Review of “Synoptic-scale controls of fog and low clouds in the Namib Desert” by Hendrik Andersen, et al., submitted to ACPD

In this study of fog and low-cloud (FLC) frequency in the central Namib coastal desert, the authors first present a novel 14 year satellite climatology (originally published in Andersen et al., 2019) of a relatively small region (~20,000 km^2). Then they select the most and least foggy days (amounting to about half of the total observations) in the two transition seasons (Fall and Spring), neither of which is the FLC frequency maximum, and then present the synoptic conditions based on reanalysis data under which foggy vs. clear days present.

The writing is clear and the figures are exceptional, however I find the inferences of causation to be quite speculative and not very convincing. I appreciate the observational nature of the analysis, and would not suggest that modeling needs to accompany it. However, the assertions, such as radiative cooling in the more arid lower troposphere somehow being the driving factor in determining fog presence, needs to have some quantitative basis – or at the very least make reference to some other studies that have shown this effect to be important. I would be surprised if a change of a few kg m^{-2} of water vapor was able to lead to increased radiative cooling rates of greater than ~0.5 K/day at the very most. Is this sufficient to dominate the influences that create foggy conditions? I am not sure, but without any reference to other work that may have found this to be true, it holds the scientific merit of nothing more than pure speculation. Therefore, I have a hard time seeing that this work can in the words of the authors bring about “a new conceptual model of the synoptic-scale mechanisms that control fog.”

One of the stark shortcomings of this work is the absence of a lot of FLC work that has been done in other eastern basin upwelling systems, which could shed a lot of light on the interpretation and analysis of this work. For example, the relationship between fog (or marine stratocumulus) and subsidence is completely overlooked, despite there being ample correlations pointed out in the literature (see, for example, Bony & Dufresne, 2005). Meanwhile lower tropospheric stability (LTS) is presented in Figures 3 & 5, but not really discussed at all. Other conspicuously missing prerequisite work includes Clemesha et al. (2017), Iacobellis & Cayan (2013), Koračin et al. (2005), and Dorman et al. (2019) to name a few. Furthermore, not nearly enough emphasis is paid to the effects of upwelling on the SST’s and the SST anomalies on the fog. This is especially surprising given that a large portion of what controls upwelling is coastal geography which influences the wind curl along the coast (see Koračin et al., 2004).

I do not wish to sound too damning in my criticism of the work being pure speculation, but let me propose an entirely different interpretation of the data in this paper that would construct a competing narrative, or conceptual model, of the synoptic controls on coastal fog. To wit, enhanced negative vorticity advection upwind of the target site on foggy days induces subsidence which increases LTS, drying the lower troposphere, reducing marine boundary layer (MBL) entrainment, increasing surface winds and thus latent heat fluxes from the ocean, and allowing for greater moisture build-up in the MBL prior to encountering the lowest SST’s of the upwelling system along the coast.

In light of the speculative nature of the manuscript as it stands, and that the value of the climatology has already been made available to the community (in Andersen et al., 2019), I would recommend not publishing this without major revisions in order to substantiate the conceptual model of fog production presented herein.

Specific Comments are presented below in order of appearance:

p.1, l.6: It is not clear why these two seasons are chosen. AMJ is not a common seasonal breakdown either – it is late fall into winter. What is meant by "characterize seasonal fog" exactly?

Figure 1: First a clarification - 1c) shows the average FLC occurrence over all days (from 3-9 UTC), and the peak is during the SH summertime, is that correct?

Also, I wonder about the wisdom of fixing this time window rigidly past the falling edge of the fog 'burn off'. Sunrise times in that area shift from ~4:00 UTC in summer to 5:45 UTC in winter, which is an appreciable portion of this 6 hr window. I worry that this could bias the FLC frequency changes observed by season.

I think it might be useful to compare your results to any other cloud climatologies that exist for the region. For example, Dorman et al., 2019 present a COADS-based fog climatology that suggests a fog peak in MAM months in the Benguela upwelling system.

I think the monthly FLC pattern is central enough to this work to warrant a line graph as opposed to this subtle gray scale figure which allows for a much less quantitative comparison of the seasons.

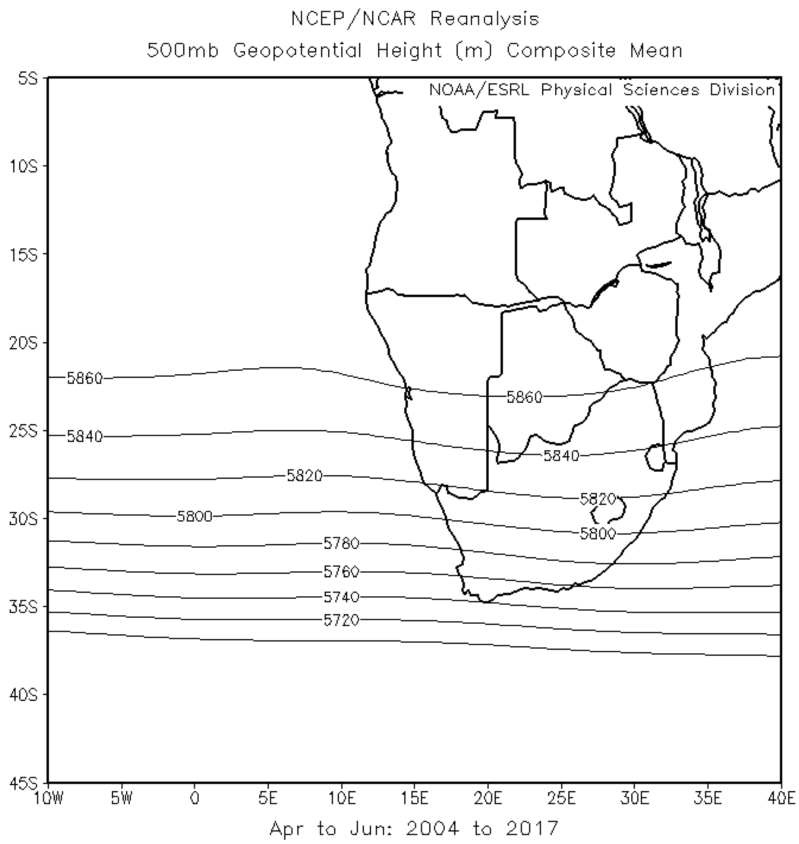
Finally, it seems to me if you are going to carry out an annual analysis of FLC-Clear (as you do in Fig 3), you need to report what fraction of your clear and FLC days from your histogram come from each season. Because the pattern you see in Fig. 3 could match the patterns you see in Figs. 4/5 for SON simply because that is where the majority of your FLC days throughout the year come from.

