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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratocumulus Cloud Clearings: Statistics from Satellites, Reanalysis Models, and Airborne Measurements" *by* Hossein Dadashazar et al.

Johannes Mohrmann (Referee)

jkcm@uw.edu

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General comments: This paper presents a new dataset of stratocumulus cloud clearings off the California coast derived from satellite observations, and examines this dataset with a variety of perspectives, including composites of satellite and reanalysis data, aircraft case studies, and a machine learning-based examination of clearing growth rates. The multitude of approaches is thorough and effective at providing a very in-depth characterization of clearing events. The paper is well-written, the text well-supported by the provided figures, and related work is sufficiently cited and referenced.

With regards to interpretation, there are a few areas where I feel the authors can improve and clarify the message of this paper. The most general is in the interpretation of how the large-scale conditions relate to cloud clearings (mainly sections 3.2, 3.3). For a clearing event to take place and be manually identified as described in section 2.1, two conditions must be met: there must be a cloud deck present, and then there must be a coastal clearing that occurs. In other words, the environment must be initially great for a cloudy MBL, and also eventually (at least coastally) poor for a cloudy MBL. The authors spend much of their interpretation arguing (and convincingly so) why certain factors (e.g. offshore winds) would be detrimental to clouds and result in a clearing, but not much on the first condition. For example, when it comes to interpreting the link between clearing days and enhanced stability (Fig 9b), I would expect that it is not so much that the stability is causing a clearing, but rather the link between strong LTS and cloudiness that allows there to be a cloud deck to erode in the first place. Whether a particular environmental factor is predictive of there being a cloud deck, or predictive of it being eroded, is something that can help understand some of the less explained results in the paper, in particular when comparing clearing vs non-clearing days. An obvious one would be the overall higher cloud fraction on clearing days. Presumably, a day with no stratocumulus deck in which to identify a clearing would be classified as "non-clearing day" (if this is incorrect and non-cloudy days are discarded, this should be clarified in section 2.1), and therefore days in which the large-scale conditions in the NEP were unfavourable for clouds would be mixed together with cases which were very favourable to clouds and no clearing occurred in the 'non-clearing day' category. While it would be sufficient to see this discussed in the interpretation with no additional figures, for their own interest the authors might consider splitting their 'non-clearing days' (of which there are approximately twice as many as clearing days anyways) into two sets, based on some criteria of overall cloudiness, and a three-way comparison between 'overall clear days', 'cloudy days-with clearing' and 'cloudy days-no clearing' might prove more interpretable.

This same point is also relevant for the growth rate discussion. The authors show that the initial growth rate is strongest. A high growth rate would obviously correlate with a larger final clearing area, and this perspective is taken throughout the discussion of growth rate influences, but also a high growth rate may be associated with initially smaller clearings (this is supposition, though the authors could easily investigate in their dataset by examining whether the fastest growing clearings tended to have smaller-than-average initial sizes). Figures 4a supports this however; the presence of a longer lower tail on 9 a.m. size and absence of a longer upper tail on 12 p.m. size (though the log scale might be overemphasizing this) indicates that small initial clearings and not large final clearings are more likely to be the result of a high growth rate. In this case, it would be equally valid to explain why certain predictors of growth rate might be associated with enhanced nighttime cloudiness (again, such as the 1 parameter, T_{850} or possibly LTS), and therefore a well (re-)formed initial deck that is then subsequently susceptible to breakup. Again, this point can largely be addressed in the discussion of results or by author rebuttal and does not require additional figures.

Specific comments:

Section 2.1, line 119: Can you describe in slightly more detail what was necessary for the visual identification of a clearing event? Approximately how large, how distinct, how much cloud had to be adjacent to the clearing? Were days when the Sc deck was completely detached from the coast or absent considered?

Section 3.2 (Clearing vs Non-Clearing)

The difference in subsidence between clearing and non-clearing days seems stark and geographically well-matched to the clearing locations, and yet it comes out as minimally important in the PD analysis. Is the only effect of subsidence to lead to a drier lower FT and therefore all it its signal is captured in T_{850}? The w_{700} discussion seemed very brief.

The difference in AOD (low AOD on clearing days, mainly from 43N and up) may be explainable by the circulations shown in figure 8, with anomalously northerly and west-

erly flow bringing in relatively cleaner air from the marine midlatitudes. That being said, there is no obvious connection between the AOD and N_d maps (low AOD but high N_d on clearing days, though not collocated) that would suggest that the AOD anomalies are having any significant microphysical effect in terms of increasing available CCN, even north of the clearing region. One remedy would be backtrajectory analysis from the low AOD anomaly region, or else looking at the species of aerosol in MERRA-2 to see whether summertime wildfires (which have a large effect on AOD) are impacting the AOD results. The authors state that this may be left for future work, which I would agree with.

Section 3.3 (Growth Rates): It's not clear to me that the condition of requiring only that $r^2 < 0.5$ is a sufficient independence constraint to allow for accurate interpretation of the PD results. For true independence, the authors could have performed an EOF decomposition of all mentioned variables, including those that would clearly correlate strongly with other variables (e.g. LTS, EIS, which as the authors point out are crucial MBL cloud variables), perform the GBRT regression and PD analysis, and additionally the correlation of leading EOFs with input variables. I admit that this would add a level of interpretation, but it would more effectively deal with the tricky problem that so many of these variables are correlated. As it stands the selection of variables seems a little arbitrary, and it is not clear that the resulting ranking of the variables in Figure 11 is physically meaningful. It might be helpful to see another relative ordering of the importance of these variables in accurately determining the growth rate, such as permutation feature importance. Machine learning results are inherently difficult to interpret and the authors have done a more thorough job than many, but one way to improve robustness of interpretation is using multiple evaluation methods.

One area where I think the authors may have stretched the interpretation past the limits of PD analysis is lines 546-558, for instance with the discussion of MSLP and GR. The problem with using PD and correlated variables is that you risk simulating completely nonphysical states which produce nonsensical results. The high and low tails of the

PD sensitivity to MSLP could be a result of the breaking of assumed independence. This could be ameliorated with the addition of a rug plot/histogram to each Figure 12 subplot, showing some kind of likelihood or frequency of occurrence of that particular state (how often a -500 Pa MSLP anomaly occurred in the region affects the degree to which the interpretation of that portion of the PD plot is nonphysical), or the addition of some ICE (individual conditional expectation) plots, both of which are commonly used to help with the interpretation of PD plots.

Technical corrections/suggestions:

Figure 12 caption (line 1250): grey shaded areas, not red.

Figure 13: It would be helpful to see the inversion levels from Table 3 marked on these plots.

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