

Interactive comment on “Fast sulfur dioxide measurements correlated with cloud concentration nuclei spectra in the marine boundary layer” by D. C. Thornton et al.

Anonymous Referee #3

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General comments:

Thornton et al. investigated a quite important aspect of CCN production due to certain marker compounds that later on can be a) used for understanding the sources of CCN as well as b) treated as empirical formulas in models. In order to gain that the authors performed certain flight patterns through shallow cumulus clouds. So far so good. But the outcome relies strongly on the correctness of the assumptions of potential sources. In this I fear the authors failed although I appreciate their measurements much. The formation of boundary layer and lower tropospheric aerosols from gaseous precursors depends not only on SO₂-concentrations but on a bunch of different species especially

C7874

when addressing the marine boundary layer. Those can be dimethyl sulfide (DMS), organo halogenes, isoprene and others. The sulphur dioxide has its major implications at anthropogenically influenced areas but not at remote sites. Thus, I guess the study would greatly benefit from including at least DMS measurements too. There are some points I like in this study and some which could be really improved. This I will note below in the specific comments. At this point the study should be carefully revised before being able to be accepted in ACP.

Comments:

Please provide a Figure with a map of the locations of the measurement area. This would allow any reader to get a better understanding of potential inputs. The information is pretty scarce and hard to get. Marine boundary layer is an expression for an atmospheric situation that can persist under very different conditions even in the tropics.

The potential sources (nucleation) and composition of marine boundary layer aerosol should be taken into account and addressed in the introduction before leading to the question of study. In this case one could comment that the input of SO₂ is negligible and thus other sources need to be identified in future work. So far the study is written too quickly. One could find a lot of benefits even in the disproving results.

The high resolution data is nice and required for air plane investigations. Why didn't the authors use a PTR-MS for the organics? Recently the implementation in aircrafts even for stratospheric measurements was achieved. So this might have improved the outcome. Other possible solutions are likely. Although budget is always low, this should be considered when spending the other money for the flights even to rule out other potential contributions.

Anyhow the statement with respect to sulphur dioxide and CCN is robust and opens a

C7875

new aspect of research to clarify in a future study potentially by those authors.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 14903, 2011.

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