

## ***Interactive comment on “Technical Note: The Role of Evolving Surface Tension in the Formation of Cloud Droplets” by J. F. Davies et al.***

### **Anonymous Referee #2**

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While it is not entirely clear to me why this is being framed as a “technical note,” and not just a paper, I find the results to be generally interesting. There is effectively a very long introduction in which the authors lament that others continue to neglect the role that surface tension can play, followed by some interesting discussion of some new experiments. I suggest that the discussion about the new experiments is expanded.

L150: I find the terminology “schematic dependences” to be a bit unclear. It would be good to see this explained a bit further. Do these come from anywhere in particular, or are they totally made up? What sort of cases are they meant to illustrate, i.e. why these three?

L170: It is not clear to me how the statement that “This... supports assertions that surface tension does not impact activation...” follows from the previous sentence. In

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the examples, the activation diameter for all three non-constant surface tension curves is lower than that obtained assuming a constant surface tension equal to that of pure water. In fact, the authors go on to say just this. So, it would seem that the activation diameter is impacted. It would be helpful if the authors could clarify their particular point here.

L182: The “discontinuity” that the authors refer to here is not entirely clear to me. Do the authors mean that each of the three variable curves shown suddenly changes relatively to assuming a constant surface tension of 30 mN/m? This is not clear from the words, as the discontinuity does not occur “at its minimum surface tension” but instead when there is a (in the model used here) discontinuous increase in the minimum surface tension.

L235: It would be helpful if the “relatively simple, phase composition- and morphology-specific surface tension model” were described here, in brief. If I understand correctly, a linear mixing rule is used.

L235: It would also be helpful if the authors were to clarify whether the model predicted the surface activation effect completely independent of the observations, or whether some amount of model tuning was included (perhaps associated with exactly what species were chosen to represent the organic phase).

L244: I suggest that the authors modify the sentence ending “process” to say “process, for these systems.” It is not clear that this is the case for all systems. If it were, we might expect to see a lack of closure more generally in ambient measurements.

L251: What is stated does not appear to be the case for the example shown in Fig. S3.7.4 in the Ovadnevaite paper.

L305: I do not find it clear what the authors mean when they state that the compressed-film model “more closely predict[s] the observations.” More closely than what? The simple model assuming a constant  $\kappa_{org} = 0.35$  seems to do quite well. I do get

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that the authors are working to say that  $\kappa_{\text{org}} = 0.35$  is not the correct answer and thus the good agreement is not for the right reason. Yet this discussion requires clarification.

L315: The evidence that the apparent (unrealistic)  $\kappa_{\text{org}}$  is particle size dependent is very compelling.

L316: Is the compressed film model here simply inadequate, even with determination of new parameters? Or can it work if it were retuned to this dataset? If the latter, this would give some indication of how uncertainty in the derived parameters for the model impacts subsequent calculations.

L328: This is only true for the smallest particles.

L330: I am not seeing entirely what information is lost by choice of procedure or data representation. Both ways of representing the data seem to drive one to similar conclusions: for a given representation the assumption of a constant  $\kappa_{\text{org}}$  can allow for model-measurement agreement, but the  $\kappa_{\text{org}}$  values are not necessarily realistic. I do not understand why the authors couldn't similarly apply the models shown in Fig. 2A/B to the data in Fig. 2C? There need not be loss of information.

L348: It is not clear to me how these are AIOMFAC calculations any longer, if the organic and salt are forced into separate phases. Does one even need AIOMFAC to do this?

L361: How is it that the essentially made up model (assuming a complete surface film) does better than the model that explicitly accounts for the system thermodynamics? Is this just saying that the thermodynamic model is wrong (e.g. bad parameterization or parameterization used outside the RH range where it is valid)? Or is there perhaps some kinetic effect that must be accounted for? The discussion that comes a few sentences later seems to suggest perhaps the former. But, some additional and more explicit discussion is necessary, rather than a statement (line 375) that this finding is

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“intriguing”.

Minor Comments:

Fig. 1: It would be good to change from the red/green color scheme to one that is more friendly to color blind people.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1201>, 2018.

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