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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How important is the vertical structure for the representation of aerosol impacts on the diurnal cycle of marine stratocumulus?" *by* I. Sandu et al.

Anonymous Referee #3

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This paper examines the response of stratocumulus to changes in aerosol content. It uses large-eddy simulation as a proxy for truth and a mixed layer model to understand the impact of associated changes in vertical structure within the boundary layer in modifying the response. In summary, the paper demonstrates very effectively how important it is to consider the entirety of the impact of aerosol changes on the boundary layer evolution, rather than to isolate any particular aspects (as is often still done).

Although this doesn't affect the overall conclusion, I don't think the authors have sufficiently recognised the difficulties LES models have with accurate evolution of stratocumulus (eg. Stevens et al, MWR 2005; Ackerman et al, MWR 2009) and the resolution

used here (10m by 50m) is relatively coarse. In particular, the latter of those two studies appears to show a well-mixed precipitating nocturnal cloud layer in the observations that most models decouple. To what extent do the authors believe the decoupled nocturnal precipitating layer in their LES?

More minor comments:

1. section 2: it is not explicitly stated whether the aerosol changes affect the radiative properties of the cloud in the LES (only the absence of the semi-direct effect is mentioned). Do they?
2. p5471, line 1: the language is strange in "the model disposes of a positive definite..". "Uses" might be better.
3. at the start of section 2 the two classes of simulations were going "to be referred to hereafter as PRIS and POL". It would therrefore help orient the reader to state in 2.3.1 that this section refers to POL and 2.3.2 to PRIS (if I've got that correct)
4. end of section 3: is it one particular forcing regime that generates the opposite sign of response in EML and LES (particularly the points in the bottom right of fig 6)? It would also be instructive to plot the POL and PRIS LES profiles for one of these points to illustrate what it is about the vertical structure the ML model is missing.
5. p5479, line 25: " the entrainment parametrizations are initialised with ...". Do you mean computed from?
6. p.5480, line20: "We recall that, as we noted above...". I can't see where this is referring back to, unless I have just missed it. The authors should be more specific about what outputs from the LES are being used. Doesn't Turton and Nicholls' parametrization require the integrated buoyancy flux profile? How is that obtained? It reads in the end as though it is taken directly from the LES.

7. I think the authors should acknowledge in the conclusions that Turton and Nicolls (QJ 1987) went on to develop a multiple mixed layer model, precisely because they recognised the importance of decoupling. Did the authors experiment with such an extension?

8. final sentence of the conclusions: “The deviations from the mixed layer state ...should be accounted for in future in the schemes of cloudy boundary layers in large scale models”. By including the words “in future” here the authors are implying that this is not already the case. This is at best an over-generalisation. All TKE and higher order closures, for example, will represent the effects on turbulent mixing of stabilisation by precipitation evaporation, for example, and I certainly know of some first-order closure that do as well. The authors should qualify this remark.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5465, 2009.