TCD

Interactive
comment

Interactive comment on "Potential faster Arctic sea ice retreat triggered by snowflakes' greenhouse effect" *by* Jui-Lin Frank Li et al.

Anonymous Referee #1

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General comments:

The paper addresses a relevant topic, which is worth to be published in TC. The overall presentation of the paper is well structured. The language is fluent, but sometimes too colloquial and often not precise enough for my taste. To ensure that the results are reproducible, the methods should be extended. As an example, trends and uncertainties are calculated, but it is often not (or not clearly) written how these are calculated. This makes it difficult to judge whether the statistics are correct. Another aspect that should be improved is testing some of the hypotheses mentioned in the text. I think that this should be easy using the model output of the CESM1-CAM5 simulations (e.g. how the sea ice thickness or how the snow fall changes). Furthermore, more references to the figures would help the reader, it is sometimes not obvious to which figure the text refers

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Discussion paper

to. Some subfigures are shown, but not discussed.

Specific comments:

- Title: I like the title, but in nearly the whole manuscript, you use the terms "falling ice radiative effects" or "snow radiative effects"; why do you use "snowflakes' greenhouse effect" in the title instead?

- page 1, line 18, "natural factors may have amplified this": Which natural factors, and how can they have amplified the recent Arctic sea ice retreat? Do you mean interannual variability? Instead of "this", I would write "the observed retreat in the last years".

- Page 1, line 23, "(extent $< 1 \times 10^6$ km^2)": Please write to what this number refers to. The minimum extent of the year? The extent averaged over some time (September)?

- Page 2, line 23, "Natural atmospheric & ocean dynamics may also contribute...":
  - I would cross the word "natural"
  - I would replace "&" by "and" (in the whole text) if there is no good reason to use "&"
  - please explicitly mention to what the dynamics contribute

- page 2, line 24, "tends to increase extent in winter but ultimately reduce it in summer":
  - "reduce": "reduces"
  - Why does this increase the extent in winter? Because it distributes the sea ice and thus increases the area with a sea ice concentration larger than 15%? Please add at least a reference.

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- page 2, line 25, "observations have been used to infer contributions due to anomalously high ice export through...": "observations have been used to infer contributions to summer sea ice reduction from anomalously high ice export through..."?

- page 3, lines 2-3: "the observed extreme low events and general retreating trend have been attributed to a combination of melt driven by global warming along with a likely natural component":

  – Kay et al. (2011) focus on one extreme event, so I would add at least one more reference.

  – I would specify what you mean with natural component. Without context, it could be anything, also a forcing such as volcanic aerosols. You could rephrase the sentence as: "the observed extreme low events and the general retreating trend in summer sea ice extent have been attributed to melt driven by global warming, along with an increased importance of internal variability when sea ice thickness is reduced." (If this is what you mean.)

- page 3, lines 6-9: You directly jump from the attribution to the importance of projections. I would insert the following sentence after "to each forcing.": "A better understanding of the processes that are mainly responsible for sea ice retreat will help to reduce uncertainties in future projections."

- Page 3, lines 13-14, "under high emissions": do you mean here high GHG emissions or a strong forcing? (because anthropogenic aerosol emissions are decreasing in RCP8.5)

- page 3, lines 17-18, "Summer retreat has been faster than the average CMIP5 model simulation, implying a large naturally forced component to recent extremes.":

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- I would write "Observed summer retreat".

- I would delete "implying a large naturally forced component to recent extremes" (and the "However" at the beginning of the next sentence). The term "large naturally forced component" is not very meaningful in my opinion. Furthermore, studies imply that internal variability has contributed to the recent extremes, not the fact that the observations show a larger retreat than the models (the models could be wrong due to other issues). In fact, if the models were correct, they would in general be able to simulate that the year-to-year variations in circulation and clouds have a higher impact on sea ice extent when the sea ice thickness is reduced.

- Page 3, line 19: I would replace "forced response" by "sea ice retreat"

- page 3, line 25, "a decrease in surface shortwave which will": "a decrease in downward shortwave radiation, which will" (if this is what you mean)

- page 4, line 1, "a somewhat different expression": "a somewhat different response"?

