Synoptic-scale controls of fog and low clouds in the Namib Desert: Response to Reviewer 2

Hendrik Andersen, Jan Cermak, Julia Fuchs, Peter Knippertz, Marco Gaetani, Julian Quinting, Sebastian Sippel, and Roland Vogt

contact: hendrik.andersen@kit.edu

We would like to thank reviewer 2 for her/his careful review of the manuscript and her/his valuable comments, thoughts and the constructive criticism. Comments by the referee are colored in black, our replies or comments are colored in blue and written in italics. We give initial responses in the introductory text, but in the applicable cases, a more detailed discussion follows in the point-by-point responses to the specific comments.

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.”

Thank you for this statement, which made clear to us that the scope of both aims and findings was open to misinterpretation and misunderstanding.

On radiative cooling being ‘the’ driving factor: By no means do we state that the radiative cooling is ***the*** *dominant factor driving FLC formation (or even it being the driving force for e.g. the seasonality), but rather that it is a contributing factor that facilitates FLC occurrence and thereby influences the day-to-day variability that is in the focus of this manuscript. Also, the free-tropospheric dry anomaly is larger than the TCWV composites suggest, as some of the TCWV difference is masked by the increased moisture in the marine-boundary layer on FLC days so that in relative terms, the difference in specific humidity between FLC days and clear days is quite substantial (up to 220 % at ~700 hPa; P11,L29). In the discussion on the specific comment, we list and discuss studies that have shown the cooling (and also warming) effects of TCWV to be important for low-cloud cover (and also land surface temperatures) in the region.*

On the conceptual model comment: Indeed we recognize not to have chosen optimal wording to communicate the conceptual model in the abstract: In the updated version of the manuscript, we use a more precise description (similar to that used e.g. in the caption for the conceptual model): “a new conceptual model of the synoptic-scale mechanisms that control fog and low cloud

***variability** [...]" . We also decided to change the title of the manuscript to "Synoptic-scale controls of fog and low cloud **variability** in the Namib Desert". Both changes are intended to clarify that in this study we do not analyze the drivers of the Namib-region FLC system, but the drivers of the day-to-day variability within the system.*

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).

Thank you for pointing us to these interesting and relevant publications, and, indeed the links to other coastal upwelling systems are now strengthened in the manuscript. We did point out on page 9, L 9-11 of the original version of the manuscript that "These stable conditions promote the formation of the southeastern Atlantic stratocumulus cloud deck and determine its seasonal cycle (Klein and Hartmann, 1993; Andersen et al., 2017)." We agree that we should discuss stability and SSTs more, as they are clearly main drivers of the FLC system. As the focus of the manuscript lies on synoptic-scale modifiers of day-to-day FLC variability within this system, and both SST and LTS difference patterns are not as marked as the other mechanisms described in this paper, we did not discuss them in similar detail. However, after carefully considering the points brought up in this review, we agree that this might actually be a shortcoming of the manuscript in its current form. Therefore we have decided to include and explain in much more detail the role of SST and LTS for FLCs in this region, and to discuss links to comparable systems, also based on the valuable sources pointed to by the reviewer. In the point-by-point responses below, we discuss this in more detail, show additional analyses and present the changes to the manuscript.

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.

We thank the reviewer for sharing her/his thoughts about this. You have triggered engaging and fruitful discussions among the authors. While we agree with some aspects of this alternate conceptual model, and will discuss those in more detail in the updated version of the manuscript (increased wind speeds lead to an increased upwind latent heat flux, building up moisture in the marine boundary layer, at least in AMJ; advection over the lowest SSTs), Fig. 6 in the manuscript gives no indication for a relevant difference in the subsidence between FLC and clear days. In fact, in this response letter we provide evidence that for most of the year, day-to-day differences in LTS over land are almost entirely driven by its surface component (T2m). The observed moisture differences clearly come from horizontal transport within the free troposphere (see also Fig. R2.7 in this document). Also, during SON, the coast-parallel winds that drive the upwelling are actually substantially weaker on FLC days than on clear days, with the SST not showing a clear pattern. To

summarize, multiple aspects of this proposed alternate conceptual model are not actually supported by our findings.
We believe, however, that here actually lies a misunderstanding: We do by no means say in this manuscript that the advection over the cool upwelling water is not a driving mechanism of Namib-region FLCs, it surely is (we also point this out on P12, L2-3)! We rather argue that the day-to-day variability that is investigated here is mainly driven by other factors – within a system in which SSTs play a key role. We now communicate this more clearly in the updated version of the manuscript as detailed below in the point-by-point responses, and have changed the title of the manuscript to: "Synoptic-scale controls of fog and low cloud variability in the Namib Desert" to more precisely describe to scope and aims of this study.

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.
We would like to state that in those passages of the manuscript where we do speculate, this is clearly shown by the language used (e.g., "potentially hinting", P9 L4). We argue, however, that in light of the clear results on many aspects of the paper (which are corroborated by the additional analyses carried out for this response), the main mechanisms described in the conceptual model are well justified, offer a coherent explanation for the observed patterns and thus bring completely new insights into mechanisms that modify day-to-day FLC variability in the central Namib. As the purpose and scope of this manuscript is to better understand synoptic-scale mechanisms that influence the day-to-day variability of FLCs, very little overlap exists to the Andersen et al. (2019) paper, which 'just' provides an observation-based FLC climatology.

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?
You are correct in pointing out that this seasonal breakdown is not common, and indeed, judging from the FLC seasonality presented in Fig. 1c), does not represent the two extreme seasons of FLC occurrence (this would be the more classical seasons DJF and MJJ/JJA). However, we have conducted this analysis in the context of understanding factors driving the day-to-day variability in fog occurrence, and in this context these seasons do make sense. During AMJ, FLCs are markedly lower in the atmosphere, leading to a maximum of fog occurrence at low-lying coastal stations ("low-FLC season" see Fig. 3 in Andersen et al. (2019)), whereas during SON, FLCs are located at higher altitudes ("high-FLC season") leading to a peak in fog occurrence at stations further inland (Andersen et al. (2019), Lancaster et al. (1984), Seely and Henschel (1998)).
We agree that this should be presented more clearly and have changed the corresponding sentence in the abstract to
"[…] during two seasons with different spatial fog occurrence patterns", and added the two following sentences to section 3.1:
"While the FLC occurrence in the central Namib peaks in austral summer, and is lowest during winter, due to the seasonal cycle in the vertical position of the cloud layer, fog peaks at coastal locations in AMJ and at inland locations during SON (Seely and Henschel, 1998; Andersen et al., 2019). For these reasons, this study focuses on mechanisms determining FLC variability within these two characteristic fog seasons."

