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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Global and regional modeling of clouds and aerosols in the marine boundary layer during VOCALS: the VOCA Intercomparison" *by* M. C. Wyant et al.

**Anonymous Referee #1**

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This manuscript provides an overview of a multi-model assessment of ability to simulate the aerosols (and aerosol-cloud interactions) documented during VOCALS-REx. It expands on a similar, previous effort that evaluated model behavior without aerosols. Regional, global forecast, and general circulation models are considered.

This intercomparison contributed to the modeling plan that motivated VOCALS. While the study was professionally done, I was not sure what to take away from it. Comparisons are made and discussed, but little interpretation on what aspects of the individual model behavior limit aerosol-cloud interaction representations is provided, limited perhaps by the diversity of models evaluated.

If the above goal is too ambitious, could the authors instead provide the community with some guidance on where it should go with such comparisons? Is there a way to rank models, or can the authors suggest useful metrics [vertical resolution?]? What advise do the authors have for the modeling plans of future campaigns? Would such intercomparison exercises be better served by focusing on models of one type, such as done within GCSS, allowing for a further drilling down to specific process representations, e.g., aerosol wet deposition? This would still allow for one study per model type, and provide more of an apples-to-apples comparison.

On p. 25, we do see a paragraph that compares the performance of Pre-VOCA models to VOCA. Can we conclude from this that incorporating cloud-aerosol interactions does not improve the representation of cloud fraction? Some more in-depth discussion here would be a useful contribution of this manuscript.

I also did not see much discussion of the Ron Brown datasets, despite those comprehensive aerosol and cloud datasets.

Abstract: Many of the statements within the abstract are vague (e.g, "some models simulate the regional low cloud cover well". . ."Most models qualitatively simulate. . ."). Can this [and other statements] be sharpened? Some of the discussion section could probably be summarized in the abstract to add some interest here.

Figures: fig. 3: worth mentioning AMSR-E is day+night in caption. fig. 4: a more general satellite-derived cloud top height would be useful here fig. 8, p 17: see also, for discussion of Ron Brown sulfur/DMS results, M. Yang and 20 co-authors, 2011: Atmospheric sulfur cycling in the southeastern Pacific - âĂĺlongitudinal distribution, vertical profile, and diel variability observed during VOCALS-REx. Atmos. Chem. Phys., 11, pp. 5079-5097. doi:10.5194/acp-11/5079-2011 for a discussion of the ship observations.

fig. 10: spell out SSA fig. 11, discussed on p. 19: a consistent approach to estimating MODIS Nd from space is not yet in the literature (e.g. doi:10.1029/2011JD016155 and



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likely others). Would recommend including the equation used.

4, 6th line “spring” => “austral spring”

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 6537, 2014.