- Page 4, lines 6-7:

  - "This should manifest later as a faster retreat, both ..." → "This should manifest later as a faster retreat of sea ice area/extent, both...";

  - you could cite here the paper by Massonnet et al. (2018) (https://doi.org/10.1038/s41558-018-0204-z)

- page 4, line 9, "there will be no offset for the stronger expected downward shortwave": "there will be no offset for the weaker expected downward shortwave radiation in summer" (?)

- page 4, line 11, "These effects...":

- Which effects? Summer versus winter? Reduced downward SW versus lower albedo?
  - I would cross the "necessarily"
  - I would write "whether one factor will dominate" instead of "should"

- page 4, line 15, "raise the melting layer": "raise the atmospheric melting layer"

- page 4, line 15-16, "leading to a reduction in the total ice water path (TIWP) in favour of liquid water, which has a smaller radiative effect.": does the "which" refer to "liquid water" or to the "reduction in the total ice water path"?

- page 4 line 23, "We ignore coupled dynamic responses in favour of ...": When I first read this, it sounded to me as if you switched off coupled dynamic responses in your model. After having read the whole paper, I realised that you just wanted to say that you did not analyse potential changes in e.g. ocean heat transport. I would rephrase this sentence.

- page 5, line 4, "for each of the...": this could be misinterpreted, i.e. that you use all ensemble members. I would just cross the "each of"

- page 5, lines 14-15:

  - "close to 1 degree x 1 degree" → "close to a 1 degree x 1 degree"?
  - Please say a few more word about these simulations by Li et al. (2014). Do they follow some protocol?

- Page 5, lines 16-17, "and it does this thanks to a two-moment cloud scheme with diagnostic snow":

  - Our model also has a two-moment cloud scheme and diagnostic snow, but cannot calculate FIRE. I think the important feature of the scheme by Gettelman et al. (2010) is that it treats both the number concentration and the

C5

mixing ratio of snow and rain (instead of only the mass). I would there-
fore rephrase the sentence to: "and it does this thanks to a diagnostic two-
moment treatment of rain and snow"

- Since the whole paper is about FIRE, a few words about how it is calculated
  would be beneficial

- "This only represents" → "The scheme only represents"

- page 5, line 19:

  - "allows... to be allowed or disallowed": please rephrase

  - please mention somewhere in the text explicitly that the only difference be-
    tween the simulations CESM2-SoN and CESM1-NoS is switching on/off
    FIRE (for both the historical and the 1pctCO2 simulations)

- page 5, lines 21-22:

  - "to estimate the first response" → what do you mean here by first response?

  - This sounds as if the output were a simulation. You could write: "we use
    output of the 1pctCO2 simulation, in which atmospheric CO_2 increases at
    1% yr -1 for 140 years."

  - Please say a bit more about this simulation. Is it a simulation with CMIP5
    input/boundary conditions? With what CO_2 concentration (corresponding to
    which year) does it start? Is this simulation also described in Li et al. (2014)?

- Page 5, line 22, "Radiative forcing definitions differ...": I think you don't mean that
  the definitions of the radiative forcing differ (which is also an important question,
  e.g. allowing for adjustments or not) but rather that the radiative forcings them-
  selves differ?

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- Page 5, line 24/25, "We use output for fully coupled CESM1-SoN and for CESM1-NoS runs following the historical and 1pctCO2 simulations.": "We use output from fully coupled CESM1-SoN and for CESM1-NoS runs following the historical and 1pctCO2 scenarios."

- Page 6, line 10-13:

  – Which data did you use for the calculations? The CMIP5 data on the original grid or the data interpolated to a 2.5 degree x 2.5 degree grid?

  – Did you consider the land-sea mask for your calculations (as you did in Section 2.3)? I think that the sea ice concentration from CMIP5 only refers to the oceanic part of the gridbox (at least on the native grids).

- Page 6, lines 19-21:

  – "This combines..." → "CERES-EBAF Surface combines..."