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?
Yes, indeed this is correct, the caption now states that "Panel c) shows monthly averages of the spatiotemporally averaged FLC cover data set."

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.
This is an interesting point, and indeed the time window includes the time of dissipation. In Andersen and Cermak (2018), the average diurnal cycle of FLC occurrence is shown for selected locations in the central Namib. It is apparent that diurnal FLC occurrence peaks around the time of sunrise so that within the considered time frame FLC occurrence rises, peaks and starts to dissipate. The sunrise shift potentially introducing a bias to the seasonal data sets analyzed is a good point, even though we do not analyze summer and winter so that the difference will be much smaller during the analyzed seasons. However, we are not comparing data sets of the different seasons to each other, but the difference between robustly different FLC and clear days within each season. Therefore, a marginal change in the selection of the days analyzed in each group is not expected to markedly influence the results. The choice of the time period and potential implications are now discussed in detail in section 2.1 of the updated version of the manuscript:
"A specified averaging time period is needed to avoid statistically mixing two separate FLC events occurring on successive nights which would be the case in a daily average FLC occurrence data set. The specific time period is chosen to include all periods of the diurnal cycle, with FLC occurrence rising, peaking, and starting to dissipate (Andersen and Cermak, 2018) during this time."
Later in Sec. 2.1 we state that:
"As the time of sunrise varies by season, the constructed data set is likely to feature a seasonal bias in FLC occurrence. It should be noted that this has no effect on the separation of FLC days and clear days within seasons, the analysis of which is the main purpose of this data set. The resulting monthly average central-Namib FLC cover (Fig. 1 c)) should not be used in a quantitative sense, but rather illustrate the general seasonal cycle of FLCs in this region."

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.
Thank you for pointing to this interesting paper. Dorman et al. (2019) use long-term weather observations from ships to create a marine fog climatology. Marine fog patterns can be quite different from FLC patterns observed from space due to the seasonality in the vertical position of the low-cloud layer (see Andersen et al. (2019), Fig. 3 c)). The fog seasonality in Dorman et al. (2019) does not agree well with the seasonality of fog observed at coastal stations in the central Namib (Andersen et al. (2019), Fig. 3 a)), indicating that marine fog occurrence over the cool SSTs of the Benguela is not necessarily a good proxy for FLC occurrence in the central Namib. While in this paper, we look only at FLCs over land, Cermak (2012) find a maximum of marine FLCs between September and January, again highlighting the importance of the vertical position of the cloud layer when comparing satellite-observed FLCs to (marine) fog. In section 2.1 we now discuss this:
"It is interesting to note that the seasonal cycle of FLCs is not necessarily coupled to the seasonal cycle of fog occurrence due to the seasonal cycle in the vertical position of the low-cloud layer. For example, at coastal locations of the central Namib fog peaks between April and August (Andersen

et al., 2019), while marine fog over the adjacent Atlantic has been found to peak between March and May, with a minimum occurrence between June and August (Dorman et al., 2019).“

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.
Thank you for this comment. The visual representation of the data was done on purpose to indicate that this figure is intended to just give a description of the specific data set used in this study (which is why it is positioned in the data and methods section), and not to introduce a novel climatology. As stated above, it was not the basis for defining the seasons, and should not be interpreted in a quantitative manner, also due to the point raised by the reviewer on seasonality of the time of sunrise. We now make this clear by stating:
"As the time of sunrise varies by season, the constructed data set is likely to feature a seasonal bias in FLC occurrence. It should be noted that this has no effect on the separation of FLC days and clear days within seasons, the analysis of which is the main purpose of this data set. The resulting monthly average central-Namib FLC cover (Fig. 1 c)) should not be used in a quantitative sense, but rather illustrate the general seasonal cycle of FLCs in this region."

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.
Yes this could be an issue, which is why we addressed this in the original manuscript by not just calculating the annual average difference of all FLC and clear days, but by first computing monthly average differences and subsequently averaging these. This addresses the outlined potential issues that would arise with yearly averages due to the FLC seasonality and is described on P7 L27, and also in the caption of Fig. 3 ("Averaged monthly mean differences"). We now recomputed the seasonal composites the same way to also address within-season changes in FLC occurrence.

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.)
We agree that this could be communicated more clearly. As outlined in our first response, the seasons were chosen with fog patterns in mind. The fog seasonality is related to FLC characteristics (vertical position), and, in Andersen et al. (2019), this lead to the definition of the two seasons that we investigate with this paper (SON and AMJ). We now state in this paragraph that:
"While the FLC occurrence in the central Namib peaks in austral summer, and is lowest during winter, fog peaks at coastal locations in AMJ and at inland locations during SON due to the seasonal cycle in the vertical position of the cloud layer (Seely and Henschel, 1998; Andersen et al., 2019). For these reasons, this study focuses on mechanisms determining FLC variability within these two seasons. "
The study region in the central Namib (~22-24°S) is chosen because nearly all research on Namib-region fog is conducted in this region, as the historical and current station measurements stem from here (Nagel 1969, Nieman et al. 1978, Lancaster et al. 1984, Seely and Henschel 1998, Henschel and Seely 2008, Kaseke et al. 2017, 2018, Li et al. 2018, Spirig et al. 2019, Wang et al. 2019). We now mention some of these sources in section 2.1 for added clarity.

p.8, l.3: The winds are southerly throughout the region, how do you infer "northerly" advection?
While indeed, over the ocean the winds are southerly, over land this is not the case. On P8 L3, we refer to the T2m anomaly that is most pronounced in the Kalahari region. In this region, northerly

advection is quite common (see e.g. Fig. 2b)), and Fig. 3a) shows that the northerly wind component into this region is strengthened on FLC days. To more clearly illustrate this, Fig. R2.1 in this response shows seasonal average T2m and 10m winds for FLC and clear days. The figure shows considerable temperature differences over land (specifically in the Kalahari region) and also the associated northerly winds that transport heat into the region. While during AMJ, the northerly flow in the Kalahari is also apparent on clear days, it does not originate from warmer regions, i.e. no substantial heat transport is expected. This figure is now added to the appendix of the paper.

