Dear Editor,

Please see below for our response to reviewers and a redlined version of our revised manuscript. Reviewer comments are in black, and our responses in red. We give separate a "**comment**" and "**changes**" sections to each comment. We believe we have addressed all reviewer comments.

We also made two changes that were not requested by the reviewers:

1) We refer to CESM1 large ensemble output to show that our findings are outside the standard internal variability simulated by this model
2) We fixed a bug 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. Conclusions are unaffected.

The authors

The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-195-RC1, 2018

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

Anonymous Referee #1

Received and published: 16 November 2018

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 re- producible, 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 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.

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?

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

Changes: N/A

• 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”.

Comments:

Changes: Change made.

• Page 1, line 23, “(extent $< 1x10^{6}$ km^{2})”: Please write to what this number refers to. The minimum extent of the year? The extent averaged over some time (September)?

Comments: We intended that month may be “ice free” but expect September to be the first occurrence.

Changes: We have changed to “monthly mean extent”.

• 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

Comments:

Changes: 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.

Comments:

Changes: 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...”?

Comments:

Changes: 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.)

Comments: 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.

Changes: we have rephrased and use 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.”

Comments:

Changes: 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)

Comments: Good point, but we wish to allow for cases of low GHG emissions but with strong carbon cycle feedbacks too.

Changes: Term changed to “radiative forcing”.

• 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.

Comments:

Changes: 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”

Comments:

Changes: 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)

Comments:

Changes: Change made and now term “$SW_{\downarrow}$” is introduced here.

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

Comments:

Changes: 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)

Comments: This is a nice paper, it fits neatly with our argument so we use it as a reference throughout.

Changes: Citation added and our discussion extended to discuss 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” (?)

Comments:

Changes: 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”

Comments:

Changes: Changed to “The $SW_{\downarrow}$ and $LW_{\downarrow}$ 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”

Comments:

Changes: 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”?

Comments: This was originally 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.

Changes: 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.

Comments:

Changes: 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”

Comments:

Changes: 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?

Comments:

Changes: 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”

Comments:

Changes: New text including:

“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)

Comments:

Changes: 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 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)?

Comments: Oops, we meant “forced”. The implementation is described in Li et al. (2014) but 1pctCO2 is not used there. We think our previous change that discusses CMIP5 protocol is enough for readers to understand.

Changes: “first” replaced with “forced”.

• 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?

Comments: 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?).

Changes: We replaced “definitions” with “estimates”.

• 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.”

Comments:

Changes: 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).

Comments: We used OImon/sic and fx/areacello, thus accounting for the ocean covered area only.

Changes: New text: “total area of all of the model’s native ocean grid cells with sic > 15 %”

• 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?

Comments:

Changes: Changes made, including to "in each surface radiative flux term".

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

Comments:

Changes: 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"?

Comments:

Changes: Done.

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

Comments: We use "controlled" to mean our CESM simulations in which we control whether FIRE are allowed.

Changes: Introduction text added: "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 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).

Comments: Since we are comparing 5-year means (we put 4-, this was a typo and has been corrected), we wish to have the difference between 5-year means and the standard deviation of the sampling distribution of the 5-year mean. We estimate it by taking non-overlapping 5-year periods and then taking their standard deviation. Quadrature is needed because we are looking at the model minus obs difference so need to combine their uncertainties.

Changes: Typo corrected for 4-year versus 5-year.

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.

Comments:

Changes: 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

Comments:

Changes: We now just say "post-1979 SIE".

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

Comments:

Changes: 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.

Comments: We like this suggestion a lot, we find that our results are robust when using Theil-Sen.

Changes: New Supplementary Figure 6 shows OLS (stationary Gaussian white noise assumed) and Theil-Sen fits for 1979—2017.

Figure 1 discussion added: “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.

Comments:

Changes: 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”?

Comments:

Changes: See previous for “controlled”

All uses of CESM1 are now CESM1-CAM5, CESM1-SoN or CESM1-NoS, with the implication being that the SoN and NoS cases use CAM5. Except when referring to CMIP5 or the large ensemble.

• 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 5â: 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 compareswell 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)

Comments:

Changes: Supplementary figure line deleted. Main text now mentions the 1979—2005 SoN-obs trend p = 0.06 and says 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

Comments:

Changes: 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 (?)

Comments: We took a more concise approach. We use Ljung-Box for detection of autocorrelation and see no strong evidence for non-white noise. The lag-1 autocorrelations that are significant in one period are not in the other, and are actually negative which means we are being cautious by using white-noise-based errors. Also, the results are similar for Theil-Sen confidence bounds.

Changes: New paragraph in Section 2.2 and Supplementary Table 2 with statistics calculated from the OLS trend residuals applied to NSIDC SIE.

Later mentions of autocorrelation deleted, we mention that we assume white noise and that Theil-Sen estimates are similar.

t-value deleted.

• page 7, line 21:

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

Comments:

Changes: paragraph has been largely rewritten.

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

Comments:

Changes: 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 CO_2 values)

Comments:

Changes: Lines added.

• 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?

Comments: We intended "naïve" to imply the conclusion following roughly linear assumptions because carbon cycle feedbacks are a massive potential maze.

Changes: 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?

Comments:

Changes: Indeed, fixed.

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

Comments:

Changes: Paragraph now discusses each panel of Figure 4, e.g.: "From Figure 4(b), the net absorbed surface SW radiation shows relatively small SoN-NoS differences because while FIRE reduces $SW_{\downarrow}$, 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 explictly refer to this subfigure.

Comments:

Changes: 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?

