Reviewer 2
Response to authors' rebuttal of review of "Synoptic-scale controls of fog and low cloud variability in the Namib Desert"

Overall I found the authors response to my initial comments quite thoughtful and mostly satisfactory. I now recommend that the revised manuscript be published in Atmospheric Chemistry & Physics without the need for further review by me. Nevertheless, in the spirit of continued dialogue on this fascinating topic I present below my final responses to their responses in the event that they might find them useful in making any final improvements to this or future manuscripts, to be done at their own discretion.

About the change of title to focus on 'variability' of fog/low-cloud (FLC) instead of just FLC: I do not see a substantive difference in describing what controls the presence of fog/low-cloud and its *variability*. In order to understand the variability you need to understand the causes, and presumably knowing the dominant factors of causation will tell you something about the variability. I do not understand the distinction being made here. The discussion spans several temporal scales and those distinctions seem more critical than the one between FLC and its *variability*.

About the shift in sunrise times across the year: It is too bad not to treat Figure 1c as quantitative because it would be nice to have a quantitative assessment of FLC cover across the annual cycle in the region for reference. I suppose you are suggesting that this is presented in Fig. 3a of Andersen et al. (2019). It is your manuscript, but I don't see why you don't use a window that follows the sun, or is pushed back off the falling edge of the stratocumulus deck in order to account for this periodicity methodically.

About the original comment surrounding p.7/l.15: OK. I see now that I believe some of the confusion arose from the use of 'high' and 'low' which could refer to frequency or elevation. Being more specific with the language in your revision helps a lot.

About Fig. R2.1: Yes, I see your point now. I was misinterpreting Figure 2, possibly because the warm colors of the MSLP gave me the impression of temperature. My apologies.
This supplemental figure shows N-NE advection into the inland region. You might consider contouring the T-field so that advection is more easily seen. Also, you do not present any wind speed legend for the vector lengths so it is difficult for the reader to estimate an advection rate.

About the discussion of SST-FLC correlations on seasonal scales and your inclusion of Klein & Hartmann (1993) Fig. 6a in your response: You cite their showing SST dominating annual LTS changes, but you are arguing that LTS does *not* influence your day-to-day variability of FLC in your domain. Furthermore, the seasonal frequency of stratus from Klein & Hartmann (1993), with its distinct peak in SON, does *not* match your data set (their Fig. 4b). Perhaps that is because theirs is for 10-20°S, 0-10°E, a region downwind of yours.

About the original comments surrounding p.9/l.10: I appreciate the extensive revisions you included in the discussion of TCWV, especially the references that attempt to quantify the longwave radiative impacts of the drier lower troposphere. However, I would suggest that your arguments with correlation should be taken with a grain of salt as they are by no means indicative of causation. This is particularly true in the case of water vapor and temperature, two meteorological variables that are highly correlated for strictly thermodynamic, and non-radiative, reasons. For example, I would not be surprised if a large portion of your Fig. R2.4 were not merely a manifestation of the Clausius-Clapeyron relationship.

About Figure R2.7: I appreciate the addition of the 3-panel (FLC, and clear, and difference) format of this new supplemental figure.

About upwelling winds and SST response: Just to put a finer point on this SST lag to upwelling winds, from experience I expect that the lag time is something of order 5-15 hours in the midlatitudes. This is more like the period of an inertial oscillation, which is half of a pendulum day. In other words, I do not think you need to wait around for an equilibrium solution time scale of a couple of pendulum days to get the bulk of the temperature response to a wind shift. It is observed to occur much more rapidly. [See for example, Lentz, S.J., J. Phys. Ocean., 1992]

About your subsidence reported in Figure 6: In regions of complex terrain and coastal geography I suspect that the low spatial resolution in the ERA5 reanalysis data probably does not capture subsidence very completely, but I realize it is the best you have available, and your other arguments are persuasive.

About your inclusion of potential temperature in Figure 8: I disagree that the primary difference is definitely the MBL Q (1.2-1.5 g/kg). The difference in potential temperature between clear and FLC days is ~2.5 K. At those temperatures the dQ/dT is about 0.7 g/kg/K, so those differences are comparable with respect to dew point depression.

In conclusion, I would like to thank the authors for their thorough and thoughtful responses to my review and for putting the significant effort into advancing the conversation and my understanding of fog and low cloud in these upwelling systems. Excellent job!