an alternative approach for estimating cloud base height, but this method is not used in the present study and is immediately followed by its exclusion. Clarifying the purpose of this citation or better integrating it into the argument would improve the flow of the paragraph.

The citation was intended to acknowledge that separating fog from low stratus is possible in principle, so that grouping them is a deliberate choice rather than an unavoidable limitation of the retrieval. We agree the sentence 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. In relation to the general comment on Ln75-77, please add boxes indicating the spatial domains used for the domain-averaged calculations.

The red outlines in Fig. 1 now show the domains used for the regional averaging, i.e. the grid cells within 24 km of the coastline between 15–17 °S (AN) and 22–24 °S (CN).

Ln189-191: The reference to a weakened South Atlantic High is not fully supported by the analyses presented, as pressure systems and subsidence are not explicitly examined. If this interpretation is based on previous literature, please include relevant references to support this statement.

We agree that the reference to a weakened South Atlantic High is not supported by the analyses presented, as pressure systems are not examined in this study, and we have removed it. We have revised the paragraph to describe the V10 structure as it appears in the coefficient fields, noting that it consists of negative values over the region and a positive band upwind to the south, and that it is consistent across all four configurations. We suggest a possible link between the upwind positive band and upwelling, and relate the negative values over the region to Andersen et al. (2020), who found less pronounced coast-parallel winds on fog days in the CN, associated with synoptic-scale disturbances producing a northerly wind anomaly ahead of the trough that favours stratus formation and onshore advection.
-Modified in 3.1: "Finally, for V10 (Fig. 2e), the sensitivities are weaker than those for U10, with negative values over the region itself and a broader band of positive values farther south. This structure appears in all four configurations and shifts northward in the AN region (See Appendix A), like the other sensitivity patterns. The positive area lies upwind of the region, where stronger southerly winds favour coastal upwelling and cooler SSTs, which in turn favour FLCs. The negative values over the region itself are consistent with Andersen et al. (2020), who found less pronounced coast-parallel winds on fog days in the CN, associated with synoptic-scale disturbances producing a northerly wind anomaly ahead of the trough that favours stratus formation and onshore advection."

Ln197: Please clarify which features are referred to as "the features discussed above", as this is not clearly specified.

The phrase referred to the sensitivity patterns described in the preceding paragraphs for each predictor. We have named them explicitly.
-Modified in 3.1: "Compared with CN, the AN sensitivity patterns are systematically shifted northward, toward the AN region: this applies to the EIS, R700, SST and wind sensitivities described above."

Fig. 3. It would be useful to include observed fog occurrence anomalies in order to better evaluate which configuration performs best.

As noted in the manuscript, the final reconstruction (Fig. 3) uses fold-averaged sensitivities, in which each month from 2004–2019 contributes to the training data in all but one of the six cross-validation folds, making the resulting reconstruction non-independent. A fully out-of-sample reconstruction can instead be obtained by combining the predictions from all test folds, yielding correlations with the observations of $r = 0.64$ for AN-ERA5, $r = 0.62$ for AN-MERRA-2, $r = 0.61$ for CN-ERA5, and $r = 0.63$ for CN-MERRA-2. Using fold-averaged sensitivities ensures a continuous time series from 1982 to 2019, which allows the analysis of interannual variability and long-term trends, but as these fold-averaged values are not

fully independent, we consider it potentially misleading to overlay the observations directly on Fig. 3.
To address the reviewer's request while keeping this distinction clear, we have added Fig. B1 (Appendix), showing the observations against both the fully out-of-sample prediction ($\hat{y}$) and the fold-averaged reconstruction, at annual resolution, for both regions and reanalyses. As expected, the fold-averaged reconstruction correlates much more closely with the observations ($r = 0.88$–0.94) than the genuinely out-of-sample prediction does ($r = 0.61$–0.64).
-Added Figure B1

Line 256-257: Please clarify the sign of the U10 and V10 trend contributions (i.e., which is positive and which is negative), as this is currently not specified despite stating that they compensate each other.

This paragraph has been revised and now states the signs explicitly: U10 contributions are negative or near-zero while V10 contributions are positive in three of the four configurations (AN-MERRA-2, CN-MERRA-2 and AN-ERA5), whereas CN-ERA5 shows the opposite pattern, with a positive U10 and a negative V10 contribution. The individual values are given for each configuration.

Ln323-325: This final statement reads more like a conclusion than a discussion point. Please consider moving it to the Conclusions section or remove/rephrase it.

