Responses to referees' comments

We thank anonymous referees for their constructive comments and helpful suggestions. We have improved the manuscripts based on the comments from the referees.
Referee's comments are indicated in *Italics*, and then our answer follows immediately.

Responses to the comments from Referee #2.

Major comment
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 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.

A: This comment is the same as one of the comment from Referee #1. Therefore, the response to this comment is the same as the response to the comment from Referee #1.

Based on this comment, we have conducted the instantaneous and lead/lag correlation analysis to infer causality of the interaction between sea ice and cloud cover. From the result, we have confirmed that the cloud cover in October is likely to increase due to decrease in sea ice concentration. This result has been added in subsection 3.2 of the revised manuscript. Related to this addition, we have divided the content in subsection 3.1 of the original manuscript in two subsections. The titles of subsections 3.1 and 3.2 in the revised manuscript were changed to “Simulated change of the Arctic sea ice and clouds” and “Relationship between changes in sea ice and cloud cover during fall”, respectively. The description added in subsection 3.2 of the revised manuscript is as below,

“Figures 4a and 4b show geographical distribution of autocorrelations of simulated sea-ice concentration between September and October and instantaneous correlations of simulated cloud cover and sea-ice concentration in October, respectively. The correlation coefficients were calculated for 1976-2005. In the autocorrelation in sea ice concentration between September and October, large positive correlation coefficients are found over most of the Arctic Ocean, with the larger positive correlation coefficients above 0.6 over the lower latitude regions from the Beaufort to the Barents Sea (Fig. 4a). In the Arctic subregion exhibiting highly autocorrelation of simulated sea-ice concentration (109-221° E, 69-78°N), which is shown in Fig. 4a with black broken lines, autocorrelations of simulated sea ice concentration (blue circle in Fig. 4c) decay with a slower time lag than those of simulated cloud cover (black circle in Fig. 4c), since the autocorrelations of simulated sea ice concentration reflect a substantially longer memory in sea ice. These results suggest that, in MIROC5, sea ice change in October tends to depend on sea ice change in September: small sea ice concentration during September is likely to continue a small sea ice concentration during October.

Negatively stronger correlations between sea-ice concentration and cloud cover in October are found in grids with dominant negative trend in sea-ice concentration during 1976-2005 in MIROC5 (Fig. 4b). These indicate that increased cloud cover in the grids tends to be associated with a smaller sea ice concentration. The negative relationship between sea ice concentration and cloud cover in MIROC5 are consistent with the observed results in Palm et al. (2010) and Liu et al. (2012). Lead/lag correlations in the Arctic subregion show that cloud cover in October is negatively correlated with the lead/lagged sea-ice concentration (green diamond in Fig. 4c). This negative correlation of could cover in October with sea-ice concentration in September suggests that, in October, small sea ice concentration continuing from September makes cloud cover increase, because of the strong autocorrelation of sea ice concentration between September and October in MIROC5. Also, as the autocorrelation of simulated cloud cover between September and October is weaker than the correlation between simulated cloud cover in October and simulated sea ice concentration in September, the increase in cloud cover in

October is unlikely to represent a continuing increase in cloud cover from September in MIROC5. On the other hand, sea ice concentration in October is also negatively correlated with lead/lagged cloud cover (red diamond in Fig. 4c). Correlation of sea ice concentration in October and cloud cover in September is weaker than that of cloud cover in October and sea ice concentration in September. Therefore, we concluded that, in MIROC5, cloud cover is likely to increase due to decrease in sea ice concentration during October. This results supports previous result with the satellite data in Liu et al. (2012) that decrease in sea ice concentration lead to increases in cloud cover.

In MIROC5, while correlation of cloud cover in October and sea ice concentration in November is strong (red diamond in Fig. 4c), autocorrelation of sea ice between October and November remains strong. Thus, change in sea ice concentration in November may be strongly reflected by those in October, rather than impact of cloud cover in October on sea ice concentration in November. Note that, since this correlation analysis uses monthly mean data, correlation between variables in time-scale smaller than one month remains unclear."

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

A: We thank you for this comment and introducing two papers related to this study. We had read the two papers. We had compared our results with results in the two papers and then the following descriptions on the comparison have been added in subsection 3.3 of the revised manuscript.