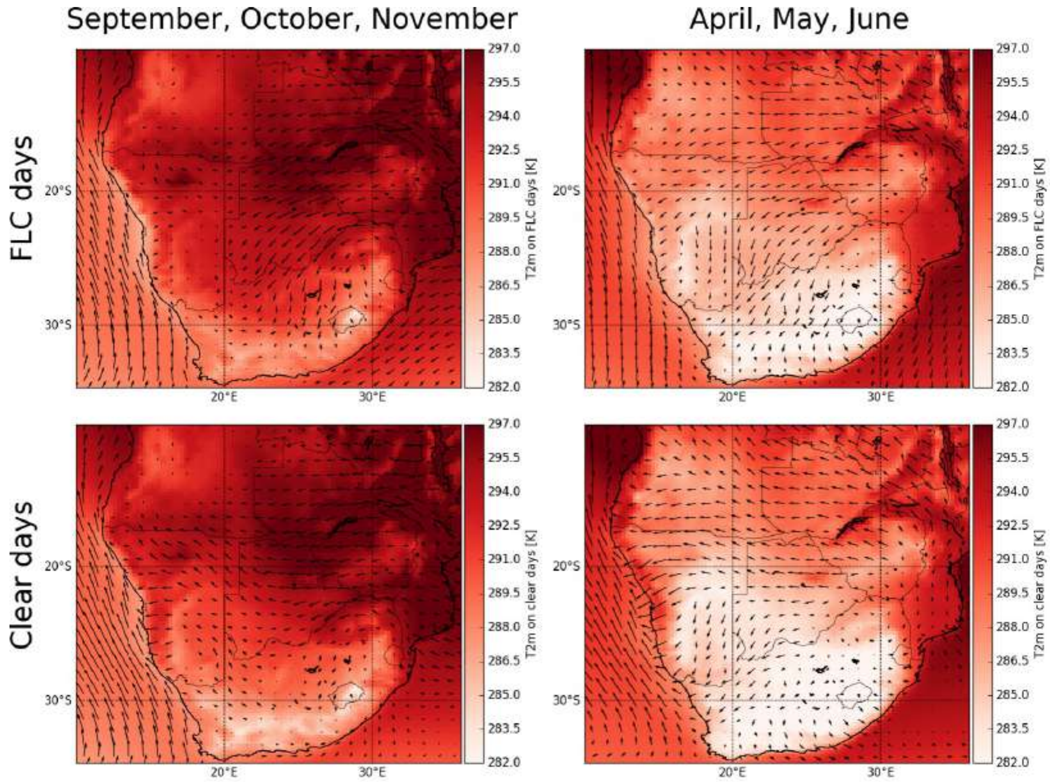
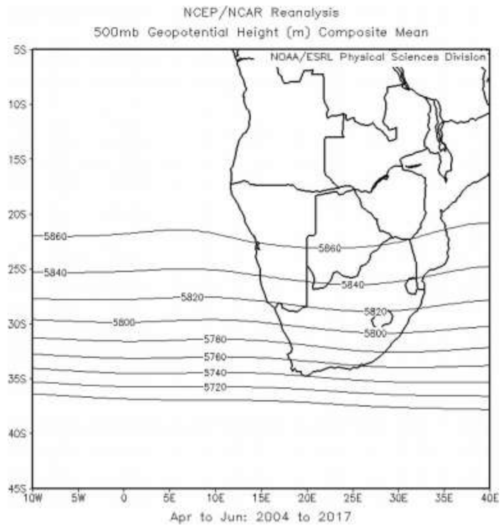


Fig. R2.1: Seasonal average ERA5 T2m and 10m winds in SON (left-hand panels) and AMJ (right-hand panels) for FLC (top) and clear (bottom) days. Winds are averaged to a 1°x1° resolution, the considered time period is 2004-2017.

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.



Thank you for this comment, the sentence on the coastal low was not communicated clearly enough. While one can see lower pressure along the coastline in the climatology of MSLP in Fig. 2 a) and b) of the manuscript, we do not find any indication for an increase in the occurrence of a coastal low on FLC days. There might still be coastal lows, of course, which is discussed on P8 L9-10. While Olivier and Stockton (1989) conduct a yearly analysis, the study does provide seasonal details that we agree not to have discussed enough. They find that the linkage between the coastal low and FLC occurrence in Lüderitz is most pronounced between November and March, and much less so from June to August, when cold fronts are more often associated with FLCs in Lüderitz. We would like to point out, though, that Olivier and Stockton (1989, p.73) state that "The coastal low which causes fog is a relatively small, local phenomenon". Therefore, while FLC occurrence in Lüderitz may be driven by the coastal low, this may not be directly related to FLCs in the central Namib as defined in this paper. This is also visualized clearly in Haensler et al. 2011 (Fig. 1a)). In fact, in Lüderitz, FLCs are much less common than in the central Namib (Fig. 2b) and c) in Andersen et al. (2019), latitude of Lüderitz: ~26.6°S), indicating that these are likely two separate regimes. Another interesting point that needs to be included in the manuscript is that more upwelling does not necessarily lead to increased FLC occurrence in the Namib region, as Olivier and Stockton (1989) point out that in the case of Lüderitz, an upwelling extent of greater than 200km actually leads to less FLCs in Lüderitz, as the local phenomenon of the coastal low is not able to transport moist air from beyond the upwelling front.
We now provide additional details in the introduction:
"In Olivier and Stockton (1989), a coastal low is described as the mechanism that, in case of a narrow coastal upwelling region, drives the onshore advection of foggy air masses into the region of Lüderitz in southern Namibia during austral summer, while during winter they find fog to be associated with cold fronts. However, they assume that, while undetected, coastal lows were also present in these cases, as they typically precede the passage of a cold front (Olivier and Stockton, 1989; Reason and Jury, 1990)."
Concerning the climatology of the region, described in Sec. 3.1 we now state:
"Coastal upwelling, which has been shown to determine marine sea fog patterns along the Namibian coastline (Dorman et al., 2019), in combination with the presence of a coastal low that drives the onshore advection of foggy air masses have been found to be major drivers of fog occurrence in southern Namibia during austral summer (Olivier and Stockton, 1989). One should note though that the relationship between SSTs and Namib-region fog is complex, as Olivier and Stockton (1989) point out that a too large upwelling extent can also lead to less fog in southern Namibia. Based on these insights, and also on knowledge from related coastal upwelling systems (Cereceda et al., 2008; Johnstone and Dawson, 2010; Del Río et al., 2018; Dorman et al., 2019), it