p.7, l.15: This is confusing because you are focusing on SON, and only the thin latitudinal band from ~22-24°S, the FLC peak actually occurs in DJF (as shown in Fig. 1c & Andersen 2019, fig. 2c.)

p.8, l.3: The winds are southerly throughout the region, how do you infer "northerly" advection?

p.8, l.7: You are referring to features of the climatological Z500 pattern without showing what that is, so it is hard to assess these statements about a trough and the absence of a coastal low.

Are you sure Olivier and Stockton (1989) are not referring to a particular time of year for their coastal trough as opposed to a year round analysis that you are presenting here?
A quick look at NCEP reanalysis data for the region shows a subtle trough upwind of the coastline.



p.9, l.4: I think this SST time lag inference is unfounded speculation on the authors' part. The wind difference indicates to me that the clear days have slightly stronger offshore wind components, which could weaken ocean upwelling. It is the alongshore wind component that determines the upwelling, and could possibly have subtle variations due to coastline geography (see, for example, Koračin, Darko, Clive E. Dorman, and Edward P. Dever. "Coastal perturbations of marine-layer winds, wind stress, and wind stress curl along California and Baja California in June 1999." Journal of Physical Oceanography 34, no. 5 (2004): 1152-1173.)

p.9, l.5: The SST anomaly having a hydrostatic impact on MSLP seems highly unlikely given that the FLC effects are associated with strong synoptic forcing as argued in the last paragraph. Furthermore, how exactly does it appear likely that SST-FLC correlations are most 'pronounced on seasonal scales'? Can't that be determined for your data set and put to the test? There is not all that much variability in SST in this region, as far as I can see from NCEP reanalysis data.

p.9, l.10: This speculation would benefit from some sort of simple calculation of the magnitude of this effect. Are you meaning to say that radiative cooling will be significantly influencing the SST's? If so, this seems unlikely in a strong upwelling system such as this. Or are you saying that

FLC, once formed, will be sustained by effective cloud-top radiative cooling due to the dry tongue over it? As it stands this just seems like a qualitative speculation that is unsubstantiated (without at least a reference to another work that has explored a comparable situation, or a back of the 'envelope' calculation on your part.) The same holds for the assertion that moisture advection influences the surface heat low by principally radiative means presented in the following sentences.

p.9, l.14/15: This hypothesis could be tested by looking at the T anomaly only during the overnight hours to see if it is an air mass difference or an insolation difference (I strongly suspect it is the latter.) My hunch is that it will be slightly warmer overnight because of radiative heating of the surface from the FLC, which would provide evidence against the air mass difference hypothesis.

p.9, l.27: In the discussion surrounding the similar annual pattern of Fig. 3b you referred to it as a trough instead of a cut-off low.

p.11, l.4: A few 0.1's K is a subtle change, but the increased wind speeds could definitely increase the latent heat fluxes in the upwind region. Here, you could get a sense of the relative magnitude of these effects by using a simple moisture exchange coefficient and quantifying differences in saturation vapor pressures vs. mean wind speeds.

p.11, l.6: I would bet that it has everything to do with upwelling induced by the wind field.

p.11, l.30: Or the dry anomalies could be associated with subsidence which augments the LTS in the fog cases. This reduces MBL entrainment and along with increased LH fluxes upwind helps to build up Q in the MBL. Along with a lower SST, these influences act in tandem to reduce the dew point depression.

Figure 5: Very little attention is paid to the LTS anomalies presented. My read of Figs. 4/5 is that regardless of season FLC is strongly associated with low SSTs, low T2m, and high LTS.

p.14, l.17: You could look at potential temperature to see what sort of effects that radiative cooling has on the foggy days. It seems that potential temperature would be a better variable to present in the back trajectories (unless, of course, it is a purely isentropic back trajectory.)

p.14, l.21 to p.15, l.1: Doesn't this contradict your hypothesis presented earlier about the lower column water vapor leading to greater radiative cooling on the foggy days?

p.18, l.19: It is not too surprising that so much is explained by the MSLP fields because they determine a lot of things. For instance, MSLP is the main variable used in calculating conventional upwelling indices. Again, I found the lack of centrality of coastal SSTs to be surprising in this work given how important it is found to be in most other studies.