

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 to. Some subfigures are shown, but not discussed.

We thank the reviewer for a thorough and thoughtful analysis of our submission. The paper is now longer, but we think that the main points are both clearer and better supported. We are grateful for advice that led to an improved the manuscript. We attach a redlined draft to show changes and provide a point-by-point response below, but first we summarise the major themes of our changes:

- 1) We now state in the intro that our main points are (1) we test whether FIRE affect simulated sea ice retreat enough to be worth highlighting to model developers, and (2) is there evidence in support of our idea that FIRE thin the initial pack and means faster future retreat. We use this to justify our focus on the radiative terms.**
- 2) We cite Massonnet et al. (2018) several times and extend our discussion on how CMIP5 results are sensitive to many different processes.**
- 3) Methodological detail added. New Supplementary Table 2 shows why we didn't reject white-noise OLS. New supplementary figures show that the nonparametric Theil-Sen estimator gives similar results to linear regression. Extra description has been added where requested.**
- 4) CESM1-SoN and NoS thickness output is analysed in a new figure, and it supports our hypothesis.**
- 5) Text has been changed for clarity but we kept some language described as “colloquial” in cases where we believe that there is no loss of scientific value and the language is more concise, flows better or both.**
- 6) We cite CESM1 large ensemble results to justify why we think our findings are not just due to internal variability. This was not requested by reviewers but we think it's important.**
- 7) A bug has been fixed in the CMIP5 output. This gave invalid values in some models when the scenarios were joined. The main change is to January 2006: the positive January trends in Figure 7(b) are now gone and the January outlier at 2006 in the prior version of Supplementary Figure 3 is also removed.**

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?

We wish to emphasise the longwave component while keeping a shorter title. “Snowflakes’ greenhouse effect” is snappier than “falling snow longwave radiative effects”

- 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”.

Change made.

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

We have changed to “monthly mean extent”. Our intent is that any month may be “ice free”, although we’d expect September to be the first occurrence.

- 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

New text: “Atmosphere and ocean dynamics may also export ice to lower latitudes. For example, stronger circulation associated with the Arctic Oscillation can increase the total area of new, thin ice but transport the thicker ice away from the coldest regions and leave it vulnerable to summer melting (Rigor et al., 2002)”.

“&” replaced throughout.

- 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.

See change above.

- 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...”?

Change made.

- 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.)

The original phrasing was meant to allow the possibility that changes in e.g. clouds and circulation could be natural, or could also be a coupled response to forcing. To avoid a long digression we have rephrased and used Kay et al. as an example of cloud anomalies and Rigor & Wallace 2004 as an example of how circulation may have primed the pack for loss.

- 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.”

Change made, without “mainly”.

- 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)

Good point, we changed to “radiative forcing” to allow for cases of low GHG emissions but with strong carbon cycle feedbacks.

- 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.”:–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.

We removed the suggested text and link the two sentences with “and”.

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

Change made.

- 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)

Change made and now term “SW↓” is introduced here.

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

We feel either could work. Change made.

- 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>)

That is a nice paper, we now refer to this several times and have extended our discussion including its findings.

- 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” (?)

This has been rephrased to focus on the local SW albedo feedback only.

- 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”

Changed to “The SW_↓ and LW_↓ effects from including FIRE should oppose each other and it is not necessarily obvious whether one factor will dominate.”

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

Change made.

- 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”?

This was originally meant to refer to a switch from falling snow to falling rain which would remove modelled TIWP and place it into the rain component, which does not interact with radiation. We have rephrased.

- 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.

This has been rephrased.

- 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”

Done.

- 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?

We now state that these follow CMIP5 protocol for both historical and 1pctCO2.

- 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 mixing ratio of snow and rain (instead of only the mass). I would therefore 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”

Changes made. We have added the following description with more precise references to the relevant papers:

“Falling snow mass and the crystal number concentration is diagnosed at each model level and time step, and is related to an effective radius as detailed in Section 2 of Morrison and Gettelman (2008). The profile of snow mass and effective radius is then related to radiative properties using precomputed lookup tables based on an assumed ice habit mixture as described in Section 2.5 of Gettelman et al. (2010).”