"Features of the simulated vertical structures of cloud fraction and relative humidity in the latter period for the case Δai- are very similar to those for low sea ice years in ERA-interim in Cuzzone and Vavrus (2011) and those for below-normal ice concentration in ERA-40 in Schweiger et al. (2008) , although values in this study are a little different

from those in these reanalysis data sets. In those reanalysis data sets, compared with cloud fraction and relative humidity in the overlying layers, those in the layers near surface is smaller. Further, our result is consistent with the result with the satellite measurements of Palm et al. (2010), which is the increase in autumn clouds within 500 m of the surface over sea ice rather than open ocean. "
"These changes in simulated vertical structures in air temperature and specific humidity between the earlier and latter periods for the case Δai- correspond with the differences in those between low sea ice years and high sea ice years in ERA-interim in Cuzzone and Vavrus (2011), although the differences in cloud fractions in the layers near surface are much larger in ERA-interim."

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 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?

A: Analyzing data from the historical simulations by a state-of-the-art climate model, MIROC5, is special about this study. There is a controversy on the vertical profile of Arctic cloud change. To assess this topic in the state-of-the-art climate model, we have analyzed the vertical profile of the cloud fraction and related variables. Although there are several results on the vertical profile of cloud changes using the simulations or the observations, we believe that showing result in the specific climate model should be essential to improve the related processes introduced in the climate model and help to understand an observed phenomenon. Further, this study evaluated relative importance of the radiative effect of Arctic cloud change resulting from reduction of sea ice to the radiative effect of warming and moistening due to increased open ocean and the global warming. This evaluation is original in this study.
We have modified the text in Introduction as follows,
"In this study, we investigate the temporal trends of Arctic cloud cover change during the recent global warming simulated by a state-of-the-art climate model, MIROC5, and focus on the effects of reduced sea ice. The simulated vertical structure of the cloud cover change is analyzed using the composite analysis technique, because a controversy on the vertical profile of cloud change remains. Furthermore, this study evaluate relative importance of changes in the cloud radiative forcing in the surface DLR to radiation

changes due to increases in air temperature and water vapor, to provide information on the role of Arctic clouds in the mechanism of the Arctic warming. The change in Arctic cloud resulting from the reduced sea ice in climate model simulations should be informative for understanding the mechanism underlying future changes in Arctic clouds and the Arctic warming."

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.

A: We agree with the comment. In linear trend analysis, we have conducted a statistical test, in which we tested if the linear trends in grids are zero or not. We have included dots which shows statistical significance at the 95% level in the revised Fig. 3. We have changed "significant" to "dominant" or "substantial" in descriptions about results without statistical test.

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.

A: We appreciate this comment. We had checked the changes in the SLP and water vapor convergence in each ensemble member, and also additionally analyzed the changes of the static stability in the lower troposphere with calculating the lapse rate of the equivalent potential temperature. From the result, we made a sure that convergences of water vapers are enhanced in all ensemble members. However, there are two ensemble members in which increase in cloud cover do not appear in the grids without dominant reduction of sea ice. In addition, we found that the weakened stability does not always lead the increase in cloud cover in the grids without dominant reduction of sea ice. Therefore we have modified the discussion of this topic in section 4 and Fig 8 as below,

“As shown in Figure 3b, increases in the simulated cloud cover are found in the Arctic Ocean near the North Pole, where the simulated sea ice does not decrease substantially. We investigated the effect of changes in both the moisture convergence and the static stability in the lower troposphere on the increase in cloud cover. Figures 8a show the simulated linear trend of the sea level pressure (SLP), moisture flux at 925 hPa, and their convergence in October, which are averages for ensemble members. The figure shows the moisture flux converges in the region with the increase in cloud cover. Accordingly, we could conclude that the cloud cover in the region near the North Pole increases in the lower troposphere with the moisture convergence despite the absence of a significant reduction in sea ice. However, analyzing the data in each ensemble member, we found several ensemble members in which increase in the moisture convergences in the region without the dominant reduction in sea ice did not lead increase in cloud cover. The enhanced moisture convergence only may be not sufficient to lead the increase in cloud cover. Further, Figure 8b shows the simulated linear trend of the lapse rate of equivalent potential temperature between surface and σ=0.9, which is also average for ensemble members. The figure shows the static stability in the lower troposphere decreases over the Arctic Ocean, but in the region without dominant reduction of sea ice, the large decrease in the static stability does not always correspond with the large increase in cloud cover. This is common in each ensemble member. Therefore, an appropriate and systematic cause of dominant increase in cloud cover in the region without substantial reduction in sea ice remains unclear. To clear this, we may needs more ensemble members of the experiment.”