Comments: We did not explain this clearly enough! We use "net" consistently to be down minus up, i.e. netRAD" is net SW + net LW, so you can't use Figure 4(a) to make Figure 4(c), you must use Figure 4(b).

Changes: The caption changed, now ends with "All values are defined such that positive indicates that the model shows greater net downward flux than CERES."

Main text changed to better describe this.

• 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"?

Comments:

Changes: 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"

Comments:

Changes: 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 refering to?

Comments:

Changes: Text added to clarify.

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

Comments:

Changes: We chose our own rephrasing: “so the full-period $LW_{\downarrow}$ trend is not responsible…”

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

Comments:

Changes: 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

Comments: We don’t reject normality (Kolmogorov-Smirnov, in previously discussed Supplementary Table). I considered the appropriate value here, and went with 2 standard deviations.

Changes: Changed to mean ±2 standard deviations to be more consistent with other approaches.

• 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?

Comments: We agree this is a solid argument. Exploring this in detail and attempting to partition this quantitatively into e.g. cloud fraction/optical depth/ctP components is tricky, uncertain and distracts from our main points, so we have instead made other changes.

Changes: We added text here and in methods by discussing how our SoN-NoS flux differences include all coupled changes due to inclusion of FIRE. We use your suggestion as an example of such a coupled process. Text added to Section 2.1 saying that rain is excluded, citing Behrangi et al. (2016) which shows that CloudSat R04 products suggest snowfall dominates precip so this probably doesn't matter much.

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

Comments: Yes it does. Added analysis combined with the Massonnet et al. discussion is compelling evidence in support of our arugment.

Changes: New Methods section 2.3 explains CESM1 thickness analysis and new Figure 4 shows results. References added elsewhere to this evidence, including in discussion/conclusions.

• 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?

Comments:

Changes: 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"?

Comments:

Changes: 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)?

Comments:

Changes: 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).

Comments: 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.

Changes: See previous added text, focus on fluxes and added thickness we feel is sufficient.

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

Comments: 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.

Changes: We have rephrased.

• 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?

Comments: We said "approximately" to give an order of magnitude, this can be seen from figures in the paper.

Changes: We have pointed at Figure 2(d).

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

Comments:

Changes: 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

Comments: 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.

Changes: N/A

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

Comments:

Changes: Caption 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.

Comments:

Changes: Sentence added in Figure 3 main text discussion.

• Figure 5:

• caption: mention that this figure shows 1pctCO2

• The units should be W/m^2.

Comments: Thanks for paying so much attention and catching this error.

Changes: Done.

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

Comments:

Changes: 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.”

Comments: “neglect” sounds judgmental to us.

Changes: Rephrased to say whether they “simulate” FIRE or not.

Final sentence rephrased to “…that provide the necessary surface flux and sea ice fields for the scenarios of interest.”

• 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)

Comments: Models are those for which we had Dr. Kirchmeier-Young’s output for comparison.

Changes: Caption rephrased to try and better emphasise that colours refer to calendar months.

• 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 (?)

Comments:

Changes: 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).

Comments:

Changes: Done.

• Supplementary Figure 5:

• “No uncertainties are shown...”: You could detrend the time series before you calculate the standard deviation.

Comments:

Changes: 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.

Comments: Based on Supplementary Table 2 and main text discussion we switch to white noise only and have checked results with Theil-Sen.

Changes: Discussion of uncertainties and AR(1) removed
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)

Comments: Agreed, but this is used a lot so we introduce $LW_{\downarrow}$ and $SW_{\downarrow}$ notation in the Introduction and sometimes use that.

Changes: "Radiation" added to the abstract and introduction, often shorthand thereafter.

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

Comments:

Changes: Change made.

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

Comments:

Changes: Changed to “Throughout the year…”

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

Comments:

Changes: 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 ...”

Comments:

Changes: 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”

Comments:

Changes: Change made.

• page 3, line 22, “tends”: “tend”

Comments:

Changes: Change made

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

Comments:

Changes: 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...”

Comments: 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.

Changes: 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)

Comments: This is nicer!

Changes: Change made.

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

Comments:

Changes: Change made.

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

Comments:

Changes: 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”)?

Comments: Scientific collective acronyms (SCA) is frequently annoying.

Changes: We have changed 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”

Comments:

Changes: Change made.

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

Comments:

Changes: Change made.

• Page 7, line 19: delete “also”

Comments:

Changes: Change made.

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

Comments:

Changes: Change made.

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

Comments:

Changes: Change made.

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

Comments: I feel that “future model versions” implies that current CAM doesn’t include it or that future versions might remove it. It is also possible that future models will be developed.

Changes: Changed to “in future modelling efforts”.

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

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

Changes: N/A

• Page 11, line 12, “shows”: show

Comments:

Changes: Change made.

Reviewer 2

Review of Li et al:

I will keep it short and to the point. Li et al bring in an important aspect into discussion here, i.e. the radiative effects (particularly longwave warming) of falling snow. From the process point of view, I do appreciate that the authors highlight its potential importance and encourage modelling community to take this process into account. The manuscript is written and presented nicely. The analysis is robust and the arguments are justified well based on the results presented here. I do however have few major comments.

The overwhelming focus on the radiative effects, by neglecting the dynamical and surface aspects, concerns me. I understand that the authors neglect them for the sake of simplicity, but they are actually important here. For example, between the two sets of CMIP5 models, SoN and NoS, the former shows more realistic trends in sea-ice extent. Could it be a coincidence? How much of it is really down to including FIRE and/or down to having different dynamical responses and surface descriptions in these sets of models? Please note that CMIP5 models vary widely in their description of sea-ice (e.g. Koenigk et al., 2014). Could the authors please check how the SoN and NoS models differ in these aspects?

