Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-1135-RC2, 2019

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Arctic cloud cover bias in ECHAM6 and its sensitivity to cloud microphysics and surface fluxes" *by* Jan Kretzschmar et al.

Anonymous Referee #2

Received and published: 4 January 2019

This manuscript uses satellite observations from CALIPSO to evaluate Arctic cloud cover in ECHAM6. The authors found that low liquid cloud cover in the Arctic is biased high over surfaces covered by snow and ice in the default version of the model. They investigate two potential reasons for the high bias — the strength of surface heat fluxes and the impact of the Wegener-Bergeron-Findeisen (WBF) process. The authors conclude that surface heat fluxes are too strong in the default version of the model and that they can instead decrease their high bias in Arctic low liquid cloud cover by allowing for slight supersaturation with respect to ice in their cloud cover scheme, which in turn impacts the WBF process in ECHAM6.

I have numerous concerns about the manuscript that are primarily related to the methodology and conclusions drawn by the authors. My comments are below.

- The description of the observational dataset does not contain a discussion of observational uncertainties associated with CALIPSO/GOCCP. Namely, lidar beam attenuation is particularly problematic in the Arctic, where many clouds are optically thick, liquid, low-lying and precipitate snow. When compared to ground-based observations in the Arctic, CALIOP cannot "see" clouds in the lowest few kilometers (see e.g. Liu et al. (2017)) and the difference with GOCCP can be quite substantial especially over the Greenland ice sheet (Lacour et al. (2017)). This was also noted to be problematic in Cesana et al. (2012), and mostly affects precipitating ice underneath optically thick liquid clouds. I worry that the authors claim of a high bias in low, liquid clouds in the Arctic and their comparison for ice clouds may be inaccurate for the aforementioned reasons. The disadvantage of ground-based remote sensing observations, of course, is their lack of spatial coverage. I would still, however, recommend that the authors incorporate Arctic ground-based remote sensing observations from a few sites collocated with GOCCP to get an idea of potential biases that might impact their conclusion. Furthermore, the description of the observational dataset also does not mention the vertical resolution and criteria used for phase discrimination in the GOCCP product. Were daytime and nighttime data used? What timeframe was used? Were data before prior to the change in nadir-viewing angle used? How were oriented crystals handled?

- The authors note that ECHAM6 mixes too strongly in the Arctic and instead decide to turn to the model's parameterization of the WBF process instead to attempt to remedy the bias in Arctic cloud cover. To this end, the authors increased the efficiency of the WBF process by decreasing the threshold of in-cloud ice water mixing ratio required to activate the depositional growth of ice. However, it appears that the authors are unaware that ECHAM6 (Lohmann and Neubauer (2018)), like many other climate models (Komurcu et al. (2014), Cesana et al. (2015), McCoy et al. (2016)), underestimates the proportion of liquid to ice in mixed-phase clouds. Decreasing the efficiency of the WBF process would only exacerbate this underestimate (Tan and Storelvmo (2016), Lohmann and Neubauer (2018)), which could also affect the climate sensitivity of the model (Tan et al. (2016), Lohmann and Neubauer (2018)). Thus, although the bias in cloud cover might be remedied, the partitioning of cloud phase would be further exacerbated. I would recommend the authors to look into how cloud thermodynamic phase is affected in the model before retuning the WBF process, which previous studies have already shown to be too efficient in climate models, including ECHAM6. Why do the authors choose to focus on the WBF process? Why not ice nucleation for example, which also plays an important role in Arctic radiation (Prenni et al. (2007), Xie et al. (2013))?

- The authors note that although there were improvements to Arctic low liquid cloud cover by increasing the efficiency of the WBF process, total cloud fraction remained overestimated. To this end, the authors then modified the cloud cover scheme to allow for slight supersaturation with respect to ice in the model (their "NEW" experiments). The authors seem to point out in the main text that cloud although some of the high bias in low-cloud fraction is reduced in their NEW simulations, new low-biases in low-cloud cover are introduced. Although improvements to the high bias in low-cloud fraction were highlighted in the abstract and conclusions, they authors fail to mention that there appears to be a simultaneous introduction of a new low bias in low-cloud cover. In fact, this low bias in Arctic low-cloud fraction was already shown for the CAM5 model (Kay et al. (2016)), which allows for supersaturation with respect to ice (Gettelman et al. (2010)). Therefore, the authors' parameterization does not seem to entirely solve the problem of the high bias in low-clouds in the Arctic, and the problem now reduces to an issue known to already exist in another model.

  Also, although their temperature-weighted scheme for saturation vapour pressure may be new to the ECHAM6 model, it is not a new concept to climate models.

  Please cite previous work that have used similar weighting schemes in the calculation of saturation vapour pressure:

  1. Fowler, Laura D., David A. Randall, and Steven A. Rutledge. Liquid and ice cloud microphysics in the CSU general circulation model. Part 1: Model description and simulated microphysical processes. Journal of climate 9.3 (1996): 489-529.
  2. Lord, Stephen J., Hugh E. Willoughby, and Jacqueline M. Piotrowicz. Role of a parameterized ice-phase microphysics in an axisymmetric, nonhydrostatic tropical cyclone model. Journal of the atmospheric sciences 41.19 (1984): 2836-2848.
  3. Wood, Robert, and Paul R. Field. Relationships between total water, condensed water, and cloud fraction in stratiform clouds examined using aircraft data. Journal of the atmospheric sciences 57.12 (2000): 1888-1905.