  – "to estimate surface fluxes" → "to calculate surface fluxes"

  – "in each term" → what do you mean with "in each term"? Of each calculated surface flux?

- Page 6, line 22, "previously gridded": "previously interpolated"?

- Page 6, line 23-25:

  – "Fluxes are calculated by taking the area-weighted average of values in each grid cell after scaling by the ocean fraction" → "Fluxes are calculated by taking the area-weighted average after scaling each gridcell by the ocean fraction (including sea ice)"?

  – "we use the CESM1-CAM5 grid" → "we use the CESM1-CAM5 land sea mask"?

- – "a consistent map" → " a consistent fractional land sea mask"?

- Page 6, line 27: "our controlled" → "our historical"?

- page 7, lines 2-5:

  - – I am not sure whether I understand what you did. Did you slice the model output in slices 1979-1982, 1983-1986, 1987-1990, . . . and calculated the standard deviation for each of these slices and then averaged all the standard deviations? And why did you quadrate these values? Maybe a formula or a sketch might be helpful.

  - – The standard deviation of the fluxes might have changed over time, e.g. as a consequence of the sea ice retreat. In my opinion, you could thus just show the standard deviation over the four years of overlap that you have (even if it is large).

- Figures in general: I think it would help the reader if the figures have sublabels (a), (b), etc. that you can refer to.

- Page 7, line 7: "post-1979 changes in SIE": this could be misinterpreted since Figure 1 does not show the changes, but the absolute values in contrast to Supplementary Figs. 3-4

- Page 7, line 11: I would mention the difference between Supplementary Figures 3 and 4.

- page 7, l.12, "The bottom panels of this figure show...generally agrees better with the faster observed retreat": Please mention which figure you mean. I don't see this in Figure 1 (and also not in Supplementary Fig. 2). In March, NoS actually compares better with the observations, and the trend looks similar between NoS and SoN (Fig. 1). In September, NoS is closer to the observations at the beginning, and SoN is closer to them at the end of the observed period. It is hard

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to see in Fig. 1 whether the trend in NoS and SoN is different in September. In Supplementary Figs. 3 and 4, it looks like the trend in September is somewhat stronger for SoN. Why don't you calculate the trends for the observations and the CMIP5 medians and compare them? Next to linear regression (which is not very robust), you could also use the Theil-Sen Trend Estimate together with the Mann-Kendall trend test.

- page 7, l.15, "differences in parameterisations for clouds, the atmosphere, oceans...":

    – clouds are a component of the atmosphere, I would not distinguish between the two.

    – Not only parameterisations, but also differences in calculations matter.

- page 7, l. 16:

    – sometimes you write CESM1-CAM5, sometimes only CESM1

    – "controlled" → "historical"?

- Page 7, line 17, "CESM1-CAM5 captures the mean extent well with a smaller discrepancy versus observations throughout the year when including FIRE (full annual cycles in Supplementary Figures 5âĂŤ6).": You should mention somewhere in the text that the trend in SoN in September is not better than NoS when we compare to the observations since the first is too strong (shown in Supplementary Figure 6). You show in Supplementary Fig. 6 also the observed trend for 1979-2017 so that one could think that the SoN trend in September compares well with the observations. In my opinion, you cannot compare observations by 2017 with simulations by 2005, since it was much warmer between 2005-2017 than before. I would delete this line from the figure (and the text where you mention the trend from 1979 to 2017).

C9

- Page 7, line 18, "full annual cycles...": mention that Supp. Fig. 6 shows trends

- page 7, lines 19-21:

  – how did you calculate the trend and how did you calculate whether the trends differ (you can also write that in the methods)?

  – you could use recursive pre-whitening to account for serial correlation (Wang & Swail 2001, Changes of Extreme Wave Heights in Northern Hemisphere Oceans and Related Atmospheric Circulation Regimes; Zhang & Zwiers 2004, Comment on "Applicability of prewhitening to eliminate the influence of serial correlation on the MannâĂŘKendall test")

  – I think it is sufficient to provide the p-value, t gives no real information (?)

- page 7, line 21:

  – "Neither show significant differences relative..." → "Neither are differences significant relative..."?