We agree and have removed this closing statement, as the point is already made in the Conclusions.
-Removed: "Overall, these two indices highlight the importance of regional SST pattern variability on FLCs and, more specifically, emphasize the roles of both tropical warm water intrusions and local upwelling cells in partially explaining the interannual variability of FLCs in the region."

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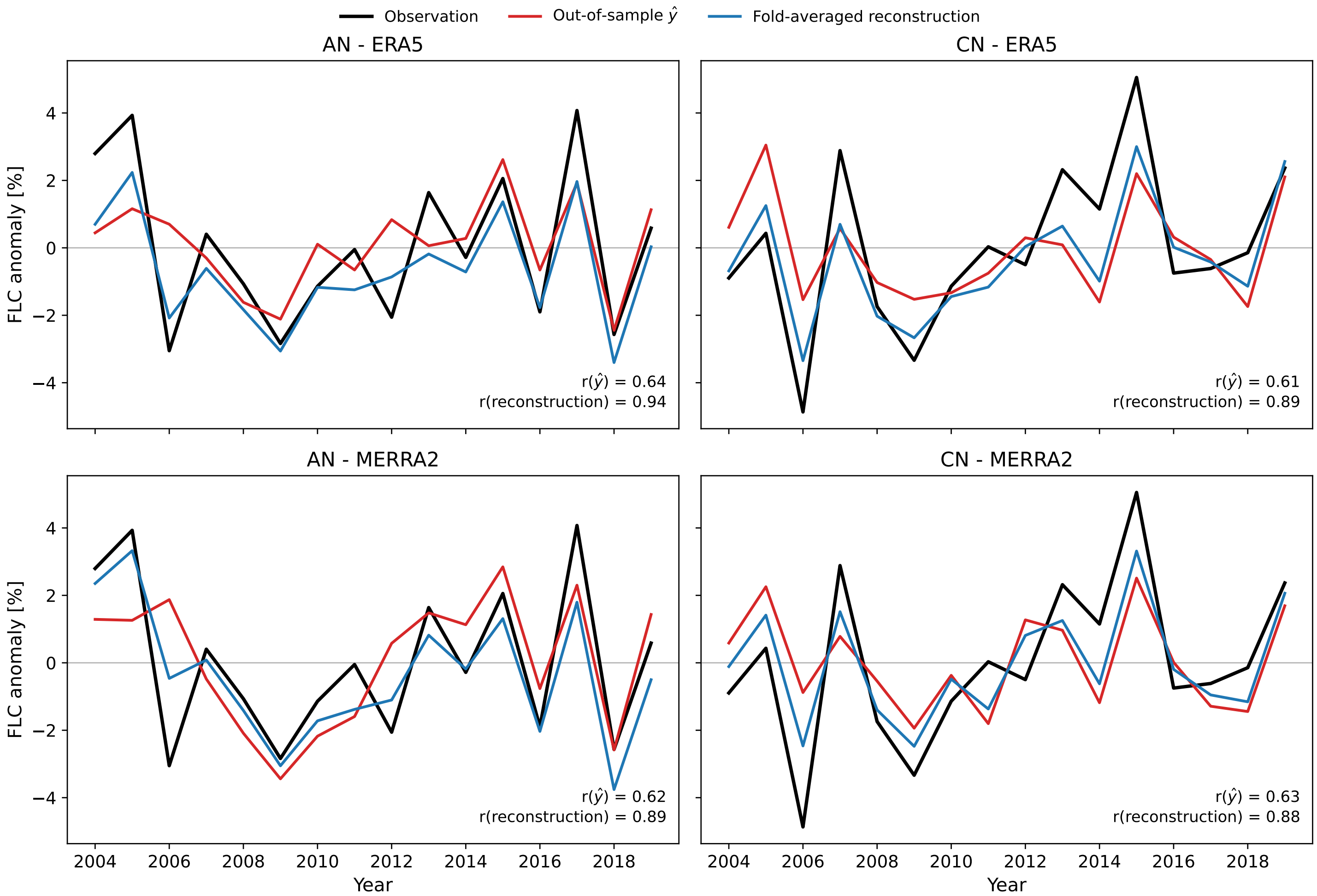

Figure 2: Comparison of observed annual FLC anomalies against the fully out-of-sample prediction ($\hat{y}$, red) and the fold-averaged reconstruction used in Fig. 3 (blue), for each region and reanalysis. The fold-averaged reconstruction is not independent of the observations and correlates more closely with them than the genuinely out-of-sample prediction, illustrating why Fig. 3 does not overlay observations directly.

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