is clear that the Atlantic anticyclone, the SSTs, and the large-scale subsidence are main drivers of this coastal FLC system."
Concerning the seasonal differences described in section 3.2 we now state that
"While a coastal low, which has been described in Olivier and Stockton (1989) as a local feature that can determine onshore flow, may still be present on FLC days, there is no indication of an increase in its presence on FLC days on average. However, as Reason and Jury (1990) describe, the coastal low is frequently followed by a frontal passage, which is a synoptic-scale signal observed here (Fig. A1)."
The average Z500 patterns are now included as additional contours in Fig. 2 of the original manuscript.

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.)
We agree that this specific statement is somewhat speculative, which is expressed by our cautious phrasing in the manuscript ("potentially hinting"). Indeed, clear days feature offshore winds, which is especially pronounced during AMJ. During this time of the year, we agree that upwelling may be more intense on FLC days, as the alongshore wind component is stronger. However, during SON we find that the alongshore wind that drives the upwelling is substantially weaker on FLC days than on clear days (see Fig. R2.2 of this document, top row). This would mean that upwelling should be reduced on FLC days, but SSTs do not show significant differences. However, in the context of interpreting these wind-ocean interactions, it is important to note the role of time scale, as the Ekman transport is not only dependent on the instantaneous wind field, but produces a steady-state situation only after a few pendulum days (Pond and Pickard, 2013). As such, the upwelling reacts to time-integrated winds, introducing complex time lag effects, which are investigated in e.g. Goubanova et al. (2013). We now discuss this in the updated version of the manuscript by stating:
"[…] potentially hinting at a time-lag response of SSTs, which is to be expected, as Ekman transport produces a steady-state situation only after a few pendulum days (Pond and Pickard, 2013)."
We also discuss coastal modulation of winds on the basis of the suggested publication in section 3.1:
"On a local scale, the near-coastal winds that drive the upwelling are additionally modulated by the coastal topography (Koračin et al., 2004)."

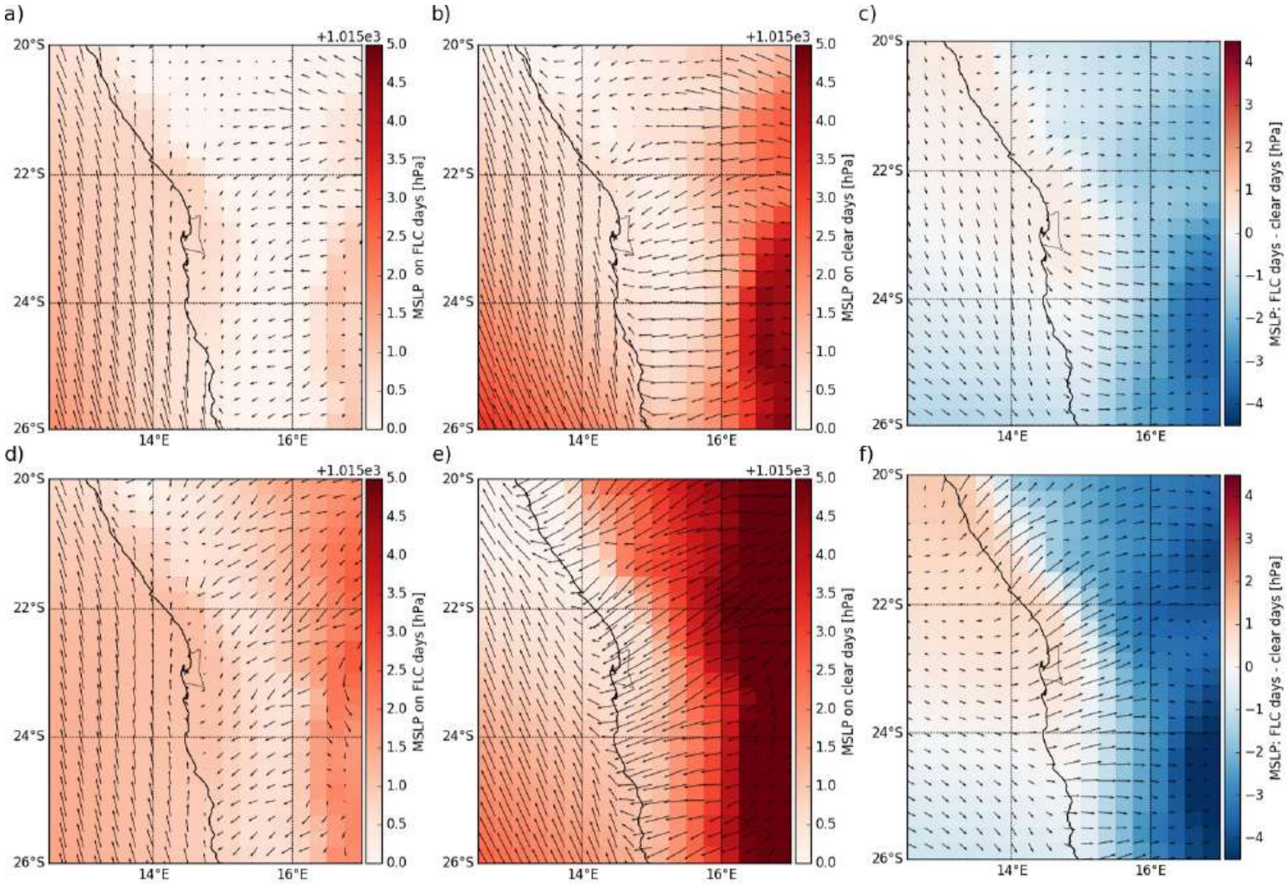


Fig. R2.2: Mean of monthly averaged MSLP and 10m winds during SON (top) and AMJ (bottom) on FLC days (left-hand panels), clear days (center), and their difference (right-hand panels).