- page 7, line 23, "the bottom panels show": of which figure?

- page 8, line 3, "majority of years...": It would be helpful to add a dashed line in Fig.3b at the year when the majority of years (i.e. 6 years) are ice-free (and down to the corresponding CO_2 values)

- page 8, line 4, "In an naïve sense this implies...": I thought that the relationship between cumulative CO_2 emissions and the CO_2 concentration in the atmosphere is not linear. Or is the approximation of a linear function valid for the time scales that you are looking at?

- page 8, line 8, "a more rapid collapse of Arctic sea ice in reality": more rapid than what? Than previously simulated by CMIP5 models?

- page 9, line 1, "Absorbed longwave dominates": absorbed the by surface? And dominates over what? Absorbed shortwave radiation (where is this shown)?

- Page 9, lines 1-2, "CESM1-SoN's lower SIE results in a lower albedo that more than offsets the reduced SW downward such that absorbed SW is also higher when including FIRE.":

  - "CESM1-SoN's lower SIE results in a lower albedo that more than offsets the reduced SW downward such that SW absorbed at the surface is also higher when including FIRE."
  - This explains why the difference in SW between SoN and NoS in Fig. 4b is not large, correct? If yes, I would explictly refer to this subfigure.

- Page 9, line 3-5:

  - "on average": yearly average?
  - I think that changes in the net radiation matter more than the downward longwave radiation (?).
  - Please also discuss Fig. 4c. It shows that the difference in the net downward radiation sum between the model and the observation is smaller for many months, but larger in September with SoN. Please also think about how to use the word "net"; for Fig. 4b, you use "net" as downward+upward; for Fig. 4c, you use "net" as LW+SW.
  - Figure 4c shows the sum of LW and SW shown in 4a if I understand the caption correctly. However, if I simply add the values in a, I don't get the same values as in 4c. Did I misinterpret the figure?

- Page 9, line 7, "This would manifest as...":

  - please mention here that you now switch to the 1pctCO2 simulations

– "differences in time" → "differences over time"?

- page 9, lines 10-11:

  – "changes are estimated" → "trends are estimated" (to be more precise because you sometimes also look at changes between two simulations or changes between observations and simulations)

  – please mention how you calculated the trend

  – "changes occur" → "trend occurs"

- page 9, line 13: what is the plus/minus refering to?

- page 9, line 14, "so this change": "so the following change in trend"

- page 9, line 15, "by year 70": refer to the figure

- page 9, line 18: why do you use a range of 14-86% here? in other occasions you showed 10-90% percentiles or 2*sigma

- page 9, line 19-21:

  – Could the following maybe also be an explanation: when there is sea ice in NoS, but no sea ice left in SoN, I expect that the cloud radiative effect in SoN is larger because there is more evaporation from the ocean's surface. When later both NoS and SoN are ice-free, the cloud radiative effect (and the downward LW radiations) would be more similar.

  – Can you diagnose the transition from snow to rain from the model output to confirm your hypothesis?

  – Are the radiative properties of rain also considered in your model or are these totally negligible?

C12

- page 9, line 25: does your simulated output confirm that the sea ice thickness becomes thinner?

- Page 10, lines 13-14, "two models that include FIRE show substantially more summertime SW...":

  - more than what (CMIP5 median)?
  - Can you show this somewhere or provide some numbers?

- page 11, line 1, "too much surface shortwave radiation": "too much downward shortwave radiation"?

- page 11, lines 17-20: Can you calculate from your model output how much sea ice has melted in your simulations (in SoN and NoS)?

- Page 11, lines 21-23: Why did you actually not look at least at some other variables? As an example, it should be easy to see how different the clouds and precipitation are between the two simulations (e.g. liquid water path, cloud cover, snow versus rain).

- page 11, line 26, "lead to counteracting processes": do you mean: "may disperse the snow radiative effect"?

- Page 12, line 1, being approximately twice as fast: Do you show that somewhere in the paper? How many years from now on for the two cases?

- Figures in general: Sometimes you use parentheses and sometimes square brackets around the units.