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

A: Related to another comment from Referee #1, we have modified a sampling way for the Δai+ as mentioned above, and then we have revised figures of the simulated vertical profiles. The revised Fig. 6 shows more clearly the differences in the lapse rate of air temperature between the earlier and the latter periods than that in Fig. 5 in the original manuscript. Thus, figures of the lapse rates of air temperature and specific humidity, which are in Fig. 6 of the original manuscript, have been removed in the revised manuscript.

The revised descriptions in subsection 3.3 are as follows,

"Figures 6e and 6f show that the specific humidity in the lower troposphere increases more dominantly in the case Δai- than that in the case Δai+, although increases in the specific humidity in whole levels shown in the figure are found for both cases (Fig. 6f). Compared with the change in the saturated specific humidity (*qsat*, dot-dot-dash lines in Figures 6e and 6f), in the case Δai-, the increase in the specific humidity is near to that in the *qsat* at levels with increases in the cloud fraction. Therefore, the relative humidity increases and enhances the cloudiness in these levels (Figures 6b and 6d). However, increases in the specific humidity are smaller than those in the *qsat* at thin layers near the surface. The large increase in the *qsat* at the thin layers is attributable to large increase in air temperature in the case Δai-. Figures 6g and 6h show that the air temperature increases with the maximum increase at the surface. The substantial increases in air temperature in the case Δai- are found in layers between the surface and σ=0.85 (approximately 1200 m) (Figure 6f). Therefore, in the surface thin layers, the relative humidity decreases and reduces the cloudiness. These changes in simulated vertical structures in air temperature and specific humidity between the earlier and latter periods for the case Δai- correspond with the differences in those between low sea ice years and high sea ice years in ERA-interim in Cuzzone and Vavrus (2011), although the differences in cloud fractions in the layers near surface are much larger in ERA-interim.

The lapse rate of the specific humidity in the case Δai- becomes large at overall lower tropospheric levels in the latter period, compared with that in the case Δai+ (Fig. 6e). We interpret that this increase in the lapse rate of the specific humidity in MIROC5 reflects an increase in water vapor from open ocean and an enhancement of vertical water cycle including processes of convection, cloud and precipitation, because simulated cloud fractions and precipitation increase in addition to evaporation from the open ocean. Much water vapor from the open ocean can be vertically transported to higher levels by vertical mixing and convection, and then the transported water vapor can be removed from the atmosphere through the phase change in processes of cloud and precipitation at levels at which cloud fraction increases. As a result, the lapse rate of the specific humidity may not decrease at any lower tropospheric levels. Thus these process can transported the water vapor increased near open ocean effectively to the higher levels, contributing to increase in cloud and precipitation. On the other hand, in the case Δai+, as the lapse rate of the specific humidity is small in the layers near the surface, vertical diffusion (turbulent mixing) of water vapor is quite weak. However, in this interpretation of the process of enhanced vertical water cycle, an effect of the horizontal advection of water vapor is not considered, because the horizontal effect was obscured by averaging the data for grids and ensemble members in the composite analysis.

The lapse rate of the simulated air temperature is extremely large in the thin layers close to surface in the case Δai- (Fig. 6g). Increase in sensible heat and longwave radiation from ocean to the atmosphere lead dominant increase in air temperature, because SST in the open ocean is near zero and much higher than air temperature. Further, the radiative cooling in all atmospheric levels can contribute to smaller lapse rate in the air temperature at the layers except the surface thin layer. Thus, the strong heat diffusion (turbulent mixing) in MIROC5 is confined in the surface thin layers."

The description of the CRE and associated ratio could use some clarifying. Please give a physical interpretation of the index of (delta CRE)/(delta 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 delta CRE term, due to the warmer and moisture atmosphere as ice cover diminishes?