FIRE would depend not only on how much it precipitates, but also on the frequency of falling snow. But there seems to be hardly any discussion about this (and how it varies across NoS and SoN). Or am I missing something here?

I hope the authors comment on these issues.

References

Koenigk, T., Devasthale, A., and Karlsson, K.-G.: Summer Arctic sea ice albedo in CMIP5 models, Atmos. Chem. Phys., 14, 1987-1998, https://doi.org/10.5194/acp-14-1987-2014, 2014

Comments: Thanks for taking the time to read & review our paper. You have highlighted ways in which the original submissions was unclear so we have substantially re-written the paper to address your concerns and those of reviewer 1.

Basically, we think you're right: the CMIP5-SoN September retreat looking "better" relative to observations is largely due to chance, so FIRE alone are not a big enough factor to overcome all other inter-model differences. Nevertheless, we are convinced that if the magnitude of FIRE as calculated by CESM1-CAM5 are realistic, then FIRE are important to improve simulated Arctic sea ice.

We don't see the benefit of a detailed analysis of model schemes, beyond how our discussion & conclusions comments that the two CMIP5-SoN models which reach ice free states the earliest are the GISS models and that is likely due to other parts of their cloud schemes resulting in underestimated IWP and way too much summer $SW_{\downarrow}$.

We made many, many changes in response to reviewer 1, including extended statistical testing and analysing initial sea ice thickness in CESM1 SoN and NoS, finding that it supports our conclusion. We hope that our changes have clarified our approach and that you agree our main conclusions are suitably supported with the caveats and uncertainties adequately explained.

Changes:

Text added to Section 1 details some ways in which CMIP5 sea ice simulations can be affected. We refer to Koenigk et al. (2014) as well as Karlsson & Svensson (2013), and Massonnet et al. (2018) while discussing the variation in sea ice schemes.

The bit about mixed phase clouds as an example of how atmospheric components can matter (with the Cesana & Tan papers) has been moved here from the discussion, and a comment on how the representation of ocean eddies with a citation to Horvat & Tziperman (2018) has also been added.

Section 5 paragraph 2 now specifically states that "the faster September retreat of CMIP5-SoN in Figure 1 is likely due to the full combination of properties in these models and not directly due to FIRE. Nevertheless, the controlled CESM1-CAM5 simulations demonstrate that the inclusion of FIRE…".

Our introduction now specifies that our two main aims are to determine whether FIRE in simulations leads to important differences in simulated sea ice, and whether our hypothesis of a thinner and more vulnerable pack is supported.

Section 2.1 now includes the following: "The strength of FIRE and the simulated response of other properties to FIRE depend on the frequency as well as the intensity of snowfall. This is accounted for in the model as radiative transfer is calculated at each model time step even though outputs are only provided monthly." Further text explains that by looking at the differences in radiation terms directly we are actually comparing the fully coupled response due to FIRE, and cites Chen et al. as an example of how FIRE can cause such coupled responses. This means we fully capture the factors that are physically relevant to the sea ice retreat, but we cannot disentangle how much of the "real" cause is direct FIRE versus changes induced in circulation. We think that our re-written paper is sufficiently careful in its phrasing to emphasise this point and limit our conclusions to those for which we have sufficient supporting evidence.

Potential faster Arctic sea ice retreat triggered by snowflakes' greenhouse effect

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Abstract. Recent Arctic sea ice retreat has been quicker than in most general circulation model (GCM) simulations. ~~Natural factors~~Internal variability may have amplified ~~this~~the observed retreat in recent years, but reliable attribution and projection requires accurate representation of relevant physics. Most current GCMs don't fully represent falling ice radiative effects (FIRE), and here we show that the small set of Coupled Model Intercomparison Project, phase 5 (CMIP5) models that include FIRE tend to show faster observed retreat. We investigate this using controlled simulations with the CESM1-CAM5 model. Under 1pctCO2 simulations, including FIRE results in the first occurrence of an "ice free" Arctic (monthly mean extent < 1×10^6 km^2) at 550 ppm CO_2, compared with 680 ppm otherwise. Over 60—90 °N oceans, snowflakes reduce downward surface shortwave radiation and increase downward surface longwave radiation, improving agreement with the satellite-based CERES-EBAF surface dataset. We propose that snowflakes' equivalent greenhouse effect ~~results in fewer safe spaces in~~reduces the mean sea ice thickness ~~which sea ice can thicken during winter~~, resulting in a thinner pack whose retreat is more easily triggered by global warming. This is supported by the CESM1-CAM5 surface fluxes ~~controlled CESM1-CAM5 simulations,~~ and a reduced initial thickness in perennial sea ice regions by approximately

0.3 m when FIRE are included. T~~but t~~his explanation does not apply across the CMIP5 ensemble where inter-model variation in the simulation of other processes ~~can~~ likely dominates. Regardless, we show that FIRE can substantially change Arctic sea ice projections and propose that better including falling ice radiative effects in models is a high priority.

1 Introduction

The Arctic region is undergoing pronounced change, becoming warmer and wetter (Boisvert and Stroeve, 2015) while its land ice melts (Jacob et al., 2012; Kjeldsen et al., 2015) and spring arrives weeks earlier than in the 1990s (Post et al., 2018). Communities in the region may have to adapt to changing hunting seasons (Rolph et al., 2018), loss of coast that was previously protected by sea ice (Overeem et al., 2011) and ~~the~~ surface destabilisation due to permafrost melt (Shiklomanov et al., 2017).

