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Interactive comment on “Effect of retreating sea ice on Arctic cloud cover in simulated recent global warming” by M. Abe et al.

Anonymous Referee #2

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This study investigates the relationship between Arctic sea ice retreat and local cloud cover using the MIROC5 GCM. The subject matter is timely, and the results are generally consistent with recent research suggesting a positive feedback between expanding open water in the Arctic and cloud coverage that enhances downwelling radiation to the surface. As such, this new study is relevant and appropriate for ACP, although a considerable amount of revising is needed, as described below.

Major Comments:

1. My biggest misgiving about this paper is its assumption of causality that sea ice changes are causing the associated changes in cloudiness, without considering that the reverse might be true or that a third factor might be driving both variables. In

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diagnosing virtually all of the relationships between sea ice and clouds, the authors assume that ice variations are driving overlying cloud variations, but that assumption isn't necessarily valid in the coupled model simulations analyzed here. One way to ascertain causality is to conduct lagged correlations, as was done in the Liu et al. (2012) study that was referenced but whose technique was not applied here. Although plausible, the expectation that sea ice reductions are leading to increased cloudiness needs to be supported with some evidence, because it's also physically plausible that cloud increases occur first and lead to enhanced downward radiation, which then helps to melt off sea ice. One example of the manuscript's assumption of causality appears on page 17535, where the statement is made, "Therefore, the increased cloud cover is confirmed to result from the reduction in sea ice," and shortly thereafter, ". . . the cloud cover increases because of reduced sea ice." One way to address the question of causality is to calculate some lagged correlations between sea ice anomalies and cloud cover to determine whether the ice variations are leading the cloud anomalies. Another helpful addition would be to calculate spatial correlations between trends in sea ice and clouds to quantify the apparent visual agreement shown in Figure 3.

2. Another major deficiency of this study is its complete reliance on a (single) climate model. The study would be stronger if it included evidence supporting the simulated ice-cloud relationships using direct observations and reanalysis products. Many such studies exist in the literature and could be used to assess the linkages between ice cover and clouds described in this paper. Some of this work was cited in the Introduction, but it would be helpful for direct comparisons in the Results section or Discussion. A couple of relevant studies include Palm et al. (2010, *J. Geophysical Research*), which used lidar to detect an inverse relationship between Arctic sea ice and cloud cover, and Vavrus and Cuzzone (2011), who analyzed the ice-cloud relationship using ERA-Interim Reanalysis and CCSM3 model output.

3. Although the topic of this study is certainly important and timely, the paper doesn't lay out what is special about this particular investigation. For example, on page 17531 the

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statement is made that this study investigates the (simulated) temporal trends in Arctic clouds and how they relate to sea ice, but the Introduction has just nicely summarized many other such studies. What is special about this study that advances our knowledge beyond what already appears in the literature?

4. Throughout the manuscript the text refers to various results as “significant”, but it’s not clear whether this term is being used in the statistical sense or more informally as being “substantial.” The authors should clarify this point and explain what type of statistical test they applied if the results were indeed statistically significant.

5. Although there appears to be a significant relationship between trends in sea ice concentration and cloud cover, the most widespread increases in clouds are over perennial sea ice in the Arctic Ocean, where the increases in latent and sensible heat fluxes are minimal (Figures 3 and 4). However, the text doesn’t address this mismatch until the Discussion, where the authors conclude that favorable circulation anomalies are the cause of the enhanced cloudiness in the interior Arctic. That might be the cause, but a simpler alternative explanation is that the stronger atmospheric stability and associated strong temperature inversion could trap even small-moderate amounts of increasing evaporation from modest expansion of open water coverage over the central Arctic Ocean. Without conducting an in-depth analysis, some insight could be gained by comparing the results of each of the five ensemble model simulations. Did every one produce the same kind of SLP change that favors advection of moisture into the central Arctic? Did every one produce the same kind of widespread cloud increase over the central Arctic? If the answer is “no” to the first question but “yes” to the second, then perhaps another explanation besides circulation anomalies accounts for the enhanced cloudiness in the interior of the basin.