- Figure 1, caption, 10-90% range: I would write "10-90% percentile range" (in the whole text) to be more precise

- Figure 2, caption: "and" before CESM1-CAM5

C13

- Figure 3, caption: please delete "but any comparison must be carefully made ...". In my opinion, statements like this do not belong to a caption but only to the main text.

- Figure 5:

- caption: mention that this figure shows 1pctCO2

- The units should be W/m^2.

- Figure 6, caption: delete "poleward of 30 degree" since you show output between 60 and 90 degree N

- Supplementary Material, Table 1:

- "whether they exclude falling ice radiative effects": this sounds as if the models have FIRE implemented but exclude them; how about "neglect falling ice radiative effects"?

- "this subset is all those for whom": please rewrite, e.g. "All r1i1p1 simulations were considered that provide the scenarios of interest and the necessary output of surface fluxes and sea ice fields."

- Supplementary Figure 1:

    – to what do the colour of the points correspond to (seasons)?
    – If there are more than 8 simulations that you compared, you could add in the caption that the other plots look similar (if this is the case)

- Supplementary Figure 3, caption: first you write that the anomaly is relative to 1979-1984, then you write that you calculated the anomalies relative to 1979 (?)

- Supplementary Figure 4, "SIE change is shown as a fraction relative to its 1979-1984 mean": I would rather write that Supplementary Figure 4 shows relative changes (instead of absolute changes).

- Supplementary Figure 5:

- "No uncertainties are shown...": You could detrend the time series before you calculate the standard deviation.

- Supplementary Figure 6, "and may be an underestimate...":

  - I would not write that in the caption but discuss it in the text.
  - Do the lag-1 correlations that you mention refer to individual months? If yes, could you calculate the trend considering the lag-1 correlation for each month individually? Does it make a large difference if you account for autocorrelation? How does it change if you take another trend estimator than linear regression?
  - Please mention how you calculate the sigma. Is this the standard deviation of the white noise? Or is it the uncertainty of the trend (which would be more important from my point of view)?
  - The error bars overlap for many months and therefore it is impossible to see the standard deviations.

Technical corrections:

- Page 1, line 24, "downward shortwave": I would (always in the paper) write "downward shortwave radiation" (the same of course for longwave)

- Page 2, lines 10-11, "Physically, ice affects both...": Physically, sea ice affects both..."

- Page 2, line 13, "From a surface perspective": the previous sentence also refers to the surface

- Page 2, line 14, "sea-ice extent": "sea ice extent"

- page 3, line 1, "From analyses of subsets of climate models in the Climate Model Intercomparison Project, phase 5 (CMIP5 (Taylor et al., 2012)), ...": This sentence sound complicated. Why not: "Based on CMIP5 data (Climate Model Intercomparison Project, phase 5; Taylor et al., 2012), the observed ..."

- page 3, line 7, "are also necessary": "are necessary"

- page 3, line 22, "tends": "tend"

- Page 4, line 5, "increased winter longwave": "increased winter longwave downward radiation"

- page 4, line 9-10, "This will mean that a non-FIRE simulation should experience more local albedo feedback due to...": "This will mean that a non-FIRE simulation should experience a stronger local snow-albedo feedback due to..."

- page 4, line 16-17, "the direct effect": "the direct consequence"? (because of "radiative effect" in the previous sentence)

- page 5, line 3, "who have": "that provide"

- Page 5, line 7, "This is a scenario of very high radiative forcing which we select...": comma before "which"

- page 5, line 11 (and in general): you use FIRE as a singular but is it not a plural ("falling ice radiative effects")?

- Page 5, line 12, "and those in which there are no snow radiative effects": "and those in which snow radiative effects are not considered"

- page 5, line 13, "These are listed...": "All models are listed..."

- Page 7, line 19: delete "also"

- Page 8, line 1, "decadal mean SIE": "decadal mean September SIE"

- page 8, line 7, "potential magnitude": "potential impact"?

- page 8, line 14, "in future models": "in future model versions"?

- page 9, line 17: "healthy" sounds colloquial to me

- Page 11, line 12, "shows": show

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