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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A Model Intercomparison of CCN-Limited Tenuous Clouds in the High Arctic" *by* Robin G. Stevens et al.

Anonymous Referee #1

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The paper presents an intercomparison of simulated summertime mixed-phase Arctic clouds from large-eddy simulations and numerical weather prediction models. The simulated case is based on observations from the 2008 Arctic Summer Cloud Ocean Study when the CCN concentration was very low ($\sim$ 1 cm-3). The study reports the results of several sensitivity tests, which show the dependence of cloud properties on the treatment of cloud droplet activation and on the number concentration of CCN or droplets. Several models are run with fixed and prognostic droplet number concentration representations. Most models show dissipation of the liquid cloud when the CCN concentration falls below a certain value, but the threshold values vary significantly among models. Sensitivity of simulations to ice crystal number concentration (ICNC) has also been tested. In general, it is found that the changes in liquid water

path (LWP) resulting from changes in ICNC are smaller that changes in LWP due to varying CCN concentration, although the ranges of tested concentrations are somewhat arbitrary. Overall, the LWP increases with increasing CCN or droplet number and decreases with increasing ICNC, in agreement with previously published results. Although in many respects models behave qualitatively similarly, there are large quantitative differences. In several sections the manuscript documents performances of and the differences among the models in these sensitivity tests, which is certainly a useful exercise. Unfortunately, the paper provides little insight into the causes of the differences. More analysis and expanded interpretation of the results are definitely needed and more simulations are highly recommended to make the manuscript publishable in ACP. At present, most of the sections describe the inter-model differences shown on the plots but provide little to none of substantive analysis. The paper summarizes a significant multi-institutional research effort and so readers expect to learn more than just that the models produce different results. Many of the statements in the paper that go beyond simple description of the results are either obvious (state previously well-established facts, e.g., that modeled cloud-aerosol interaction depends on droplet size distribution and autoconversion scheme) or speculative. I strongly encourage the authors to expand the analysis to tease out specific reasons for the differences and hopefully provide practical rather than general recommendations to other modelers and observationalists. Several more specific comments and recommendations are given below.

General comments: 1) Model and simulation choices: Although six models participate in the intercomparison, for each considered case no more than 2 LES models or 3 NWP models can be directly compared, so the representativeness of the results is somewhat questionable. The setup of LES and NWP models is so different by design, that comparing or even putting them on same plots is not really meaningful. Although in the text the paper acknowledges these differences between LES, run with constant forcing and aimed primarily at steady state regimes, and NWP models, in which results at any location are affected by mesoscale variability evolving in time, the presentation of the results in figures implies that the two groups are stacked against each other, which will likely confuse some readers. Furthermore, out of 3 LES models (UCLALES-SALSA, COSMO-LES, and MIMICA) only MIMICA ran all the cases, while the other two do not even have a single overlapping case. Simulations from three NWP models (COSMO-NWP, WRF, and UM-CASIM) are more evenly distributed with 10 cases run by all three. 2) LES results: It is hard to make sense of LES results because the models disagree a lot even for a seemingly simple case of no ice and "high" droplet concentration (80 per cc). LES of boundary layer clouds have a long history of intercomparisons, including several for super-cooled clouds (e.g., M-PACE, SHEBA, and ISDAC cases), so the modeling community has a relatively good understanding of what the simulations should behave like under similar conditions. The models used in this study are relatively new and it is not clear how they measure up against an ensemble of previously tested models. Cited "numerical instabilities" that prevented COSMO-LES to be run for the required period of time are very disconcerting and cast shadow on all simulations from that model. Notable differences in LWP between USLALES-SALSA and MIMICA in the CCN80prog_noice case, which are apparent in figure 7, deserve an explanation, especially since UCLALES-SALSA employs an original and relatively untested microphysics scheme, which is different from what was previously used in the UCLALES model. Repeating these simulations with collision-coalescence (or autoconversion) turned off may help to identify the sources for the difference. Explaining this difference may also shed light on much stronger sensitivity of SALSA microphysics to the droplet concentration reduction from 80 to 30 per cc. 3) Aerosol, CCN, and droplet concentrations: The study targets clouds with extremely low droplet concentrations ($\sim$1 to 10 per cc) and more justification is needed to convince readers that these concentrations are relevant for the considered case. This can be accomplished by answering, or at least discussing, the following questions. Are surface-based CCN measurements representative of cloud layer conditions, given that, according to the sounding in figure 2, the cloud layer between 600 and 1000 m appears to be decoupled from the surface? Supersaturation of 0.2 % for which the CCN concentration were measured may

represent conditions in boundary layer clouds in a typical aerosol environment. When the CCN concentration is very low, however, wouldn't higher supersaturation values be achievable even in clouds with moderate updrafts? Another way to look at it is this. Are there smaller aerosol particles, or less efficient CCN, that could be activated in the considered clouds? Finally, are there any measurements, direct or via remote sensing retrievals, of actual droplet concentrations in these clouds that would serve as a target for model simulations?