A: We have considered the impact of changes in both water vapor and air temperature on the surface DLR using the clear-sky surface DLR in our analysis. However, it is very tough work for us to distinguish the impact of water vapor from the impact of air temperature, due to limited data. In the original manuscript, we had not included the change in each component such as the CRE of surface DLR and the clear-sky surface DLR in Fig. 7 and the detailed description of the each component in the original manuscript. This might complicate the understanding of the results and the discussions. In the revised manuscript, we have included the change in each component in the revised Fig. 7 and explained these in the content.

The larger indexes in winter than autumn do not mean that cloud changes have a stronger impact during the winter month. The index is the ratio of the change in the CRE of the surface DLR to the change in the clear-sky surface DLR, meaning only relative importance of the change in CRE to the change in clear-sky surface DLR which is considered as the change due to the changes in air temperature and moisture.

The negative indexes mean the CRE change is negative because of the positive change in the clear-sky DLR. The negative indexes are found during summer. However, difference in the index between the cases with and without dominant reduction of sea ice are not distinct since large uncertainties are included.

The added and revised descriptions are as follows,

"Figure 7a shows the change in CRF of surface DLR (ΔCRF_{SDLR}) and clear-sky surface DLR (ΔCS_{SDLR}) between the former and the latter periods. The changes in the figure were averaged for the $\Delta ai-$ grids and $\Delta ai+$ grids in each month. Positive ΔCS_{SDLR} are ascendant in the both cases. Particularly, during autumn-winter-spring, positive ΔCS_{SDLR} is dominant in the case $\Delta ai-$, rather than that in the case $\Delta ai+$. This positive ΔCS_{SDLR} resulted from both warming and moistening due to the increased open ocean and the global warming. Thus, this indicates the positive ΔCS_{SDLR} due to increase in both water vapor and air temperature can affect largely the surface energy balance in the grids with dominant reduction of sea ice concentration.

Positive large ΔCRF_{SDLR} in the case $\Delta ai-$ are found during September-April, whereas that in the case $\Delta ai+$ is small. This result suggests the increase in the CRF of surface DLR is not negligible and potentially contribute to increasing the radiation energy into the surface at the grids with dominant reduction of sea ice concentration. However, compared with positively large ΔCS_{SDLR}, ΔCRF_{SDLR} is smaller.

During summer, positively large ΔCS_{SDLR} and small ΔCRF_{SDLR} are found in the both cases, but the differences between the cases are quite small. This means that, in MIROC5, reduction of sea ice is unlikely to enhance difference in variation of surface DLR during summer.

Here, we introduce an index defined by the ratio ($(\Delta CRF/\Delta CS)_{SDLR}$) between ΔCRF_{SDLR} and ΔCS_{SDLR}. Using this index, we evaluate relative importance of the change in CRF of surface DLR to the dominant change in clear-sky surface DLR. Figure 7b shows the annual time series of the index, $(\Delta CRF/\Delta CS)_{SDLR}$. The ΔCRF_{SDLR} is positive in grids in which the sea ice is reduced because the cloud cover increases as a result of reduced sea ice during fall-winter-spring, but that is negative during summer (Fig. 7a). Additionally, the ΔCS_{SDLR} is positive over the entire Arctic Ocean because of the increased air temperature and moisture (Fig. 7a). The indexes in Figure 7b are approximately 0.4-0.7 during the fall, winter and early spring, varying among the seasons. Although the indexes during winter are larger than that in the fall, the uncertainties shown with error-bar are quite large during winter. Further, the uncertainties in the indexes during spring are also large. The greater uncertainties are due to the small sample numbers of Δai- grids during winter and spring. Thus, it is hard to obtain a substantial result on the indexes during winter-spring. Further, as described above, during summer, no substantial differences between the cases in the clear-sky surface DLR and the CRF of the surface DLR are found. This is common in the indexes during summer, showing no substantial differences in the indexes between the cases Δai- and Δai+ during summer (Fig. 7b).

In contrast, the uncertainties in the indexes during October-December are small in both the cases Δai- and Δai+. An increase in the cloud cover as a result of reduced sea ice enhances the surface DLR. The indexes during October-December means the all-sky surface DLR in the case Δai- increases by approximately 40-60% compared to the clear-sky surface DLR change. The indexes in the Δai- grids are larger than that in the Δai+ grids, although, in November, the index in the Δai- is not distinguished clearly from the index in the Δai+ grids. Thus, considering the reduction of sea ice area in October, the change in the CRF because of the reduced sea ice cannot be disregarded as a factor affecting the Arctic warming. This finding disagrees with Rinke et al. (2013). However, the index shown in Figure 7b is different from the averaged value over the Arctic Ocean. The averaged value is smaller in the winter and early spring because the area with a significant sea ice reduction is small during these seasons. "

Minor comment

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 longstanding 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)).