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.
Thank you for this comment. We developed the hypothesis of the hydrostatic impact on MSLP on the basis of the yearly differences shown in Fig 3 a) of the manuscript. After consideration of your comment, we delete this hypothesis from the manuscript, as it is not corroborated by the seasonal patterns of Fig. 4 of the manuscript.
About the comment on SST seasonality and variability: Klein and Hartmann (1993) have shown that the seasonal cycle of SSTs dominates the seasonality in LTS (Fig. R2.3 of this response) in the Namibian stratocumulus field. Hutchings et al. (2009) state in their abstract that:"The southern Benguela region is characterised by a pulsed, seasonal, wind-driven upwelling at discrete centres [...]", but Tim et al. (2015) state on page 484 that "[…] a clear picture of the upwelling seasonality is not established yet." Concerning other timescales of SST variability, Goubanova et al. (2013) note that subseasonal SSTs close to the coast feature two regimes of variability: an 11 day oscillation, and a 61 day oscillation. In the updated version of the manuscript, we now state that "It appears likely that effects of SST patterns on FLC variability are most pronounced on time scales (i.e. seasonal to interannual) that feature higher SST variability (Hutchings et al., 2009; Goubanova et al., 2013; Tim et al., 2015), as also observed in the Chilean Atacama desert (Del Río et al., 2018)."

We agree that it would be useful to further explore SST influence on longer time scales, but 14 years might not be enough for meaningful statistical analyses of e.g. seasonal relationships. Also, with this paper, we are specifically looking at day-to-day variability within these seasons.

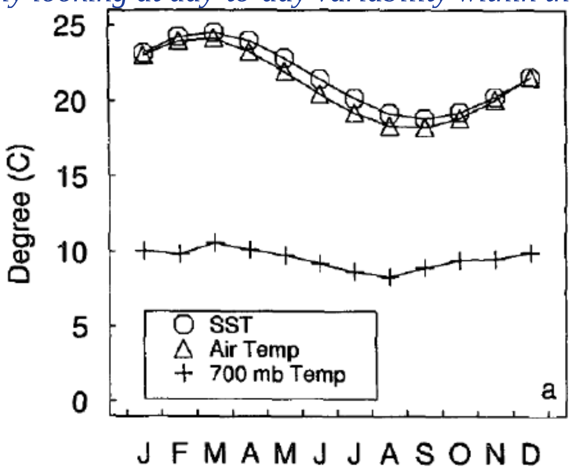


Fig. R2.3: The seasonal cycle of the SST and 700 mb Temp components of LTS in the Namibian stratocumulus clouds field (Figure taken from Klein and Hartmann (1993)).

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.
While the TCWV differences might not seem to be much in absolute terms (between ~2 and 5 kg m^{-2}), in relative terms, they are quite substantial. Also, in case of the free-tropospheric dry anomaly over the coast, one should note that part of the dry anomaly is actually masked by the moist anomaly within the marine-boundary layer (see Fig. 6 in the manuscript). As we point out on page 11, lines 26-29, the relative difference in Q in the dry anomaly is as high as 220%, and in the continental moist anomaly, Q is about twice as high on FLC days than on clear days. Over the land this will certainly lead to a substantial warming, as observed during AMJ, and has been observed in the Kalahari (Manatsa and Reason (2017)), and in other dry subtropical deserts before (e.g. the Sahara: (Evan et al. 2015, Alamirew et al. 2018), or the Sahel (Oueslati et al. 2017)). In Fig. R2.4 of this response, we show the statistical relationship between the TCWV anomalies and the T2m anomalies during AMJ over land for pixels which feature significantly higher T2m on FLC days than on clear days (cf. spatial patterns of Fig. 5 b) and d) of the original manuscript). A clear relationship is obvious, with a Pearson correlation coefficient of 0.75. This statistical relationship indicates that more than half of the observed T2m anomalies can be statistically explained by the TCWV anomalies, underscoring the relevance of the greenhouse effect for T2m.
In section 3.2 of the updated version of the manuscript we now state that
"These moist air masses may contribute to the observed T2m heat anomaly via greenhouse warming (Fig. 3 c)). This effect of free-tropospheric moisture on surface temperatures has been observed in the Kalahari (Manatsa and Reason, 2017) and other arid or semi-arid regions before (Evan et al., 2015; Manatsa and Reason, 2017; Oueslati et al., 2017; Alamirew et al., 2018)."
In section 3.3 of the updated version of the manuscript we now discuss this in more detail:
"Here the T2m anomalies closely follow those of the TCWV (Pearson correlation coefficient of 0.75 in continental regions with significantly higher T2m on FLC days than clear days), suggesting that

the increased moisture causes an additional surface heating due to greenhouse warming as discussed in Sec. 3.2. It is likely that the TCWV anomaly is caused by a large-scale free-tropospheric moisture transport from the tropics, which is supported by the marked wind anomalies at 500hPa (Fig. 4 d)) that show a northwesterly anomaly, and the absolute wind and moisture fields at 700 hPa during this time (Fig. A1)."