- Section 3: It seems to me that there is a "chicken and egg" game when using observations of the vertical profiles of temperature and humidity to establish a cause for high bias in low liquid clouds in the model. Low-clouds can in turn affect temperature and relative humidity, so how can one establish the cause for the low-cloud bias?

References:

1. Liu, Yinghui, et al. Cloud vertical distribution from combined surface and space radar–lidar observations at two Arctic atmospheric observatories. Atmospheric Chemistry and Physics (Online) 17.9 (2017).

2. Lacour, Adrien, et al. Greenland clouds observed in CALIPSO-GOCCP: Comparison with ground-based Summit observations. Journal of Climate 30.15 (2017): 6065-6083.

3. Cesana, Gregory, et al. Ubiquitous low-level liquid-containing Arctic clouds: New observations and climate model constraints from CALIPSO-GOCCP. Geophysical Research Letters 39.20 (2012).
4. Lohmann, Ulrike, and David Neubauer. The importance of mixed-phase and ice clouds for climate sensitivity in the global aerosol–climate model ECHAM6-HAM2. Atmospheric Chemistry and Physics 18.12 (2018): 8807-8828.
5. Komurcu, Muge, et al. Intercomparison of the cloud water phase among global climate models. Journal of Geophysical Research: Atmospheres 119.6 (2014): 3372-3400.
6. Cesana, G., et al. Multimodel evaluation of cloud phase transition using satellite and reanalysis data. Journal of Geophysical Research: Atmospheres 120.15 (2015): 7871-7892.
7. McCoy, Daniel T., et al. On the relationships among cloud cover, mixed-phase partitioning, and planetary albedo in GCMs. Journal of Advances in Modeling Earth Systems 8.2 (2016): 650-668.
8. Tan, Ivy, and Trude Storelvmo. Sensitivity study on the influence of cloud microphysical parameters on mixed-phase cloud thermodynamic phase partitioning in CAM5. Journal of the Atmospheric Sciences 73.2 (2016): 709-728.
9. Tan, Ivy, Trude Storelvmo, and Mark D. Zelinka. Observational constraints on mixed-phase clouds imply higher climate sensitivity. Science 352.6282 (2016): 224-227.
10. Prenni, Anthony J., et al. Can ice-nucleating aerosols affect Arctic seasonal climate?. Bulletin of the American Meteorological Society 88.4 (2007): 541-550.
11. Xie, Shaocheng, et al. Sensitivity of CAM5-simulated Arctic clouds and radiation to ice nucleation parameterization. Journal of Climate 26.16 (2013): 5981-5999.

12. Kay, Jennifer E., et al. Evaluating and improving cloud phase in the Community Atmosphere Model version 5 using spaceborne lidar observations. Journal of Geophysical Research: Atmospheres 121.8 (2016): 4162-4176.
13. Gettelman, Andrew, et al. Global simulations of ice nucleation and ice supersaturation with an improved cloud scheme in the Community Atmosphere Model. Journal of Geophysical Research: Atmospheres 115.D18 (2010).

Minor Comments:

- Abstract, line 9: “Phase partitioning" typically refers to mass ratio or frequency ratio defined as liquid/(liquid + ice) in mixed-phase clouds within a grid cell or specified domain. Here, the authors refer to the ratio of total low liquid cloud cover to total cloud cover. I recommend changing the terminology to avoid confusion.
- I suggest changing the title of Section 2.1 to “GOCCP" to reflect the fact that this CALIPSO-derived product was used in the analysis.
- Page 2, lines 20-23: I would also mention the advantage that active satellites are also able to provide vertical profiles of clouds.
- Page 5, lines 10-13: If the mid-level cloud bias is similar to the low-cloud bias because of how low- and mid-level clouds are defined, then shouldn’t that mean that the bias in mid-level clouds for JJA should resemble the bias for high clouds? It does not appear to.
- Page 5, line 20: This is an overstatement without formal proof. I would suggest replace “is” with “appears to be”.
- Page 6, lines 19-22: This is an interesting hypothesis that may or may not be true. I would be more careful in emphasizing that the statement is speculative.

- Page 8, line 13: Please add a reference for the WBF process and note the ways in which models simplify it (e.g. lack of dependence of vertical velocity). Please see Korolev (2007).
- Page 8, line 21: “will" should go in front of “depositional".
- Page 10, Lines 11-12: Please specify that this the overestimate is with respect to GOCCP.
- Page 11, lines 15-17: I disagree with this statement. The Karcher and Lohmann paper refers to cirrus clouds. In mixed-phase clouds, where liquid and ice clouds coexist and the WBF process occurs, the cloud may not necessarily glaciate immediately and will instead depend on how the liquid and ice are spatially distributed within the cloud (Tan and Storelvmo (2016)).
- Page 12, Line 17: “reduce to" ⇒ “reduce the"
- Page 12, line 18: Please specify that supersaturation is with respect to ice.
- Figure 4: strength ⇒ strength
- Figure 5: Please consider labelling the first value as the “default” value of the model in the legend of this figure for easy reference.
- Please remove all instances of “the" in front of “Arctic amplification".

Reference:

1. Korolev, Alexei. "Limitations of the Wegener–Bergeron–Findeisen mechanism in the evolution of mixed-phase clouds." Journal of the Atmospheric Sciences 64.9 (2007): 3372-3375.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-1135, 2018.