- page 5, line 19:
 - “allows... to be allowed or disallowed”: please rephrase
 - please mention somewhere in the text explicitly that the only difference between the simulations CESM2-SoN and CESM1-NoS is switching on/off FIRE (for both the historical and the 1pctCO2 simulations)

Change made and clarification added to the end of this sentence.

- 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 CO₂ 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 CO₂ concentration (corresponding to which year) does it start? Is this simulation also described in Li et al. (2014)?

Oops, we meant “forced”. This sentence has been changed. The SoN and NoS implementation is described in Li et al. (2014), but these were not used in that study. We think the added description should be enough for readers to identify and understand what we did.

- 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 themselves differ?

We wished to express that radiative forcings definitions differ (e.g. depending on which adjustments are included) and also that calculations for doubled CO2 differ (e.g. fixed SST versus Gregory plot, and if you use Gregory approach then over what period do you regress?). We have replaced “definitions” with “estimates” to better cover these cases.

• 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.”

Change made.

• 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).

We used the Olmon/sic files and areacello for each file, thus accounting for the ocean covered area only. New text: “total area of all of the model’s native ocean grid cells with sic > 15 %”, we think the new text and Kirchmeier-Young reference helps readers to follow this.

• 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?

Changes made, including to “in each surface radiative flux term”.

• Page 6, line 22, “previously gridded”: “previously interpolated”?

Done.

• 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”?

Done.

• Page 6, line 27: “our controlled” → “our historical”?

In the introduction we now specify: “We refer to these as our “controlled” simulations to emphasise that we controlled the inclusion of FIRE and to distinguish them from other studies’ CESM1 simulations.”

- 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 quadrature 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).

There was a typo: we put 4 year averages but meant 5 year average, comparing 2001—2005 inclusive. This has been corrected.

The statistic we are comparing is a 5-year average, so we decided to report our best estimate of the internal variability in that statistic. A new paragraph in Section 2.3 explains our approach.

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

These have been added.

- 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

We now just say “post-1979 SIE”.

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

Text in parentheses rewritten, and these are now supplementary figures 4—5.

- 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 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.

We like this suggestion a lot and have added a new Supplementary Figure 6 showing OLS (stationary Gaussian white noise assumed) and Theil-Sen fits for

1979—2017. Figure 1 discussion now includes: “Trend analysis shows that the median CMIP5-SoN retreat is visibly greater than CMIP5-NoS from June through October, in better agreement with observations (Supplementary Figure 6).”

- 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.

Agreed. Changed to “differences in parameterisations and calculation methods for the atmosphere, oceans and sea ice...”

- page 7, l. 16:
 - sometimes you write CESM1-CAM5, sometimes only CESM1
 - “controlled”→“historical”?

As in previous response, we now specify in the introduction that “controlled” refers to our SoN/NoS runs. We have changed the text throughout to avoid CESM1 alone except when referring to the CMIP5 or large ensemble members. We now refer to CESM1-CAM5, CESM1-SoN or CESM1-NoS, with the implication being that the SoN and NoS cases use CAM5.

- 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 5a: 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)

The supplementary figure has been changed and the line deleted. We now mention the 1979—2005 SoN-obs trend $p = 0.06$ and say that while observed loss rates increased after 2005, we can’t do a direct comparison with the available output.

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

This has been rewritten with separate sentences for mean extent and 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 Mann-Kendall test”)
 - I think it is sufficient to provide the p-value, t gives no real information (?)

We have added a new paragraph to Section 2.2 along with Supplementary Table 2 with statistics calculated from the OLS trend residuals applied to NSIDC SIE. We explain that we calculate each calendar month separately and don't think there is robust evidence of non-white or non-Gaussian noise. For 1979—2005 the derived lag-1 autocorrelations are negative, which would reduce our sigma estimates, and they are not robust since their significance disappears with more data.

We decided against doing the same analysis for all model runs as the level of significance is not a major factor in our primary conclusions. We deleted further mentions of autocorrelation and just mention that our uncertainties assume white noise (and later that the Theil-Sen estimates are similar).

