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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Development and impact of hooks of large droplet concentration on remote southeast Pacific stratocumulus" *by* R. C. George et al.

Anonymous Referee #2

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The article by George et al (2013) talks about "hooks" structures produced in SEP stratocumulus performing a comprehensive study using different sources of information (in situ observations, satellite data, trajectories and 3D fully interactive model) explaining its sources, conditions when is produced, characteristics and impacts. It's very well written, very clear and thorough and makes great use of the available literature. These structures have been mentioned before, but a study like this one was necessary to more deeply explore this phenomenon. I support publication after some changes. I feel the paper would be further strengthened if an effort is made to improve model ability to reproduce the timing and extent of observed hooks by using recommendations found in the literature and by including further analysis of the properties of the aerosol plume that generates the hooks.

One of my main concerns is the partial inability of the model to represent the different observed hooks. The 1st hook has a 24 hour shift in the prediction, and for the other 2 the structures are not well represented. The authors should make the best of efforts to configure the model to try to reproduce the events to the best extent that computational time and model limitations provide. Previous WRF-Chem modeling studies (Yang et al. and Saide et al.) both have used finer vertical resolution (64 and 72 layers respectively) with most of the layers in the first 1-3 km, which have been demonstrated to better reproduce the MBL dynamics. As the current study uses 27 layers, this could be a point of improvement. Another difference with the two previous studies is the use of a nested domain, where the previous ones were not nested and obtained boundary conditions directly from analysis. Having the analysis forcing away from the domain boundaries could generate inconsistencies as simulations are continuous and not re-initialized from analysis. As an important concern is the timing of the event, performance could improve if model meteorological initialization is made closer to the event (according to Saide et al (2012), only 3-4 days are needed for WRF to recover from biases in the global analysis). If the authors wanted to go a little further (and if its computationally feasible), they could include an additional 2-way nested 3rd domain centered in Central Chile extending to the south to resolve the complex flow that happens in the region that connects the point of emissions to the point where pollutants go over the ocean. An evaluation of improvement (or not) from adding additional levels of complexity should be made as it would be very useful for the community to know what's really needed to achieve different levels of accuracy.

The discussion of albedo and aerosol sources would benefit from including describing aerosol single-scattering albedo in the hooks, in-situ observed and modeled. Are the source aerosols for hooks in any way different from those in other parts of the region? Do they have different mass or number distributions or composition? Do they undergo changes during hook development that differ from typical aerosol aging? Are aerosol:cloud effects in any way accelerated in the hooks?

Many aspects of the hooks described in this article were already briefly described and explained in Section 3.4 and Figs. 8 and 10 of Saide et al (2012) and in Spak et al., (2010) (e.g. attribution to anthropogenic central Chile sources, entrainment from free troposphere, conversion to SO4 once it reaches the MBL, DMS contribution, location of the subtropical high as source of the off shore flow, etc). Please reference accordingly. Of course a better and more thorough explanation, demonstration and description of these structures was needed, and the one that this article provides does an excellent job.

4.2.2. “Hook impact on cloud properties”. Another interesting cloud property to track over the hook trajectory would be cloud base and top height with and without Anth emiss, where you should expect deeper cloud when the hook is present (Pincus and Baker, 1994).

4.3. Impact of hook on albedo (and other sections). A big influence is found by DMS. Previous studies (Yang et al and Saide et al articles) have found overestimation of DMS against VOCALS observations. I think it would be good to include this comparison for this model configuration to better interpret the results, since if this model is also overestimating observations, then model DMS effects would be amplified compared to reality. Fig 9. The no DMS runs show narrower hooks, more similar to observation. Maybe because DMS is overestimated so is less realistic?

4.6 Sources of hook aerosols. It is mentioned here and in previous sections that when some sources are turned off this changes the location of the hook. Can you elaborate why this happens? Is it the large scale meteorology that changes because of the aerosol interactions? Or is related to having different plumes from different sources that when overlapped look like 1 single hook but when some are removed they just shrink and generate the shift impression?

Page 2494, Line 11. As said in the conclusion, I think is better to say “lower free

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troposphere” to avoid confusions of long-range transport.

3.1 model configuration. Please state if wet deposition is used or not.

Fig 1. Add tick marks of lat/lon to a) and d) as well, not only to c)

Fig 1. 30-40S 90-100W has typical stratocumulus?

Page 2521, line 9. Correct “al.edo”

Pincus, R. and Baker, M.: Effect of precipitation on the albedo susceptibility of clouds in the marine boundary layer, Nature, 372, 250–252, 1994.

Spak, S. N., Mena-Carrasco, M. A., and Carmichael, G. R.: Atmospheric transport of anthropogenic oxidized sulfur over the Southeast Pacific during VOCALS REx, CLIVAR Exchanges, 53, 20–21, 2010.

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Interactive Comment