

Interactive comment on “Cirrus clouds in convective outflow during the HIBISCUS campaign” by F. Fierli et al.

Anonymous Referee #3

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The paper presents cloud data obtained from a balloon-borne lidar flown during the HIBISCUS campaign in Brasil. Data of the same flight but different instruments have already been discussed in other papers of this special issue, i.e. Durry et al. and Marecal et al.. The authors present model studies using a trajectory and a mesoscale model to compare with the observations; satellite data are used as additional information of the general situation and for large-scale comparison with the model.

The argumentation, the elaboration of the results and the supporting material in the manuscript is not well structured. Thus also the scientific content of the paper becomes much weaker than it could be. In the following I give several explanations and recommendations, which should be addressed in a revised version each by its own, but more importantly, might also require a new structure of the argumentation flow.

The presented new balloon-borne lidar data are not really used for a comparison with the mesoscale model. The authors rather use the H₂O data which are from Durrý et al. and Marecal et al., e.g. in Figure 7 left panel. Even if these data are published in the accompanied papers, it is essential to display these data here again, best in connection with Figure 3. The second model parameter shown is the tracer, but neither backscatter ratio nor depolarisation from the lidar observations presented in this paper. The authors claim several times that these optical properties need to be known and investigated, but in fact this issue is not further discussed in the paper with a link to the data.

Two related issues:

What are the crosses in Fig 7 left panel? The saturation data of SDLA look different (down to 10 km). What are the diamonds? The explanation in the text is misleading and not clear.

Figure 3: It's helpful to display in the Figure where the H₂O profile measurements have been obtained as well as to show the H₂O profile or that of supersaturation. Provide in the text information whether SDLA measures gas-phase only, and whether 130% RH_i were measured in or outside of clouds.

The comparison between BOLAM and GOES is made rather qualitative. The agreement is achieved on a general base (as for high cloud area fraction in Figure 5) or by the synoptic-scale evolution in Figure 1. For the detailed comparison with balloon data, the comparison yields agreement as well as disagreement (Figure 7). As it is a single-case study and no sensitivity tests using different microphysical parameterisation are provided, it remains highly speculative whether the disagreement in the aged outflow layer really reflects a deficiency in the simulation of clouds and H₂O. There is only indirect evidence from the tracer discussion that the transport seems to be captured well. It remains unclear what are the reason for the sometimes occurring disagreement and - due to missing statistics - how often and when it can be expected. Thus the conclusion

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that “cirrus clouds can have a complex structure on account of concurrent and partially unknown formation processes, which cannot be fully reproduced by state-of-the-art meteorological model” sounds somewhat simple and much generalised. In this sense, the manuscript has not provided a good step forward to solve the problems which are addressed in the introduction.

Related issue: Figure 1 should be moved into section 4.3 as its main value for the manuscript is to show the comparison with BOLAM; the description of the meteorological situation can sufficiently be done by Figure 2.

One recommendation: A comparison of the BOLAM results with the model studies by Marecal could improve the scientific significance of the manuscript.

The discussion on the trajectory study leads to a negative result. The authors should clearly discuss the value of this finding, e.g. that mesoscale modelling has advantages compared to trajectory models (if this is true?). The paragraph starting with “The reader should be warned that FLEXTRA trajectory analysis may have important uncertainties” does not address this issue appropriately. I do not recommend removing that Chapter as referee #5 suggested, but its value for the full study has to be worked out more thoroughly.

There are many improvements of grammar and style required, but I do not give recommendations at the present stage and refer to those given by referee #5.

In summary, I do not recommend publication of the paper in its present form. However, I see the potential to improve its scientific relevance if the given recommendations are considered.

As a general comment on the TROCCINOX special issue, I think that the strategy to publish several papers on very similar topics and based on more or less the same data has the risk that a single paper includes only an incremental augmentation of our knowledge and thus the significance of the certainly important findings of that project

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