In particular, Arctic sea ice retreat potentially opens area for resource extraction or transport routes (Smith and Stephenson, 2013) and has national security implications for neighbouring states. Physically, sea ice affects both top-of-atmosphere and surface heat fluxes. In winter it insulates the ocean, restricting the leakage of heat to space via infrared cooling, and in summer it predominantly reflects sunlight and cools the surface (Tietsche et al., 2011). ~~From a surface perspective~~Throughout the year~~,~~ it restricts evaporation and therefore affects the hydrological cycle (Bintanja and Selten, 2014). It has been proposed that reduced sea ~~-~~ice extent may further smooth the latitudinal temperature gradient, thus weakening the high latitude jets and making it easier to shift into a “wavy” pattern, which is associated with long-lived extreme events at mid latitudes (Francis and Vavrus, 2012). However, these proposed impacts at lower latitudes are currently speculative and disputed (Cohen et al., 2014).

The recent rapid Arctic sea ice retreat included extreme minima in 2007 and 2012 which received particular attention. Regarding the 2007 minimum, a reduction in cloudiness during the melt season relative to previous years was shown to change surface energy balance by enough to thin sea ice by up to 0.3 m over three months (Kay et al., 2008).

~~Natural atmospheric~~ Atmosphere and~~&~~ ocean dynamics may also ~~contribute by~~ export~~ing~~ 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)~~, which tends to increase extent in winter but ultimately reduce it in summer~~. ~~For example, s~~Surface pressure observations have been used to infer contributions to summer sea ice reduction due to anomalously high ice export through the Pacific sector in 2007 (Zhang et al., 2008) and the Fram Strait in 2012 (Smedsrud et al., 2017).

~~From analyses of subsets of climate models in the~~Based on CMIP5 output (Climate Model Intercomparison Project, phase 5~~ (CMIP5~~; Taylor et al. (2012)), 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~~ internal variability, such as extreme cloud anomalies affecting surface radiation (Kay et al., 2011) and from 1990 through the early 2000s, potentially wind-driven factors (Rigor and Wallace, 2004). One recent study suggested an equally important role for anthropogenic warming and natural variability for the extreme 2012 loss (Kirchmeier-Young et al., 2017).

Reliable attribution requires the ability to quantify physical processes and relevant responses to each forcing. A better understanding of the processes that are responsible for sea ice retreat will help to reduce uncertainties in future projections. Accurate future projections are ~~also~~ necessary for informed decisions with the changing Arctic, such as by investors or insurance companies who may wish to assess the risk associated with proposals for future shipping routes. A common criterion is determining if and when a seasonally "ice free" Arctic will occur, arbitrarily defined as when sea ice extent falls below 1×10^6 km^2. At this point the remaining ice would cluster around islands and coasts, leaving the basin largely open.

Climate models are crucial tools to inform projections but their Arctic response varies widely (Massonnet et al., 2012; Stroeve et al., 2012). The time at which the Arctic is likely to become "ice free" under high ~~emissions~~ radiative forcing in CMIP5, for example, ranged from 2041—2060 in Massonet et al. (2012) while Stroeve et al. (2012) only stated that "a seasonally ice-free Arctic Ocean within the next few decades is a distinct possibility".

Observed ~~Summer~~ summer retreat has been faster than the average CMIP5 model simulation~~, implying a large naturally forced component to recent extremes. However,~~ and if the CMIP5 models do not adequately include factors that influence ~~the forced response~~sea ice retreat then their projections will be biased. We have previously shown that the majority of CMIP5 models do not properly account for atmospheric ice in their radiation codes. While they include suspended ice, falling ice is excluded and this causes region-dependent biases in the surface energy budget that, for example, tend~~s~~ to result in a larger mean Antarctic sea ice extent (Li et al., 2017).

Here we focus on sea ice extent changes and the surface energy budget over oceans from 60—90 °N. In the simplest terms, falling ice should produce a year round increase in downward surface longwave radiation ($LW_{\downarrow}$) and a decrease in downward surface shortwave radiation ($SW_{\downarrow}$), which will be greatest in local summer. Li et al. (2017) showed that in the Antarctic this results in a dampened annual cycle with the increased wintertime ~~longwave~~ $LW_{\downarrow}$ restricting maximum sea ice extent, which then results in a lower albedo when the sun rises again. This lower albedo counteracts somewhat the reduction in sunlight arriving at the surface due to reflection by snowflakes.

With regards to the Arctic, we expect a somewhat different ~~expression~~ response due to (1) wintertime maximum extent being restricted by continental boundaries and boundaries with warm ocean currents, (2) generally thicker sea ice (Kurtz and Markus, 2012; Kwok and Cunningham, 2008) and (3) faster local warming under the early part of CO_2-driven heating.

It is therefore possible that increased ~~winter longwave~~ $LW_{\downarrow}$ from FIRE may not have a substantial effect on winter sea ice extent, but may restrict its thickness. This should ~~manifest later as a~~favour faster retreat in sea ice cover~~,~~ both during a typical summer melt season and during long-term warming. ~~.~~ However, if the maximum wintertime extent is not strongly affected then the albedo will begin the melt season at a similar level regardless of FIRE~~,~~ and ~~therefore there will be no offset for the stronger expected downward shortwave. This will mean that~~ a non-FIRE simulation should ~~experience more~~ have a stronger local sea ice albedo feedback due to its stronger $SW_{\downarrow}$~~incident sunlight~~. ~~These~~The $SW_{\downarrow}$ and $LW_{\downarrow}$ effects from including FIRE should oppose each other and it is not necessarily obvious whether one factor ~~should~~ will dominate.

