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Comment

## ***Interactive comment on* “Supersaturation calculation in large eddy simulation models for prediction of the droplet number concentration” by O. Thouron et al.**

### **Anonymous Referee #2**

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This article compares three different methods to compute supersaturation and its impacts on CCN activation using Large Eddy Simulations. The paper is concerned with the spurious supersaturation peaks that occur in Eulerian cloud models when supersaturation is coupled with explicit condensation and droplet activation schemes. The problem is an old one that has been examined at intervals over the past 30 years. The authors here test three different schemes for diagnosing or partially prognosing the supersaturation. The paper is clearly written and easy to follow, though there are a few spelling and typographical errors. I think the article should be acceptable pending the revisions regarding prior methods of computing supersaturation given below and I think this revision should be relatively minor.

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The method of computing supersaturation in this paper varies from simple to complex. A very simple method (Scheme A) in which liquid is determined by saturation adjustment and CCN activation is predicted based on a Twomey-style equation. Such methods are only applicable to situations in which cloud motions are not resolved, as is typical of GCMs, and hence all processes affecting supersaturation are occurring on the subgrid-scale. The second method (Scheme B) is a diagnostic approach in which supersaturation is derived first based on advective and thermodynamics processes, and then this supersaturation is used to drive nucleation and growth. The third method (Scheme C) uses the explicit supersaturation equation that has a source through vertical motion (cooling)/advection, and a sink through condensational growth. This semi-prognostic approach first computes the change in supersaturation due to advective motions as a simple difference over time, the condensational sink derivative is then computed explicitly using the supersaturation from the current time. The supersaturation at the next time-step is then computed by assuming that the supersaturation change over time is linear (derivatives are multiplied by the time-step.) The authors show that substantial problems occur when the adjustment scheme is used, which is not surprising given that the method is not applicable for modeling the sub-time step evolution of the supersaturation when vertical motions are resolved and grid-spacing is relatively small. Spurious cloud top supersaturation peaks occur in all of the schemes, but it is Scheme B and C that are of most interest since they are arguably most relevant to situations in which the dynamic forcing of the microphysics is resolved. The spurious supersaturation spikes then produce artificial nucleation of CCN at cloud top, an effect that one would like to mitigate if possible since it is an erroneous source of drop concentration occurring in the very region where radiative and entrainment processes are important. It seems logical that Scheme C would perform better than Scheme B when it comes to mitigating cloud-top supersaturation peaks and hence artificial drop formation: By coupling advective sources and condensational sinks so that the time-variation of the supersaturation depends on both should lead to a better overall estimate of the supersaturation. Scheme C certainly seems to reduce the number of instances of su-

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persaturation spikes as compared to Scheme B leading only a small increase in the drop concentration in the vicinity of cloud top. For the moist case, the increases in supersaturation and drop activation at cloud top are very small for Scheme C.

The title touts this method as a new way to compute supersaturation in cloud models, specifically LES. However, the authors don't clearly indicate which of the schemes being tested are currently used in LES that represent CCN activation. I cannot imagine that Scheme A is used, though perhaps it is (as I stated above this method seems more appropriate for GCMs.) If this paper is testing LES parameterizations of supersaturation, then why not also test some of the other methods that have been used and proposed in the past? For instance, Stevens et al. (1996) improved Tzivion et al.'s (1989) prediction of supersaturation by including the vertical motion term and then analytically solving for the change in vapor excess over a time-step. The solution is an exponential decay, the average of which is then used to force condensational growth and activation. I may be mistaken, but it seems that Scheme C is a linearization of this approach, though with the full advection and thermodynamic terms included in the thermodynamic forcing part of the supersaturation. What about the method that Grabowski and Morrison (2008) discuss in which temperature and vapor mixing-ratio are adjusted instead of adjusting the supersaturation? It would seem that both of these methods are viable alternatives to the one posed in the current paper, and it seems to me that it would be of great interest to compare these different methods. I haven't seen a linear method like the one proposed here published before, and so I am quite curious to know how well the method works in comparison to these other schemes. Otherwise, how will one know whether it is better to choose Scheme C from the current paper, or one of the other methods used in LES in the past? At the least, I think it would be good to know how Scheme C compares to an exponential decay of the supersaturation over a time-step as in Stevens et al. (1996).

Another question comes to mind: I wonder if an explanation should be given of why the supersaturation spikes occur? The authors do discuss this some, but I recall Stevens

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et al. (1998) providing the explanation that the spikes exist fundamentally because a cloud boundary cannot be tracked through a model grid volume. So it seems there is a fundamental problem here with tracking the supersaturation, and this could be a strong argument for a method like Grabowski and Morrison's (2008).

Grabowski, W. and H. Morrison, 2008: Toward mitigation of spurious cloud-edge supersaturation in cloud models. *Mon. Wea. Rev.*, 136, 1224-1234

Stevens, B, G. Feingold, W.R. Cotton, and R.L. Walko, 1996: Elements of the microphysical structure of numerically simulated nonprecipitating stratocumulus. *J. Atmos. Sci.*, 53, 980-1006.

Tzivion, S., G. Feingold, Z. Levin, 1989: The evolution of rain-drop spectra. Part II: Collision collection/breakup and evaporation in a rain shaft. *J. Atmos. Sci.*, 46, 3312-3327.

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[Interactive comment on Geosci. Model Dev. Discuss.](#), 4, 3313, 2011.

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