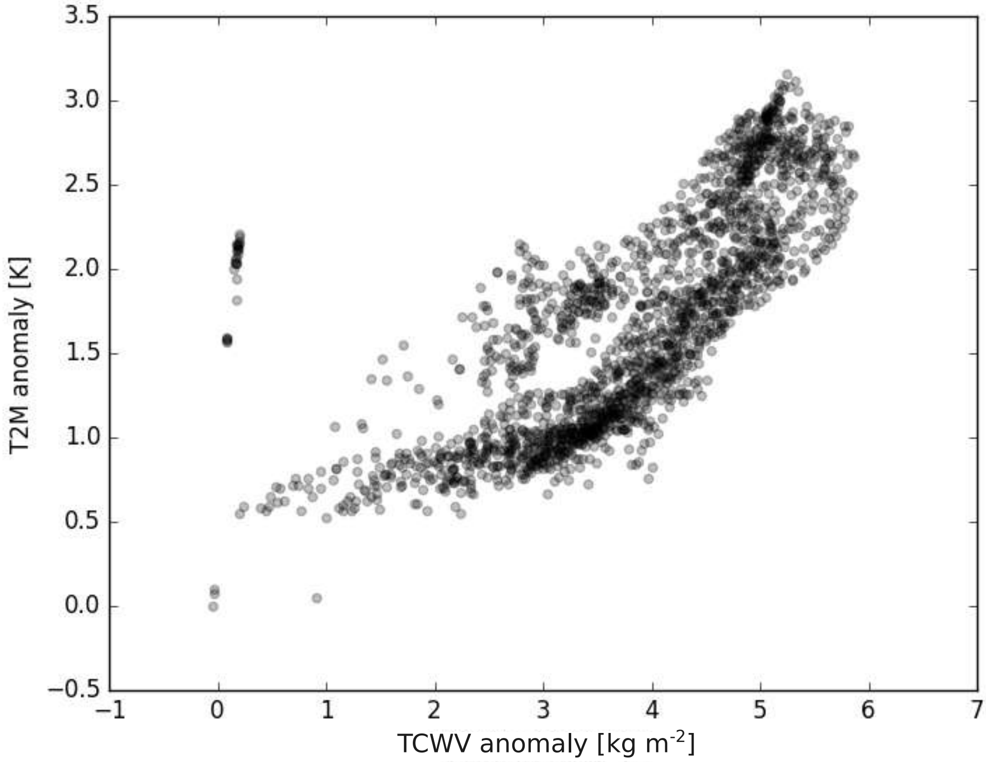


Fig. R2.4: The relationship of TCWV anomalies and T2m anomalies over land during AMJ, and in regions where T2m is significantly higher on FLC days than on clear days. The figure is added to the appendix of the manuscript.

While the SST will surely not respond as strongly as the LST, and in fact might be negligible, studies exist that point to the impact on stratocumulus clouds in the southeastern Atlantic. For this region, Adebiyi et al. (2015, p. 2015) have shown that with an increase in "midtropospheric moisture of about 1.2 g kg^{-1}, the downwelling longwave radiation averaged between 550 and 750 hPa increases by about 15 Wm^{-2}, reducing the net longwave cloud-top cooling by the same amount[...]". Recently, Adebiyi and Zuidema (2018) have shown that in the southeastern Atlantic, free-tropospheric moisture has a significant effect on stratocumulus cloud cover, where increases in free-tropospheric moisture are associated with a decrease in low-cloud cover. With specifically chosen variations of the predictors selected in their multivariate statistical model they could attribute this cloud response clearly to the greenhouse effect of water vapor. It is interesting to note that during the summer season (DJF), the time of maximum FLC occurrence, this dry anomaly is far larger, with anomalies up to 12 kg m^{-2} as shown in Fig. R2.5 of this document.

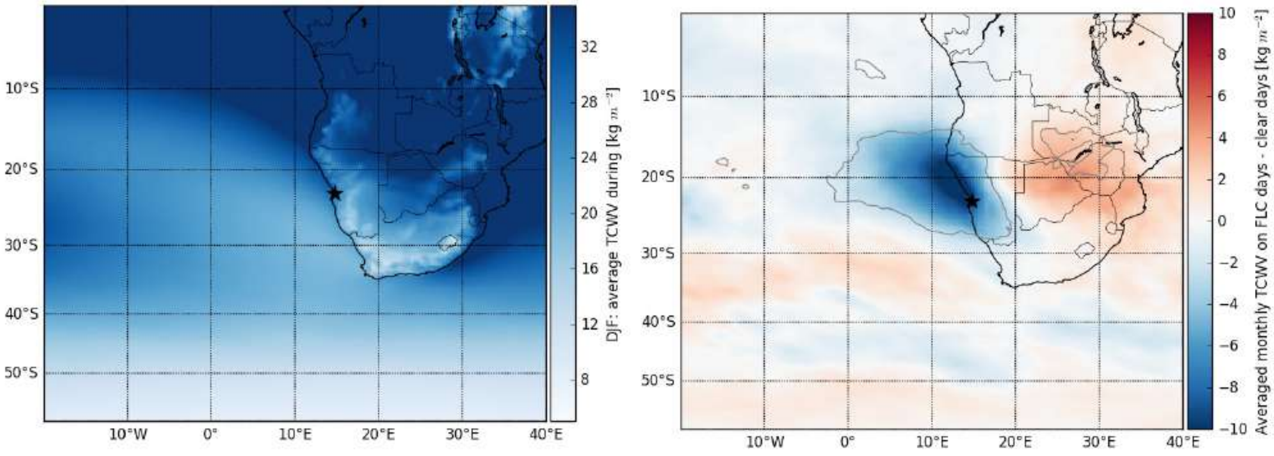


Fig. R2.5: left: DJF average TCWV; right: mean of monthly average TCWV differences (FLC days - clear days) during DJF.

Throughout the manuscript we have changed the wording from "facilitating FLC formation" to "increasing FLC cover".
In section 3.2 of the updated version of the manuscript, this is discussed in more detail:
"This is likely the dry slot (Browning, 1997) or dry air intrusion of the synoptic-scale disturbance that leads to increased longwave cooling at cloud top in case of FLC presence, which has been shown to be a main determinant of cooling within the marine boundary layer (Koračin et a. 2005). This enhanced cooling can increase FLC cover, which has been observed to be a significant mechanism for stratocumulus clouds over the southeastern Atlantic (Adebiyi and Zuidema, 2015; Adebiyi et al., 2018)."
In section 3.3 we state that
"It is interesting to note that the marine dry anomaly peaks between December and February (not shown), the season with maximum FLC cover in the central Namib, with TCWV anomalies exceeding 10 kg m^{-2}."