From the Antarctic sea ice results of Li et al. (2017), we suspect that the longwave effect is more important for the mean state. Our hypothesis is that FIRE increases year round $LW_{\downarrow}$ and results in a thinner sea ice cover on average. It is then easier to melt this pack as temperatures warm and our hypothesis is related to the recent findings of Massonnet et al. (2018) who also describe several relevant physical processes. They found that across CMIP5 models, sea ice retreat is correlated with parameters representing seasonal growth and retreat. ~~-~~They considered differences between the level of sophistication of the sea ice components of the CMIP5 models and found that the background thickness was more strongly related to sea ice retreat than model sophistication. This sensitivity of sea ice retreat to initial thickness supports our hypothesis although we focus on an atmospheric driver of changes in the initial mean state of thickness, namely FIRE.

As well as changes in the mean state which could affect retreat through the initial pack's robustness, it is also possible that local fluxes could vary in different ways under warming. For example, in a simulation where FIRE are~~is~~ included, warming could raise the atmospheric melting layer during summer, leading to a reduction in ~~the total ice~~snow water path ~~(TIWP)~~ in favour of ~~liquid water~~rainfall, which is not included in the radiation code. ~~which has a smaller radiative effect.~~ The direct ~~effect~~ consequence of this would be to reduce the trend in $LW_{\downarrow}$~~downward longwave~~ and increase the trend in $SW_{\downarrow}$~~downward shortwave~~, relative to a simulation where FIRE are~~is~~ excluded. This ignores further coupling to atmospheric conditions that could similarly affect feedbacks.

Here we investigate the importance of FIRE using both standard CMIP5 output along with ~~controlled~~ simulations with a CMIP5 era climate model, the National Center for Atmospheric Research-Department of Energy (NCAR-DOE) Coupled Earth System Model version 1 with the Coupled Atmosphere Model version 5 (CESM1-CAM5). 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.

Our two main aims are to determine whether FIRE substantially change simulated Arctic sea ice and, more specifically, to test our hypothesis that FIRE tends to reduce mean initial sea ice thickness and thereby leave it more vulnerable to retreat under warming.

CMIP5 output will be used to determine whether differences in simulated sea ice can be detected between FIRE and non-FIRE models across the ensemble, and if so whether the changes can be linked to radiative heat fluxes in a way consistent with our expectations from FIRE.

The CMIP5 models have many differences that may affect sea ice extent, most obviously in their sea ice components. The sea ice albedo schemes for example vary in their sophistication and treatment of snow on ice, melt bonds and response to temperature. The resultant inter-model spread in local albedo feedback does not appear to explain much of the inter-model variance in long-term retreat (Koenigk et al., 2014), but modelled sea ice albedo does correlate with the amplitude of the annual cycle sea extent (Karlsson and Svensson, 2013). As described in Massonnet et al. (2018), any process that affects the baseline thickness may be related to future retreat, and this includes ocean eddy heat flux (Horvat and Tziperman, 2018) and cloud schemes that affect surface radiation and temperature change. For example in CESM1-CAM5.1, matching the the observed prevalence of mixed phase clouds at low temperatures (Cesana et al., 2012, 2015) results in approximately 1 °C more warming under CO_2 doubling (Tan et al., 2016). Such a large increase in warming would be expected to also change projected sea ice extent. Differences in sea ice, ocean and atmosphere schemes may drive changes that confound detection of FIRE-driven sea ice effects across the CMIP5 ensemble so our analysis of controlled CESM1-CAM5 simulations in which the only difference is the inclusion of FIRE allows a direct comparison. In these simulations our analysis ignores coupled dynamical responses in favour of studying the surface radiative flux terms that provide a direct test of our hypothesis. We ignore coupled dynamic responses in favour of studying the direct surface radiative flux terms to simplify the analysis.

The paper is structured as follows: Section 2 lists the data and methodology, Section 3 reports on the simulated and& observed sea ice changes, Section 4 looks at the simulated & and observed surface radiative fluxes, Section 5 synthesises and discusses the results and their limitations, and Section 6 concludes.

2 Methods and Data

2.1 CMIP5 and CESM1-CAM5 Simulations

We use outputs from the CMIP5 archive (Taylor et al., 2012) and select models who havethat provide all surface energy balance terms plus the fields necessary to calculate sea ice extent for each of the preindustrial control (piControl), historical and Representative Concentration Pathway 8.5 (RCP8.5, Riahi et al. (2011)) scenarios. The historical scenarios run through 2005, after which we append the RCP8.5 output. This is a scenario of very high radiative forcing, which we select to better identify forced response over internal variability, and we make no judgment about the probability that this forcing will occur. For each model we select the first simulation in each case, r1i1p1 in CMIP5 nomenclature, which results in 25 simulations.

We split these into two sub-ensembles depending on whether FIRE are~~is~~ allowed: those including snow radiative effects (CMIP5-SoN, N = 7) and those in which ~~there are no~~ falling snow radiative effects are not considered (CMIP5-NoS, N = 18). ~~These~~ All models are listed in Supplementary Table 1.

