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Interactive comment on “Marine boundary layer cloud regimes and POC formation in an LES coupled to a bulk aerosol scheme” by A. H. Berner et al.

Anonymous Referee #3

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Very interesting results from 2D and 3D LES modeling of POC formation in Sc using improved but still simplified aerosol parameterization. Simulations are impressive and the results seem compelling. My only request would be to make the comparisons to past work (by Bretherton and others) more explicit, showing comparable results in graphs and tables for comparison. Similarly, the many references to qualitative similarities with obs would be much more useful if key plots from prior work were repeated here for comparison (on comparable scale) so that the reader can easily see what is described as qualitative similarity. This would be a much more useful contribution to the literature, and would help the reader to see specifically which improvements in this

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version of an ongoing series of similar simulations resulted in the more accurate simulation of the VOCALS results. I realize this is not always straightforward to do, but I am confident that the authors can do better job in doing this (since they know the important changes and how to highlight them whereas after reading this lengthy manuscript it is not clear to me). Since I realize that many things are published in ACP without such explicit comparisons to literature, I can only ask that the authors consider this a plea to help out the more novice reader by going a step beyond what is required – since this work clearly merits publication.

Also, I think this paper concludes that POCs are formed by horizontal heterogeneity in the spatial distribution of aerosols. Is that only a sufficient condition or is it necessary? Or does that require further work to explore. I ask since this is an interesting point that merits further study.

Abstract: This reads more like “what we did” rather than “what we learned”. I recommend shortening and focusing on the latter, to highlight: (1) improved aerosol scheme and scavenging, (2) qualitative consistency with some features of B+C oscillations, (3) aerosol gradients necessary/sufficient for POC formation, (4) constant forcing for persistence.

Section 3.2 in order to make it possible compare directly to published msmts, provide number concentrations in #/cc.

Section 6.2 it would be good to justify more explicitly why the 2D winds do not have a major impact on results rather than just saying “we don’t think” it does.

Incorrect: “Thus we hypothesis. . .”

Fig. 17 – how is this different from standard parameterizations? Would be more useful to overlay with such standard isopleths (e.g. Seinfeld and Pandis) so that differences are evident. Otherwise it is not clear why it is needed.

Specific Comments: 1. The abstract was hard to understand. It sounded more like a

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large summery, but I could not follow much of it until after reading the entire paper. 2. In line 10 of P.18147 it says “Satellite observations showed POCs form preferentially in the early morning. . .” I think ‘early morning’ is open to interpretation. Early morning could be just before or just after sunrise, which are completely different meteorological conditions. 3. It seems counterintuitive to call an equation “liquid-ice static energy” but then neglect ice. (line 1 of P.18152) 4. There are several sources referred to throughout the introduction with similar results that I feel could be better consolidated. [ex. POC observations in “ultraclean layer” (line 8-10 of P.18147), “. . .found increased open-cellular organization with decreasing cloud droplet or aerosol concentration. . .” (line 10 – 11 of P.18148), “. . .development of open-cell organization smoothly increased as initial CCN decreased. . .” (line 6 of P.18149)] Currently these similar ideas are spread across a few pages. 5. Σ_g is not really defined, I am a little confused as to what it is. 6. I am not sure how Autoconversion is used here. Doing a quick search gives several definitions, so it would be great if the authors specified their use.

Minor: 1. The mention Na in line 5 of P.18146 before defining it (in line9-10) 2. I do not think drizzliest is a word. (line 11 of P.18147) 3. There are several locations in which ‘an’ is used when ‘a’ should be used. 4. Varying the hygroscopicity of the aerosols might be interesting. Specifically, for a POC case. In the POC cases they vary the horizontal number concentration of aerosol. In a marine environment, I am not sure how a horizontal aerosol distribution like the one mentioned could develop. I would expect some variation in sources to cause this, and a variation in sources between regions, might also mean a variation in hygroscopicity between aerosols emitted.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 18143, 2013.

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