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.
We have conducted the suggested analysis and find that during nighttime (1 UTC and 3 UTC), the coastal regions are significantly cooler, pointing to a difference in air mass between FLC and clear days (cf. Fig R2.6 of this response letter for 1 UTC differences). We discuss now this in the manuscript:
"As this anomaly is also apparent during nighttime (1 and 3 UTC, not shown), it is likely that this pattern is mainly due to the relatively warm subsiding continental outflow that is apparent on clear days, rather than a radiative effect of FLCs as found in California (Iacobellis and Cayan, 2013)"

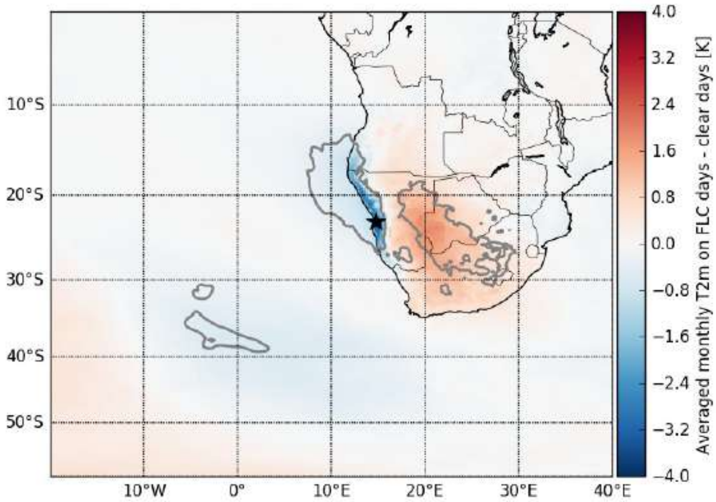


Fig R2.6: Averaged monthly mean differences (FLC days - clear days) of T2m at 1 UTC. In each pixel, an independent two-sided t-test is computed to identify significant differences between FLC and clear days for each month. Contours mark regions where the distributions differ significantly at the 0.01 level (median of the monthly p values <0.01).

We also discuss the correlation between T2m and coastal low cloudiness found in Clemesha et al (2017):
"It should be noted that in a comparable upwelling system (coastal California), Clemesha et al. (2017) also find a positive relationship between T2m over land and coastal low-level cloudiness, with the T2m anomaly shifted poleward by about 5° latitude from the cloud field. They propose that the T2m-cloud relationship is due to spatially-offset associations between coastal low-level cloudiness and stability (potential temperature at 700 hPa), which is strongly correlated to T2m over land thereby resulting in the T2m anomaly, rather than T2m driving the onshore advection. While in the central Namib, the anomaly patterns between potential temperature at 700 hPa and T2m are similar in that they are also positively correlated during SON (and therefore compensate each other in terms of LTS, Fig. 5 c) and e)), they are uncorrelated during AMJ (and also in the annual averages), when T2m over land is strongly correlated to TCWV. Also, during all times of year, the T2m and MSLP anomalies are directly inland from the cloud field, suggesting an influence on onshore advection."

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.
After reviewing the absolute fields of Z500 and Z700, we conclude that the synoptic-scale disturbances are not strictly speaking cut-off lows and Rossby wave breaking, and have deleted the corresponding wordings. The trough is visible in the absolute wind fields shown in Fig. R2.7 of this document. This figure is now included in the appendix of the updated version of the manuscript.

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.
Yes, this increase in upwind latent heat fluxes leading to increased marine-boundary layer moisture is precisely our hypothesis. However, we agree with Reviewer 2 that we did not state this clearly enough. We do not believe that quantifying the contribution of each specific mechanism is within the scope of this manuscript, and particularly in this case, as we would also need to take into account horizontal transport and vertical mixing. We state more clearly now:

"In isolated patches further south, upwind of the study area, SSTs tend to be significantly higher on FLC days. This could lead to increased surface latent heat fluxes, increasing the moisture content of the marine boundary layer, particularly during AMJ when stronger near-surface winds are also apparent. A few 100 km to the west and south of the Namibian coastline, SSTs could similarly add to the increased moisture within the marine boundary layer."

p.11, l.6: I would bet that it has everything to do with upwelling induced by the wind field.
This aspects seems to be dependent on the considered time scale. We now state that:
"It is not clear yet, however, what exactly drives the observed anomaly patterns of SSTs. As upwelling reacts to the time-integrated wind field forcing over longer time scales than analyzed here (Pond and Pickard, 2013), the SST response to the instantaneous winds that are considered here is expected to be relatively weak. However, in the case of a relatively stationary disturbance as discussed above, the upwelling patterns could indeed reflect an SST response to a synoptic forcing."

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.
In Fig. 6 we do not find substantial differences in subsidence, and during SON the opposite is actually the case (the increased stability over the ocean is driven by the decrease in T2m, likely due to relatively warm continental outflow on clear days). In Fig. R2.7 of this document, we show the average Q and winds at 700 hPa, which is the layer with strongest Q differences (see Fig. 6 of the manuscript). It is clearly apparent that the dry anomaly during both seasons and the moist anomaly over continental Africa during AMJ is driven by horizontal transport induced by the synoptic disturbance on FLC days. We agree that the increased LH fluxes upwind should build up Q in the MBL, which we state on P11 L4. In the original version of the manuscript, we did not clearly enough present the link between increased winds, SST and MBL Q. This is now clarified in the updated version of the manuscript:
"In isolated patches further south, upwind of the study area, SSTs tend to be significantly higher on FLC days. This could lead to increased surface latent heat fluxes, increasing the moisture content of the marine boundary layer, at least during AMJ when stronger near-surface winds are apparent. A few 100 km to the west and south of the Namibian coastline, SSTs could similarly add to the increased moisture within the marine boundary layer."