- page 7, line 21:
 - “Neither show significant differences relative...” → “Neither are differences significant relative...”?

This paragraph has been largely rewritten.

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

We now refer to panels, in this case Figure 2(d).

- 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 CO₂ values)

Lines added.

- page 8, line 4, “In an naïve sense this implies...”: I thought that the relationship between cumulative CO₂ emissions and the CO₂ 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?

We intended “naïve” to imply the conclusion following roughly linear assumptions because carbon cycle feedbacks are a massive potential maze. We have added a citation to Matthews et al. whose Figure 2(a) shows pretty constant airborne fraction under 1pctCO2 for years 50—70.

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

Indeed, fixed.

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

Paragraph extended to discuss each panel of Figure 4 in detail, e.g.: “From Figure 4(b), the net absorbed surface SW radiation shows relatively small SoN-NoS differences because while FIRE reduces SW_↓, it also reduces SIE and so lowers the mean albedo. The net absorbed surface longwave radiation is consistently

greater in SoN, explaining the majority of the remaining difference in net radiation in Figure 4(c)."

- 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 explicitly refer to this subfigure.

See above.

- 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?

We did not explain this clearly enough!

The caption has been changed and now ends with "All values are defined such that positive indicates that the model shows greater net downward flux than CERES." We use "netRAD" to be net SW + net LW.

With the new caption and main text discussion which refers to each panel we hope that this is now understandable. Figure 4(a) is downward only, so you need the sum of the Figure 4(b) terms to reconstruct Figure 4(c).

We are uncomfortable with making stronger conclusions from these outputs because of all the coupled responses that could be entangled. FIRE increases downward longwave, which increases surface temperature, which increases upward longwave, thereby reducing the net longwave difference... we can't see how to separate these in a tidy way and so have restricted our discussion somewhat.

- 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"?

Done.

- 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”

Text changes made, and we added “OLS” to the sentence “multiplying the OLS trend gradient”. This acronym for optimised least squares is introduced in our new methods Section 2.2.

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

Text added to clarify.

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

We chose our own rephrasing: “so the full-period LW_↓ trend is not responsible...”

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

Done.

- 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

We don’t reject normality (Kolmogorov-Smirnov again) so I changed to mean ± 2 standard deviations. I considered standard error on the mean but decided that people who care about that can easily work it out by dividing by ~ 6.3 ish, but the standard deviation contains immediate and useful information about the magnitude of interannual variability.

- 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?

This suggestion seems solid. We have touched on this here and in the methods section by discussing how our SoN-NoS flux differences include all coupled changes due to inclusion of FIRE.

We don’t see much added value from exploring the detailed source of the flux differences e.g. by partitioning SoN-NoS differences into cloud optical depth and cloud frequency components. Such a partitioning would be uncertain and distract from our main points. So we have used your suggestion as an example of factors that might matter, and leave the focus on the radiative flux differences.

Text has been added to Section 2.1 to explain that rain is excluded, citing Behrangi et al. (2016) which shows that CloudSat R04 products suggest snowfall dominates precip.

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

We have re-written the introduction including discussion of Massonnet et al. (2018), added a new Methods section 2.3 to explain how we selected regions and calculated thickness in CESM1 and provide a new Figure 4. As hypothesised the SoN simulation has a thinner initial snow pack when we select regions that are well ice covered in both SoN & NoS.

Combined with the Massonnet results this provides more compelling evidence for our suggestions, and we have changed the discussion & conclusion text to mention the mean changes in thickness too.

- 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?

Pointer to Figure 6(d) added, text changed and example value given.

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

Changed to SW↓ as throughout.

- 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)?

Text added based on new Figure 4. Conclusion: ~30 cm difference in mean state for years 1—20.

- 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).

As stated above, we thought that the necessary justification is supplied by the presented changes in fluxes, sea ice extent and thicknesses. The flux differences alone are, we believe, sufficient to achieve the two functions of our paper: (1) test our main proposed hypothesis and (2) determine whether FIRE can play a large role in simulated Arctic sea ice change.