6. I find the text describing Figure 6 to be confusing (pages 17537-17538), especially the parts about the Δa_i^+ and Δa_i^- curves. Can the graphs in Figure 5e,h be used instead to convey the same message?

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7. The description of the CRE and associated ratio could use some clarifying. Please give a physical interpretation of the index of $(\Delta \text{CRE})/(\Delta \text{CS})$. What does it mean for this ratio to have a negative value? In Figure 7, do the larger values of this ratio during winter than autumn imply that cloud changes actually have a radiatively stronger impact during the winter months? Also, do positive values of < 1 imply that the clouds offset the radiative heating that causes the clear-sky downwelling (CS) to increase more than the net-sky ΔCRE term, due to the warmer and moisture atmosphere as ice cover diminishes?

Minor Comments:

1. In the Introduction (p. 17531), an important couple of additional caveats regarding past and present studies of simulated Arctic clouds are (a) climate models have long-standing difficulty in representing polar clouds, and (b) not only is the radiative effect of polar clouds difficult to measure, but even detecting and defining a polar cloud is challenging (e.g., Curry et al. (1996)).
2. Page 17532: In describing the model's resolution, I think the authors mean the "lid" of the model is 3 hPa, rather than the highest resolution of any layer being 3 hPa.
3. Page 17532: A bit more information about the sea ice model would be helpful, such as whether it allows ice motion and, if so, what type of dynamical ice scheme is included (EVP, etc.)
4. Page. 17534: Although the agreement with the sea ice trend from HadISST is evident in Figure 1, how does the magnitude of the sea ice trend in MIROC5 compare with that observed in HadISST?
5. In referring to Figure 2a, for example, the text should make clear whether the figures are showing simulated model results or observed results. This clarification should also be made elsewhere in the paper where necessary (e.g., the reference to Figure 3 on the next page), so readers can immediately tell whether they are looking at model

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output or actual observations.

6. Page 17534: What method is being referred to in the phrase “using this method” in reference to Arctic Ocean cloud cover?

7. Page 17536: It’s true that positive trends in LE and SH occur at grids where sea ice declined substantially (Figure 4), but large increases also appear to happen in the Barents Sea to the south of large sea ice reductions, at least based on the contours of large negative sea ice trends.

8. Page 17536: Please rephrase the statement about how Δa_i^- is defined as trends less than -0.1 per decade, so it’s clear that this means places where the decline is more than 0.1 per decade.

9. Page 17536: Not only does the cloud fraction decrease at levels below $\sigma = 0.95$ where large sea ice declines occur (as noted), but this also happens at the Δa_i^+ points (where ice cover increases). What explanation accounts for both of these responses?

10. Page 17537: Why does relative humidity exceed 1 near the surface in Figure 5d?

11. Page 17537: The term “diffusion” isn’t the best choice of a word, considering that turbulent mixing may also occur within the stable boundary layer.

12. Page 17538: The text should define the term cloud radiative effect (CRE) and/or use the more common term, cloud radiative forcing (CRF). Also, the heading of Figure 7 uses CRF, rather than CRE.

13. Page 17539: Better to move the first paragraph of the Discussion section into the Results section, since that material is still describing results of the simulations.

14. Page 17540: The term “weaker” is better than “lower” in line 5, in order to clarify that this term doesn’t refer to cloud height.

15. Page 17541 (line 8): I’m not sure how to interpret the phrase “is unlikely to change,”

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given that widespread cloud increases do occur in the model over the central Arctic Ocean, where there are not large ice reductions.

16. In the explanation for why cloudiness increases in the lower troposphere except at the surface (Results and page 17541), isn't a simpler explanation that the large warming immediately near the surface causes such a large temperature rise and associated increase in the moisture-holding capacity of the air in this layer that the relative humidity decreases, whereas the temperature rise in overlying layers is less extreme and therefore the relative humidity responds more strongly to the moisture increase?

17. Please specify in the captions of Figures 5 and 6 that October is the month plotted.

18. Figure 8: The gray lines defining the region are not visible in my version. Some other color or thicker line would be clearer.

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