A: Base on this comment, we have included the text in the introduction as follows,

“However, a debate remains surrounding the change in Arctic cloudiness and the lack of understanding of the effect of the reduced sea ice on Arctic cloud cover because of insufficient observed data and longstanding difficulty in representing realistic polar clouds with the climate model.”

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.

A: We are very sorry that this is an error.
The correct is "with the top at 3 hPa".

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.)

A: We have added the following explanation on the sea ice model.

"In the sea ice model, thermodynamic variables such as sea ice concentration and thickness for each category are advected by the sea ice horizontal velocity with conserving the volume, which is common for all the categories in a grid."

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?

A: The sea ice trend in MIROC5 was calculated with the ensemble mean data. The description in the original manuscript had not included this point. We have added this point in the revised manuscript.

"This simulated negative trend in the Arctic sea ice area averaged for ensemble members is consistent with that from the Hadley Center Sea Ice and Sea Surface Temperature data set (HadISST)…"

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 output or actual observations.

A: Based on the comment, we have revised many sentences related to this point in the revised manuscript. The followings are examples in subsection 3.1, in which the additional texts are underlined.

"Figure 2a shows the seasonal cycle of the Arctic sea ice area averaged for 1976-1985 (blue line) and 1991-2005 (red line), and Figure 2b displays the differences in the seasonal cycle. The MIROC 5 shows that the maximum sea ice area occurs in March, and that the sea ice then decreases to reach a minimum in August. This seasonal cycle of sea ice area in the MIROC5 is slightly different from the observed seasonal cycle (Komuro et al., 2012). According to the observations, the seasonal minimum sea ice area occurs in September, and generally, the Arctic sea ice cover starts to recover in October. Although such discrepancies are found, the basic features of the seasonal cycle of the Arctic sea ice such as the summer reduction and autumn recovery in sea ice are simulated in the MIROC5. During recent global warming, the simulated Arctic sea ice decreased in all months from 1976 to 2005, displaying a maximum reduction in September. The simulated maximum reduction in the Arctic sea ice area in September is consistent with the observations of the Arctic sea ice (Comiso et al., 2008).

For the cloud cover over the Arctic Ocean in the MIROC5, Figure 2c and 2d are identical to Figure 2a and 2b except for the total and low-level cloud cover, respectively. The Arctic Ocean in the MIROC5 is covered by low-level clouds during the summer. From summer to autumn, the simulated cloud cover over the Arctic Ocean decreases, reaching a minimum during April. The simulated seasonal cycle of the total cloud cover is similar to that of the low-level clouds. Therefore, in the MIROC5, the seasonal cycle of the total cloud cover is able to be explained by that of the low-level clouds. When compared with the seasonal cycle of cloud cover observed by TOVS satellite and surface-based cloud climatology reported by Schweiger et al. (1999) and Hahn et al. (1995), the seasonal cycle of the total cloud cover averaged over the Arctic Ocean is realistically simulated using this model. As shown in Figure 2d, in the MIROC5, the Arctic cloud cover for autumn-winter-spring increases during (1976-85) – (1996-2005) but not substantially. The increase in simulated cloud cover is the largest in October. The increase in the total cloud cover is also explained by the increase in low-level clouds in the MIROC5. This result agrees with previous studies using satellite data and climate model simulations (Liu et al., 2012; Vavrus et al., 2011; Wu and Lee, 2012). Compared to the low-level cloud cover, the middle- and high-level cloud covers are small in the MIROC5, and their changes during (1976-85) – (1996-2005) are approximately zero (not shown)."

Page 17534: What method is being referred to in the phrase "using this method" in *reference to Arctic Ocean cloud cover?*

A: We are very sorry that this is an error.
The correct is “using this model”.

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.

A: We have analyzed the difference in simulated vertical profile of cloud fraction, air temperature, and specific humidity between regions. We also have analyzed the differences between ensemble members. Based on this analysis, we have included the following discussion in the content. (We apologize that the trends in LE, SH, and sea ice shown in the original Fig.4 were miscalculated due to a program error. So, Figure 4 was corrected.)