For CESM1-CAM5 we use previously published historical simulations (Li et al., 2014), which are run on a spatial resolution close to a 1×1° latitude-longitude grid and follow the CMIP5 historical protocol. CAM5 is one of the few atmospheric models that allows snow radiative interactions, and it does this thanks to a two-moment ~~cloud scheme with diagnostic~~treatment of rain and snow. 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) ~~(Gettelman et al., 2010; Morrison and Gettelman, 2008)~~. Th~~is~~e scheme only represents the stratiform component of falling ice and not that in convective towers, but the majority of Arctic snowfall will be included. With ~~This~~ this scheme ~~allows~~ snow radiative effects can~~to~~ be allowed (CESM1-SoN) or disallowed (CESM1-NoS), and the inclusion or exclusion of FIRE is the only difference between the SoN and NoS simulations. The radiative effects of rain are not included in any of the CESM1-CAM5 simulations, but this is unlikely to be an issue for much of the Arctic. Even ignoring the differences in how rain and snow affect radiation, CloudSat radar-based products report that Arctic precipitation frequency and amount is dominated by snow (Behrangi et al., 2016).

The strength of FIRE and the simulated response of other properties to FIRE depend on the frequency as well as the intensity of snowfall. This is accounted for in the model as radiative transfer is calculated at each model time step even though outputs are only provided monthly. Note that the CESM1-SoN and

CESM1-NoS simulations are independent so will have different amounts and patterns of snowfall, and that by including FIRE there can be coupled changes in heating rates, circulation and precipitation (Chen et al., 2018). We later use the SoN-NoS surface radiative flux differences because these include the full coupled changes due to FIRE and are the properties most directly relevant for sea ice changes.

~~-~~Unfortunately, output is not available for any RCP, which forces observational comparisons to end in 2005. To estimate ~~the~~ how ~~first~~ sea ice extent changes under greater forcing~~response,~~ we use output from available ~~the 1pctCO2 output~~simulations following the CMIP5 1pctCO2 protocol~~, –~~ in which ~~is a simulation in which~~ atmospheric CO_2 increases at 1 % yr^{-1} for 140 years from an initial value near 280 ppm. Radiative forcing ~~definitions~~ estimates differ, but typical values for quadrupled CO_2 are 5.3—8.6 W m^{-2} (Forster et al., 2013) , meaning that total forcing is similar to the historical-RCP8.5 series used for CMIP5. We use output ~~for~~ from fully coupled CESM1-SoN and for CESM1-NoS runs following the historical and 1pctCO2 simulations.

2.2 Sea Ice Extent

Sea ice extent (SIE) is defined as the area of ocean with sea ice concentration (sic) greater than 15 %. This was originally developed for satellite-based passive microwave products to be a robust identifier of ice edges when compared against aircraft observations (Cavalieri et al., 1991). This ~~aids~~ threshold means retrieved sea ice edges are less sensitive ~~with robustness of the retrieval~~ to changing weather conditions or melt ponds on the ice which may interfere with the observed brightness temperatures. For observations we use the National Snow and Ice Data Center (NSIDC) monthly series of total sea ice extent (Fetterer et al., 2017) which is calculated from gridded data on a nominal 25 km grid. We use the complete years that were available as of analysis time: 1979—2017.

The standard CMIP5 output is the sea ice concentration within an ocean grid cell, and we calculate sea ice extent following a previously published method (Kirchmeier-Young et al., 2017), by reporting the total area of all of the model's native ocean grid cells with sic > 15 % (see Supplementary Figure 1 for verification of this calculation). This is not a fully consistent comparison due to differences in grid cell sizes and as observations may underestimate sea ice concentration in the presence of substantial melt ponds. Here we assume that these factors have little effect on the large-scale changes under study.

To represent the magnitude of changes in SIE we apply optimised least squares (OLS) to each calendar month's time series separately (e.g. all Januaries for 1979—2005) assuming Gaussian white noise and report both trend estimates and their associated errors. We justify this based on analysis of the detrended residuals of the NSIDC dataset applied to 1979—2005 and 1979—2017. While some months reject white noise at $p < 0.05$ according to the Ljung-Box test applied for lag-1 autocorrelation, these results are not robust since no calendar month rejects white noise over both periods. No month shows residuals that are significantly different from normality according to the Kolmogorov-Smirnov test: see Supplementary Table 2 for summary of Pearson's r, Ljung-Box p and Kolmogorov-Smirnov p.

2.3 Sea Ice Thickness

Given that our hypothesis is that FIRE drives changes in the initial mean sea ice thickness, we also compare the CESM1-SoN and CESM1-NoS sea ice thickness in the 1pctCO2 simulations. Regional average sea ice thickness is calculated by appropriately area weighting the ice covered area of each grid cell included in the region. For a consistent comparison we select all grid cells where both simulations have greater than 80 % mean sea ice concentration for all calendar months averaged over years 1—20 and 21—40 of their 1pctCO2 simulations. The selected region changes between each period, and a static region poleward of 80 °N as in Massonnet et al. (2018) is also shown. The 80 % concentration threshold means the areas are consistently ice covered and includes about five times as much area as using a 90 % threshold, so our thicknesses are more representative than using a stricter cut off (Supplementary Figure 2). The mean thickness in each region is calculated for each calendar month and our hypothesis is supported if the CESM1-SoN mean thickness is greater than the CESM1-NoS mean thickness in this region.

2.43 Surface Energy Budget

We use 1°×1° monthly estimates of surface fluxes from the Clouds and the Earth's Radiant Energy System Energy Balanced and Filled-Surface (CERES-EBAF Surface, Kato et al. (2013)) product, for which we have complete years for 2001—2015. ~~This~~ CERES-EBAF Surface combines satellite data with a radiative transfer model to ~~estimate~~ calculate surface fluxes and is estimated to have a monthly root mean square error of ±11 W m^{-2} in each surface radiative flux term over oceans (Kato et al., 2012).