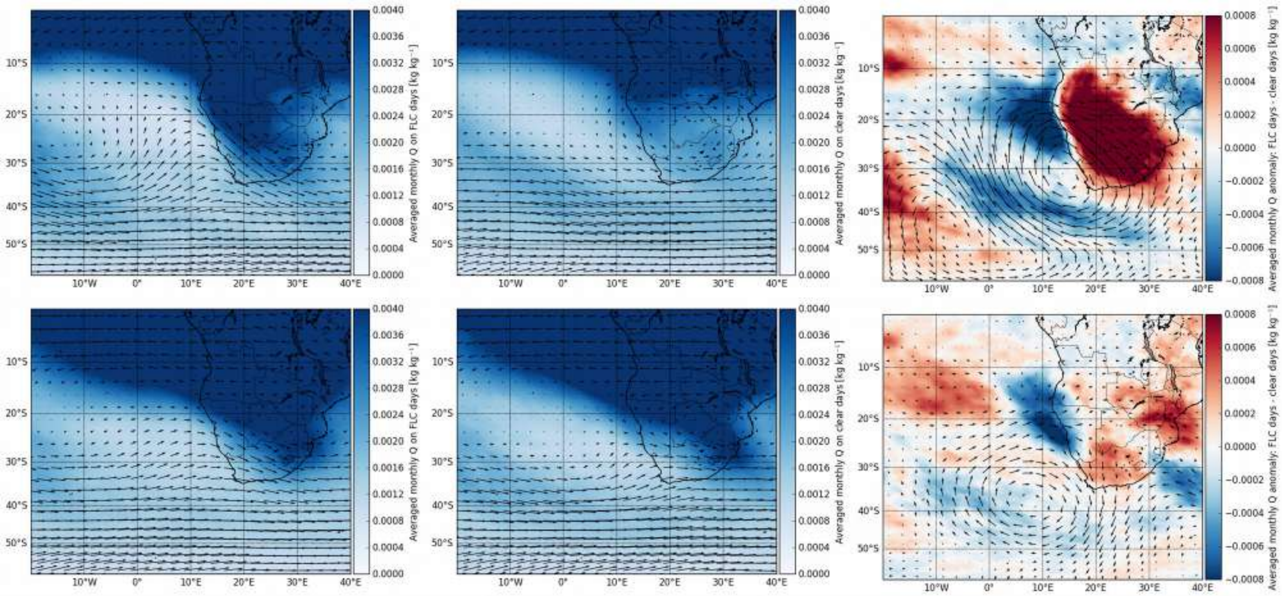


Fig. R2.7: Seasonal averages of Q and winds at 700hPa on FLC days (left), clear days (center), and their difference (right) during AMJ (top) and SON (bottom).

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.

In general, we agree that FLCs will preferentially occur in situations with low SSTs and high LTS. This is actually the reason to show the climatology in Fig. 2 of the manuscript. Of course, the region is rich in FLCs due the low SSTs and the subsidence that combine to lead to very stable conditions. We actually state that "these stable conditions promote the formation of the southeastern Atlantic stratocumulus cloud deck and determine its seasonal cycle [...]". Also, Fig. 3 shows that the differences in LTS over land are almost entirely caused by the surface component of LTS (Pearson correlation coefficient is -0.90 for land pixels for the yearly averages). During SON, this relationship is weaker, which is now discussed in the updated version of the manuscript. We now describe seasonal differences in LTS in a new paragraph:

"While the yearly averaged composites show that over land, LTS is driven to a large extent by T2m (Fig. 3 c) and d)), this is not quite as pronounced during SON (correlation coefficient =-0.57; Fig. 5 c) and e)). Over continental southern Africa, the differences in T2m (Fig. 5 c)) are frequently compensated by similar differences in potential temperature at 700 hPa (not shown). The most pronounced LTS feature during both seasons, however, is the coastal anomaly of increased LTS (over land and weaker over the adjacent ocean), which is driven by T2m. As this anomaly is also apparent during nighttime (1 and 3 UTC, not shown), it is likely that this pattern is mainly due to the relatively warm subsiding continental outflow on clear days, rather than a radiative effect of FLCs as found in California (Iacobellis and Cayan, 2013). During AMJ, LTS is significantly lower over a large marine region south of 25°S, which is likely caused by the synoptic-scale disturbance."

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.)

In the updated version of the manuscript, the potential temperature is additionally shown in the backtrajectory figure. It suggests that the main difference between the trajectory groups remains to be the difference in MBL Q, as stated in the original version of the manuscript.

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?

No, as in Fig. 8, different subsets of the data are compared (only the blue lines of Fig. 7 a) and c)), and differences are computed by following the backtrajectory. As shown in Fig. 5 a) of the manuscript, the lower TCWV is mostly a local phenomenon that is especially pronounced north of ~23°S, and would therefore modify the trajectories only for a limited time.

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.
Yes, indeed changes in MSLP can modify upwelling intensity. However, we would argue that to first order, the differences in MSLP that we find in this paper explain most of the variability in FLC occurrence because they comprise the information on the marked differences in dynamics between FLC and clear days which is clearly shown in the contrasting backtrajectories of Fig. 7 of the manuscript. This is the first-order mechanism, as the offshore winds that are apparent on most clear days (Fig. 7 of the manuscript and Fig. R2.2 of this document) will hinder onshore advection of moist/cloudy marine-boundary layer air masses. In that case it does not matter for FLC occurrence in the Namib whether marine fog or low clouds are formed or not. We do agree that SSTs could be included in the discussion of these results, and will likely be relevant for marine fog occurrence as analyzed in e.g., Dorman et al. (2019).
The manuscript now discusses this in more detail:
"It should be noted that changes in circulation additionally influence upwelling intensity (e.g. Hutchings et al., 2009) such that some of the explained variability may also be attributed to factors influencing FLC formation rather than advection. However, due to the longer time-scale of SST responses, and due to the marked contrasting differences in air mass history on FLC and clear days, the latter is thought to be the first order mechanism leading to the high model skill."

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