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Interactive comment on "Impact of poleward heat and moisture transports on Arctic clouds and climate simulation" *by* Eun-Hyuk Baek et al.

Anonymous Referee #1

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This manuscript evaluates the impact of the UNICON unified convection scheme on Arctic cloud biases by comparing AMIP simulations using CAM5 (which implements the Park & Bretherton (2009) and Zhang & McFarlane (1995) convection schemes, against AMIP simulations using the SAM0 model. The authors find that the UNICON scheme increases moisture transport from lower latitudes to the Arctic and results in enhanced cloud fraction and decreases in surface flux and cold surface temperature biases in the Arctic. They show that enhanced poleward moisture transport is also associated with enhanced total and low-cloud cover in the CMIP5 models.

The manuscript presents the potentially interesting finding that poleward moisture and heat transports can somewhat improve the low cloud fraction and liquid biases and therefore radiation budget, surface temperature and sea ice in the Arctic in cli-

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Discussion paper

mate models relative to observations, but I find that the conclusions drawn from the manuscript are very speculative and based on correlation rather than drawn through rigorous analysis. Major and minor comments follow.

Major comments: * It is not clear whether the only difference between SAM0 and CAM5 is the UNICON scheme. Is this the case? Even so, the scheme itself seems to contain many different changes, making it difficult to isolate cause and effect. There is an awful lot of speculation in the manuscript, from attributing increases in cloud fraction and liquid in the Arctic to increased condensation rate to attributing the increase in condensation rate to poleward transport of heat and moisture. Because it's not clear what exactly is different in SAM0 compared to CAM5 without reading the references in the manuscript in detail, I recommend that the authors conduct sensitivity tests to isolate the individual effects they are speculating. For example, if the authors claim that poleward moisture and heat transports are the main factors in SAM0 that cause an increase in condensation rate in the Arctic, then they could do sensitivity tests where they increase and decrease poleward moisture and heat transports in SAM0 by varying degrees to get a sense of whether or not they are dominant factors in affecting Arctic condensation rate. * The errors of the two observational datasets and Reanalysis data used are not discussed or addressed whatsoever. Please include a detailed description of the errors and biases in all three datasets. In particular, the GOCCP dataset does not account for lidar beam attenuation, which is particularly problematic in the Arctic, where optically thick supercooled liquid clouds attenuate the beam. Precipitating ice particles underneath these layers, which are known to commonly exist, would not be detected. If comparing the results of the models to GOCCP alone in terms of cloud amount, GOCCP might underestimate the actual cloud amount. I suggest that the authors either include a ground-based observational dataset to get an idea of the potential biases involved when comparing the models to GOCCP. * Although SAM0 is able to produce more low cloud amount and cloud liquid and less cloud ice as illustrated in Figure 2, it is not clear from the figures until Figures 6-9 how the models compare against observations. It could be that SAM0 overshoots low-cloud amount/cloud liquid or undershoots cloud ice relative to the observations. I would suggest including observations in Figure 2 as well. This could be done if the authors were to e.g. run the model in single column mode and compare their results with ground-based observations from the M-PACE field campaign. This could also provide additional evidence to support the authors' claims using an additional complementary ground-based observational dataset. This should also be clarified on lines 17-19 in the Abstract, where it should be specified what observational dataset the reduced biases are with respect to. * Figure 3: Why these microphysical tendencies? Why not include other microphysical processes such as accretion, autoconversion, wet/dry deposition as well? This analysis may be missing processes that are more important than net condensation rate. Also, the nonlinear interactions between the model tendencies are not quantified in Figure 3; the various processes all feed back and are dependent on one another. The authors could make this analysis more rigorous by quantifying the contribution of these liquid and ice tendencies to cloud liquid and ice mass using a multiple linear regression approach. * The strong negative bias in TCA seems to persist in the summer (Figure 8), yet why does there appear to be little to no SWCF bias? Is LCA more relevant than TCA?

Minor comments: Page 9: LCA was never explicitly defined. I'm assuming this stands for low-cloud amount. Does this include clouds from 700hPa to 1000 hPa? * Section 2.1: What is the vertical resolution of the model? What was used as the spin-up time of the model? Which COSP simulator was used (e.g. was it the lidar simulator?) * Figure 3: Does the WBF process include snow? * Section 3.2: The sentences referring to the temperature inversions are written in a confusing way. It seems like the temperature inversions should be mentioned after the effect of LCA on surface fluxes, not before since it's a consequence of the clouds. * Rather than state that "most of the shaded area" is statistically significant, why not shade the statistically insignificant areas to avoid crowding the plot?

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