“In the Barents Sea and near the Greenland, however, significant positive trend in the simulated cloud cover are not found, despite of the dominant reduction of sea ice. Dynamical impact on atmosphere from the lower latitude region is strong in the Barents Sea and near the Greenland, as major atmospheric flows from the lower latitude are found during the fall, in MIROC5. Thus the dynamical impact may weaken the thermo-dynamic effect resulting from the increased open ocean in some ensemble members.”

Page 17536: Please rephrase the statement about how delta ai– 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.

A: We have added the following sentence after the definition of “Δai-“.

“As shown in Fig. 3b, many of the Δai- grids are located mainly over the wider region including the Laptev Sea, the East Siberian Sea and the Beaufort Sea.”

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 delta ai+ points (where ice cover increases). What explanation accounts for both of these responses?

A: In the original manuscript, the decrease in cloud fraction at levels below sigma=0.95 for the case Δai+ may be related to the same mechanism as that for the case Δai-. Although there are relative large increases in specific humidity near the surface, the relative humidity near the surface decreases. Thus, stronger warming should occur near the surface even in the case for Δai+. However, this warming cannot be attributable to increases of heat flux from the surface, because trend of sea ice is positive. Possible mechanisms are speculated to be the heat advection and the radiative heating due to increase in air temperature and water vapor, but further analysis focusing on this effect would be needed to resolve this mechanism.
In the revision process, however, we modified the sampling way for the case Δai+, and then the result of vertical profiles were changed a little. The modified vertical profile of cloud fraction does not show the decrease in cloud fraction near the surface for the case Δai+. Then this point has not been included in the revised manuscript.

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

A: We found a program error to calculate the relative humidity. Because original data of the relative humidity have not stored, in post-processing, we had calculated the relative humidity using monthly data of air temperature and specific humidity in all the ensemble members. The program error has been debugged and then the relative humidity has been calculated again with the corrected program. The correct data are used in the revised Figs. 6c and 6d.

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.

A: We have added phrase "turbulent mixing" in parentheses after term "diffusion".

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.

A: We have changed the term "the cloud radiative effect (CRE)" to the term "the cloud radiative forcing (CRF)", based on the comment.

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.

A: As responded to another comment, we have analyzed additionally to check the common cause of the increase in cloud cover at grids without dominant reduction of sea ice, which are located near the North Pole, in each ensemble members. However, we could not detect the common cause in ensemble members and grids. We decided that the topic in the first paragraph of the Discussion section remains a discussion.

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.

A: Based on this comment, we changed the term "weaker" to "lower".

Page 17541 (line 8): I'm not sure how to interpret the phrase "is unlikely to change," given that widespread cloud increases do occur in the model over the central Arctic Ocean, where there are not large ice reductions.

A: The criticism is correct. The sentence including the phrase "is unlikely to change" was removed from the description.

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?

A: We appreciate this comment. Base on this comment, we have modified the descriptions related to the topic in the revised manuscript. The revised descriptions in subsection 3.4 are as follows,

"Figures 6e and 6f show that the specific humidity in the lower troposphere increases more dominantly in the case Δai- than that in the case Δai+, although increases in the specific humidity in whole levels shown in the figure are found for both cases (Fig. 6f). Compared with the change in the saturated specific humidity (*qsat*, dot-dot-dash lines in

Figures 6e and 6f), in the case Δai-, the increase in the specific humidity is near to that in the *qsat* at levels with increases in the cloud fraction. Therefore, the relative humidity increases and enhances the cloudiness in these levels (Figures 6b and 6d). However, increases in the specific humidity are smaller than those in the *qsat* at thin layers near the surface. The large increase in the *qsat* at the thin layers is attributable to large increase in air temperature in the case Δai-. Figures 6g and 6h show that the air temperature increases with the maximum increase at the surface. The substantial increases in air temperature in the case Δai- are found in layers between the surface and σ=0.85 (approximately 1200 m) (Figure 6f). Therefore, in the surface thin layers, the relative humidity decreases and reduces the cloudiness."

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

A: We have modified the figure captions in Figure 6 of the revised manuscript. Figures on the lapse rate have been removed in the revised manuscript.

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

A: In this revision, the gray lines have been removed, because the lines became unneeded.