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

We have rephrased. This was meant to highlight how (1) CESM1-CAM5 might have stronger FIRE than other implementations and (2) if other modellers add FIRE, then subsequent tuning of other parameters could counteract the sea ice changes.

- 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?

We have pointed at Figure 2(d). Combined with Figure 3 or a quick in-the-head calculation we hope it's clear we're looking at around year 40 onwards. We state "approximately" as we just aim to give an order of magnitude.

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

All converted to square brackets for units.

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

We think this is precise enough and given that it's used in other papers so should be clear to most readers, we prefer to keep the shorter phrasing.

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

Caption has been completely changed to refer to panel labels a—d.

- 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.

Done. Sentence added in Figure 3 main text discussion.

- Figure 5:
 - caption: mention that this figure shows 1pctCO2
 - The units should be W/m².

Done, thanks for paying so much attention and catching the axis label error.

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

Done

- 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."

Rephrased to say whether they "simulate" FIRE or not, I prefer this to "neglect" as it sounds more factual and less judgmental. We agree that "neglect" is still better than our original phrasing.

Final sentence rephrased to “...that provide the necessary surface flux and sea ice fields for the scenarios of interest.” We find this more precise, since some models had the fields for some scenarios, but we only used them if they had all fields for all scenarios.

- 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)

Caption rephrased to try and better emphasise that colours refer to calendar months. These are the models for which we had Dr. Kirchmeier-Young’s output for comparison. Comment added to this effect.

- 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 (?)

This was a typo, we have corrected to 1979—1984 in all cases.

- 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).

Done.

- Supplementary Figure 5:
 - “No uncertainties are shown...”: You could detrend the time series before you calculate the standard deviation.

Done, and we show 2 standard deviations to be consistent with other figures. Caption has been rewritten and points are offset to prevent overlap.

- 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.

Discussion of uncertainties removed, we rely on the main text discussion. We have removed the AR(1) analysis based on Supplementary Table 2 and main text discussion. We provide both OLS and Theil Sen, which give similar results. We base our main conclusions on OLS.

Caption now describes the sigma calculations, they are uncertainty of the trend.

Points have been offset slightly so that the bars can be seen on inspection.

Technical corrections:

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

Agreed. Change made in the abstract, but to avoid getting too wordy we now introduce the LW_{\downarrow} and SW_{\downarrow} notation in the Introduction and sometimes use that.

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

Change made.

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

Changed to “Throughout the year...”

- Page 2, line 14, “sea-ice extent”: “sea ice extent”

Change made.

- 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 ...”

Change made. The Cryosphere style in the reference manager won’t remove the brackets on the year, but this can be done during editing if accepted.

- page 3, line 7, “are also necessary”: “are necessary”

Change made.

- page 3, line 22, “tends”: “tend”

Change made

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

We now use LW_{\downarrow} , having introduced this previously.

- 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...”

This was meant to refer to the sea ice albedo feedback over the ocean, not surface snow. Our argument being that for a given retreat in sea ice cover, the no-FIRE simulation has more SW_{\downarrow} so a larger $dSW/dsic$. We have changed phrasing to “sea ice albedo feedback”.

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

Change made. This is nicer!

- page 5, line 3, “who have”: “that provide”

Change made.

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

Change made.

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

Scientific collective acronyms (SCA) is frequently annoying. We have changed all cases we could find to treat FIRE as plural.

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

Change made.

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

Change made.

- Page 7, line 19: delete “also”

Change made.

- Page 8, line 1, “decadal mean SIE”: “decadal mean September SIE”

Change made.

- page 8, line 7, “potential magnitude”: “potential impact”?

Change made.

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

Changed to “in future modelling efforts”. I feel like “future model versions” implies that current CAM doesn’t include it or that there is a risk of future versions of CAM removing FIRE. It’s also possible that new models will be developed.

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

It may be somewhat colloquial but we do not believe that it damages comprehension or reduces precision, so we prefer to keep it.

- Page 11, line 12, “shows”: show

Change made.