Technical comment: P 5, lns 19-25: It is worth to provide a brief basic description of the ASCOS campaign, specifying location, overall synoptic situation, whether all measurements were collected from the surface or whether aircraft was involved, etc.. Table 1: (i) typo in coarsest vertical resolution below 2 km for COSMO-LES; it should be smaller than 228.3 m; (ii) any reason why LES models don't use identical horizontal grid size? Seems like an extra and unnecessary source of uncertainty to deal with; (iii) please include domain size in the table, at least in horizontal directions. For NWP models it is given somewhere in the text, but I don't recall seeing numbers for LES. P 9, ln 31: remove "five" P 10, ln 8-9: delta_ICNC has units of concentration and, therefore, is not technically a "rate". P 13, ln 16-17: Showing profiles from a single grid column in the middle of the domain is an unorthodox way to compare LES models. Wouldn't the comparison be more robust if domain mean profiles were used? NWP output can then also be averaged over LES domain-size area to be more comparable in terms of represented horizontal scales. P 13, ln 26: "Despite no inclusion of ice . . ." The sentence does not make sense, because ice won't help models in their current setup to produce clouds. Please re-phrase. P 14, figure 3: Here and in number of subsequent figures, the deepest mixed layer or highest cloud top seems to be predicted by a model with coarsest vertical resolution (COSMO-NWP). May be worth pointing this out. P 14, ln 10: ". . . adequately resolve . . ." This statement seems too optimistic. Although time varying advective tendencies at the studied location in NWP models are almost certainly more realistic than constant tendencies imposed on LES, it is not clear how "adequate" they are. Boundaries are 100's km away from that location and so even if the boundary conditions were perfect (they are not), shallow layers and sharp inversions can be significantly eroded during advection, e.g., due to excessive diffusion because of coarse vertical resolution. P 15, figure 4: High autoconversion rates in COSMO-LES between 200 and 700 m altitudes are puzzling since no cloud water is shown for these levels in figure 3. An explanation is needed here. P 16, ln 1-2: Since different papers often use different formulations and/or notation for gamma distributions, please provide the functional form of that distribution. This would also define your shape parameters, which are currently undefined. Also, in most common notations, nu=0 results in an exponential size distribution, which is often employed for precipitation species, but presents a questionable choice for cloud droplet size spectra. Please clarify what droplet size distributions are used in COSMO and UM-CASIM and, if they are indeed exponential, justify the choice. P 16, ln 31: “mass concentration”: I think “mixing ratio” was used earlier in the paper. Better to use the same terminology/unit throughout. P 18: This is one of the sections, which lacks a clear message. What the reader is supposed to take out of this, except that the model results differ? The description in this section is mundane: turbulence may contribute, collision-coalescence could play a role, and an activation scheme obviously affects how many droplets are formed. Is there anything new that the intercomparison can teach us and that has some broader implications? P 19, figure 6: here and on other figures, lines that are called “red” and “purple” are hard to distinguish on this plot. P 20, ln 7-9: “It is therefore possible . . .” As written, it is not clear if narrower spectra in UCLALES-SALSA is the author’s speculation or an actual finding of the study. This can be shown clearly and explicitly by plotting the cloud droplet size distributions from different models. P 20 ln 10-28: I find it odd to pull 1 out of 3 figures in this set of sensitivity experiments out of the main paper into the supplement leaving 1/2 page description in the text. Suggest to put the figure back into the paper. Figure S2, caption: “CCN80prog_noice” should be “CCN30prog_noice” P 21, figure 7: Non-zero CDNC throughout the vertical column in MIMICA during the model spin up looks odd and should probably be masked outside of clouds. P 22, ln 2-3: Not clear what is meant by the statement “Evaporation of falling rain . . . transports

moisture . . . to the lower cloud layer”, since the cloud layer is presumably saturated. Please re-phrase. P 22, ln 21-25: This section summary is not very insightful. Do we need a model intercomparison to state that the decrease in CDNC leads to thinning or collapse of the cloud layer and that this effect is sensitive to cloud-rain partitioning? P 31, ln 1-2: The recommendation to include linkages between aerosol and clouds in models is too general to be useful. Please elaborate. Many, if not most, climate models include prognostic aerosol and CDNC. Whether prediction models are a different story though. Do you recommend that NWP models move to prognostic aerosol too? Should they consider assimilating aerosol information? Please be more specific. P 32, ln 16-26: Specific “threshold” CCN or CDNC values for a given model and a single case are of little value to the broader modeling community. On the other hand, the authors can do more to disentangle some of the effects at play here. E.g., CDNC is certainly a factor in autoconversion rate, but the rate also depends on LWC. The models predict hugely different LWC and it is not clear whether different autoconversion rates are the reason or the consequence of differences in LWC. The most direct way to determine this is of course to swap the autoconversion parameterizations between the models. Another approach would be to examine autoconversion rate normalized by LWC, which may provide some hints into the interplay between the two but could still be affected by other factors. P 33, ln 9-13: Surface fluxes no doubt are important in the Arctic, but how is this relevant to the current study. Aren’t the fluxes set to zero for LES models and very small in NWP models? And isn’t the cloud layer, in fact, decoupled from the surface to begin with?

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