CESM1-CAM5 output is provided monthly at 1°×1° and for all CMIP5 models, we use previously ~~gridded~~interpolated 2.5°×2.5° monthly data. Fluxes are calculated by taking the area-weighted average of values in each grid cell after scaling by the ocean fraction (total ocean fraction, including sea ice covered ocean). For CERES and CESM1-CAM5 we use the CESM1-CAM5 ~~grid~~land sea mask, and for all CMIP5 models we use a consistent ~~map~~ fractional land sea mask built from the 0.125°×0.125° European Center for Medium Range Weather Forecasts European Reanalysis-Interim (ECMWF ERA-Interim) land mask. For comparison of the mean state fluxes between CERES and our controlled historical CESM1-CAM5 simulations we only have ~~4~~ 5 complete years of overlap, 2001—2005 inclusive.

We consider the difference CESM1-CAM5 minus CERES but since our simulations are coupled, internal variability could increase the apparent model-observation discrepancy. As an estimate of the magnitude of internal variability on our 5-year averaged fluxes, we detrend the model output over 1981—2005, and the CERES output over 2001—2015 then slice these into non-overlapping 5 year periods. The standard deviation is calculated for the modelled and observation-based samples and then these are added in quadrature to provide a value for the CESM1-CAM5 minus CERES difference.~~We therefore show the results for these 4 years with error bars estimated by slicing both CERES and CESM1-CAM5 post-1979 data into non-overlapping 4-year periods and taking the standard deviation of these samples. For differences these standard deviations are added in quadrature and reported as an estimate of the uncertainty.~~ This estimate only represents the effect of internal variability due to our use of a short time period, and may be biased if the variance in these terms changed greatly from 1979—2015. Given the brevity of the available data record we consider this simple approach to be adequate.

3 Observed and Simulated Sea Ice Extent and Thickness Results.

3.1 Sea Ice Extent

Figure 1 shows the March and September post-1979 changes in SIE in NSIDC observations and CMIP5 simulations. These are the months of maximum and minimum SIE (all months are shown in Supplementary Figure 32). Figure 1(b)The upper right panel shows that observed September retreat approaches the lower 10^{th} percentile of the CMIP5 ensemble. When plotted using anomalies, the retreat falls outside the model range (see Supplementary Figures 34 for absolute anomalies, Supplementary Figure —45 for relative anomalies).

In Figure 1(c,d) the results are split into CMIP5-SoN and CMIP5-NoS sub-ensemblesThe bottom panels of this figure show that the CMIP5-SoN sub ensemble generally agrees better with the faster observed retreat, with Figure 1(d) showing that CMIP5-SoN better captures the observed September retreat over 1979—2017. The median CMIP5-SoN trend is more negative than that of CMIP5-NoS from June through October, in better agreement with observations (Supplementary Figure 6). In March, trends are similar but CMIP5-SoN shows greater extent, which is the opposite of expectations if wintertime $LW_{\downarrow}$ longwave from FIRE were the main cause of differences. However, inter-model differences in parameterisations and calculation methods for clouds, the atmosphere, oceans and sea ice can change the mean state, so to isolate FIRE we present the controlled CESM1-CAM5 simulations in Figure 2.

CESM1-CAM5 captures theOver 1979—2005 there is a smaller mean extent well with a smaller discrepancy between CESM1-CAM5 and observations for monthly mean extent versus observations throughout the year when including FIRE (full see annual cycles in Supplementary Figures 7). R5—6). Historical retreat during the same period is also faster in CESM1-SoN than in CESM1-NoS: for September the SoN minus NoS series is significant at 2.61σ (white noise $p = 0.01$, see Supplementary Figure 8). The CESM1-SoN September retreat is faster than in reality over 1979—2005, but not significantly so ($p = 0.06$)., but only significantly so if white noise is assumed Real world Arctic sea ice retreated more rapidly after 2005, but we do not have the output to determine whether this means that CESM1-SoN would then show better agreement. ($t = 2.39$, $p = 0.012$), whereas after accounting for lag-1 autocorrelation above 0.4 the difference is insignificant ($t = 1.51$, $p = 0.073$). Neither show significant differences relative to NSIDC observations over 1979–2005 although the 1979–2017 trend is detectably faster than the CESM1-NoS changes through 2005. The bottom panels show that inclusion of FIRE results in a much faster September retreat beginning around year 40 of the simulation in the 1pctCO2 simulation. For increased warming we must turn to the 1pctCO2 output, and Figure 2(d) shows accelerated retreat in CESM1-SoN following year 40, corresponding to CO_2 levels of 416 ppm, a value that current trends suggest will occur in the 2020s.

