"A satellite and reanalysis view of cloud organization, thermodynamic, and dynamic variability within the subtropical marine boundary layer"
by Kahn, Brian H. et al.

General comment and recommendation

This manuscript presents a comparison of (correlations between) cloud properties and thermodynamic and dynamic fields derived from AIRS and MERRA data, with those derived in previous literature and derived in this paper from MODIS. The manuscript comes across as rather unfocused, wandering between a variety of objectives, none of which end up convincingly presented. The manuscript appears to: a) evaluate AIRS and MERRA against MODIS and other cited products; b) provide physical insights into what explains the transition by means of reflectance, optical depth, boundary layer depth and effective radius; c) present the AIRS cloud product that can best reflect the transition from stratocumulus to cumulus, where a variety of measures are tried out; d) present different physical behaviours between four regions in which the transition between stratocumulus and cumulus occurs.

Sometimes the lack of focus and a specific question of interest seems to shines through in the authors' writing, for instance, when they introduce new sections, which might represent choices that are "not optimal", but simply provide "a fresh look at available products", or when they describe that choosing which variables to plot in their joint pdfs is challenging. If the focus would be on presenting novel insights, I had expected that beyond abstract descriptions of behaviour of different quantities in the joint pdf's the authors explain what this behaviour actually tells us about observed cloud fields. If the focus would be on an evaluation of AIRS products, I would expect that the evaluation were more thorough and go beyond a comparison of seasonal averaged fields. What contributes to the wandering is that the authors use different data sets for different objectives as they present themselves. AIRS and MERRA are used for interpreting skewness measures and the transition between cloud types, along with comparisons of MODIS. Only MODIS is used for the purpose of evaluating effective radii in the two regimes, and what might physically or methodologically explain effective radii behaviour. In much of the authors assertions, previous literature is referenced, but often not explained.

Because the manuscript does not present a novel insight and fails to convincingly argue for any method or the AIRS or MERRA datasets at providing novel insights, I recommend a rejection of the manuscript.

Specific comments

- The title is unspecific (which satellite and reanalysis data sets?) and promises more insight than the paper offers. The term cloud organization is not well-chosen, because the authors do not present results on cloud organization nor discuss what cloud organization means. The words organised and disorganised are repeatedly used throughout the manuscript, but mostly in reference to *organised* stratocumulus and *disorganised* trade-wind cumulus. Both stratocumulus and trade-cumulus can be organized and disorganized, depending on some definition of organization. Sometimes it seems the authors refer to homogeneous and heterogeneous, but mostly it seems that they refer to the two different cloud types.

- The word novel is repeatedly used, but seems an overstatement. In much of the manuscript, the authors confirm insights found in previous studies, and that citation list is long.

- One novelty that is argued for is the use of AIRS and MERRA datasets at their instantaneous native resolution. But to prove the suitability for these datasets for this kind of study, the authors qualitatively compare the morphology of the stratocumulus to cumulus transition from seasonal averaged AIRS and MERRA data with the morphology known from existing studies. I do not think a qualitative comparison of the seasonal mean transition tells us enough about how good AIRS and MERRA perform at their native resolution.

- The authors make an argument for separating the cloud regimes stratocumulus and cumulus based on infrared-based thermodynamic phase (rather than by dynamical regime such as done in previous literature). The thermodynamic phase provides information about whether just liquid or ice is present in the detected clouds. Based on a single scene in Figure 1 and 2 the authors argue that stratocumulus is well identified by those pixels that are detected as liquid, whereas trade-wind cumulus are those pixels that have an unknown thermodynamic phase. How do the authors know that this separation holds well for other scenes? After all, trade-wind cumulus are also made of liquid only, and it is unclear and not explained why they could not be identified as such in other scenes.

  It is also not clear for what purpose the two cloud types are separated here in this paper. Mostly this seems to be a proposition to use AIRS thermodynamic phase in future studies, but with insufficient evidence.

- One aspect of the paper that prevents it from providing clear physical insights (if this were the main objective) is that the authors never explain what the skewness in reflectance or optical depth tells us about the nature of the cloud field that is observed (and this is true for many of the behaviours derived from the joint pdfs). The skewness measure has been used in previous studies, and can with some background of course be interpreted, but the authors never explicitly do. This makes the description of results rather abstract.

- The discussion in section 4.5 and the conclusions argue for both physical causes (precipitation) as well as retrieval-related biases (inhomogeneity) for the observed larger effective radius in cumulus clouds compared to stratocumulus. But whereas first is stated that (L24) "the observed increase in re is entirely consistent with environmental variability (winds/droplet growth/precipitation)", it is written further along that the greater inhomogeneity in such precipitating cumulus fields can cause assumptions used in retrievals to break down. Hence, should I trust the retrieved larger effective radii observed?

- In the last paragraphs of section 4.6 and the summary, the authors argue two seemingly contradicting statements with which they end their manuscript. Namely, that three of the four regions studied show similar relationships and behaviours among cloud-related quantities and the (thermo)dynamic state, but also that the relationships are non-unique (can vary greatly), for which their datasets provide a good opportunity for further exploration. I understand the subtlety, but is this the best ending?