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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulated and observed horizontal inhomogeneities of optical thickness of Arctic stratus" *by* Michael Schäfer et al.

Anonymous Referee #2

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General comments

In the manuscript the authors compare optical thickness of Arctic stratus cloud observed in the course of VERDI measurement campaign to that simulated by LES COSMO model. The authors conclude that the model produce clouds more homogeneous that the observed ones, yet the directional structures and the tendency of increasing /decreasing degree of inhomogeneity are reproduced in the simulations.

The attempt described in the manuscript is interesting, important and well described, however the results are not convincing. The real problem of the study is insufficient resolution of the numerical simulations to effectively match the experimental data. The majority of efforts is set to averaging of the experimental data which allows produce optical thickness fields of the resolution comparable to the model output due to limited

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Discussion paper

model domain and poor resolution of the simulations. This causes that conclusions are weak and too far going. Nevertheless, even weak and unconvincing results from very challenging efforts can be published on condition of critical analysis of the results and suggestions for improvements. I suggest a major revision of the paper before final acceptance.

Specific comments and suggestions for improvement

p. 5 Fig.2 Why you do not show wind components? Later you discuss directional shear...

3.1. Simulations Model set-up is not detailed enough. Please describe fluxes, radiation, microphysics in few sentences, referencing is not enough. Subversions on May 14-15 and 16-17 are at very different heights. Was vertical resolution at inversion height comparable?

p.6 l. 17 “to avoid numerical issues” really? Or data from dropsondes represent actual realization along trajectory, not a good choice for initial profiles?

p. 7 l. 12 WD in Fig 4 I guess is for wind direction, but generally the figure is hard to interpret. E.g. the same wind shear whether in the middle of the given colour and at the edge of colours can be visible or not. I fill not comfortable with this plot.

Section 4.2 The section shows nicely discrepancies between the experimental data and the simulation. Why in conclusion do you not call for higher resolution simulations? In the supplementary material of the paper you cite (Pedersen et al., 2016) there are suggestions that basic cloud patterns are reproduced reasonably in smaller domain. Why do you not perform sensitivity analysis due to model resolution?

Section 4.3 Again: your model domain larger than the swath of the measurements. Why not to run model in smaller domain but at higher resolution? In particular when you conclude that the decorrelation length increases with decreasing spatial resolution. . ..

Section 5.1. I think that your conclusion that the motel captures temporal changes of

inhomogeneity is not well justified, there are only 4 points analysed. Moreover, the maximum modelled inhomogeneity is dated May 16th, while is observed on May 14th. On these days vertical profiles indicate that clouds ans boundary layer properties on these days are substantially different.

Section 5.2. Results in this section are more convincing. However, these results could be strengthened discussing dynamical patterns (convective rolls) th the boundary layer. Does the maximum optical thickness correlate with location of updraughts and maximum cloud top heights? Analysis of that could help to publish the paper, since conclusions are weak and should be supported with additional investigations which can increase our understanding of modelled processes. This is particularly important in terms of your sensitivity study in Section 6.

Section 6. Again: this section calls for more thorough analysis as pointed above.

Section 7. After the additional analyses this section (and abstract) should be updated adequately.

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