To allow easier interpretation, we take overlapping decadal averages of mean SIE and the number of years within that decade with SIE < 1×10^6 km^2, and plot these as a function of atmospheric CO_2 concentration (assuming year 0 = is approximately 280 ppm) in Figure 3. Below the 2017 atmospheric CO_2 concentration, Figure 3(a) showsthere are only small differences in decadal mean September SIE, but for concentrations higher than this the Arctic sea ice retreats far more rapidly under global warming when FIRE is are included. Note that these simulations exclude non-CO_2 forcings such as aerosol, which are present in reality. In the CESM1-SoN simulation, Figure 3(b) shows that the majority of years are classified as ice free once atmospheric CO_2 passes 550 ppm, compared with 680 ppm in the CESM1-NoS simulation. In a naïve sense (i.e. assuming approximately constant airborne fraction as occurs for these decades in some 1pctCO2 simulations, e.g. Matthews et al. (2009)) this implies a difference of almost 100 % in cumulative future anthropogenic CO_2 emissions before the Arctic commonly becomes ice free if these CESM1-CAM5 1pctCO2 simulations are representative of the real world. Figure 3 shows that the potential magnitude impact of FIRE on Arctic sea ice retreat is large, but we do not argue that this necessarily means a more rapid collapse ofthat Arctic sea ice will necessarily collapse more rapidly than indicated by CMIP5in reality. Firstly, CESM1-CAM5 may have compensating biases due to other processes and secondly the disappearance of ice under transient CO_2-driven warming may not correspond to reality where a mixture of radiative forcing agents is changing. Some of these, such as aerosols, may drive stronger seasonal, regional, and dynamic responses than well-mixed greenhouse gases like CO_2 (Hansen et al., 1997).

A further consideration is that internal variability can change when an ice free state occurs. Under RCP8.5 the CESM1 large ensemble of 40 runs (Kay et al., 2015) shows a 14-year range between members when ice free is defined based on the five-year average (Jahn et al., 2016). The CESM1-SoN to CESM1-NoS 1pctCO2 difference by this criterion is 20 years so our conclusion that FIRE drives faster retreat is likely robust to internal variability.

These simulations show that falling ice radiative effects could lead to much greater Arctic sea ice retreat when the system is forced under global warming and support the inclusion of FIRE in future modelling effortss. Next, we investigate whether the surface radiative energy balance allows us to identify candidate physical processes that explain these changes, and whether the processes identified using CESM1-CAM5 can be detected across the CMIP5 ensemble.

3.2 Sea Ice Thickness in CESM1-SoN and CESM1-NoS

Figure 4(a,d) outline the regions within which thickness is calculated for years 1—20 and 21—40, and the annual cycles of mean thickness for each period and simulation are shown in Figure 4(b,c,e,f). Consistent with our hypothesis, the CESM1-SoN ice pack starts off thinner than that of CECSM1-NoS. Over the Arctic interior the pack tends to be 20—30 cm thinner throughout the year. The remaining perennial >80 % sea ice concentration region for years 21—40 in Figure 4(e) shows a 1.4 m difference.

4 Observed and Simulated Surface Radiative Fluxes

4.1 CESM1-CAM5 Controlled Simulations

In Section 1 we discussed the expected direct effects of FIRE on surface longwave (LW) and shortwave (SW) radiative fluxes and how these might be related to SIE. We begin our analysis with the downward fluxes at the surface, $LW_{\downarrow}$ and $SW_{\downarrow}$ in CESM1-CAM5 compared with CERES-EBAF Surface observations during their overlap period of 2001—2005. Uncertainties are based on the standard deviation of non-overlapping fourfive-year periods from the rest of their records as described in Section 2. The CESM1-CAM5 minus CERES-EBAF Surface flux differences over 60—90 °N oceans are displayed in Figure 5 for each calendar month. As expected, inclusion of FIRE results in increased $LW_{\downarrow}$ and decreased

$SW_{\downarrow}$, resulting in better agreement with the observation-based CERES data. Figure 5(a) shows that the $LW_{\downarrow}$ difference is greatest in winter, when the $SW_{\downarrow}$ is negligible due to the lack of available sunlight. The SoN-NoS difference in $SW_{\downarrow}$ is greater than in $LW_{\downarrow}$ during summer, but only marginally so, and the annual average $LW_{\downarrow}$ difference is greater. From Figure 5(b), the net absorbed surface SW radiation shows relatively small SoN-NoS differences because while FIRE reduces $SW_{\downarrow}$, 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 5(c). ~~Absorbed longwave dominates, but CESM1-SoN's lower SIE results in a lower albedo that more than offsets the reduced $SW_{\downarrow}$ such that absorbed SW is also higher when including FIRE.~~ The annual average ~~downward longwave~~annual average $LW_{\downarrow}$ is 11 W m^{-2} higher ~~on average~~ when including FIRE, which will increase mean ice temperature and increase heat input, resulting in a thinner pack that is more vulnerable to warming.

It is also possible that local radiative feedbacks could be different when including or excluding FIRE. This would manifest as a change in the SoN minus NoS flux differences over~~in~~ time and for this we switch back to analysis of the 1pctCO2 simulations. ~~is tested in~~ Figure 6~~, which~~ includes the 1pctCO2 $SW_{\downarrow}$ and $LW_{\downarrow}$ differences for each season: December-January-February (DJF), March-April-May (MAM), June-July-August (JJA) and September-October-November (SON). Long-term ~~changes~~ trends are estimated by multiplying the OLS trend gradient by the length of the period, and the only significant ($p < 0.05$) ~~changes~~ trend occurs in SON~~,~~ where there is a decrease in the radiative flux difference between the two simulations.

However, the SoN minus NoS $LW_{\downarrow}$ trend is insignificantly positive during the first 70 years (+0.08±0.09 W m^{-2} yr^{-1}, ±2σ error in OLS trend), so ~~this~~ the full-period $LW_{\downarrow}$ trend~~change~~ is not responsible for driving the faster disappearance of sea ice in CESM1-SoN which has largely occurred by year 70 (as shown by Figure 2(d) for September). Instead, the difference appears related to differences in the relative effects of FIRE between icy and ice-free states. During the first 40 years when the simulations both have a healthy Arctic ice cover the ~~median~~ mean SON difference in $LW_{\downarrow}$ is 11.6~~2~~±11.1 W m^{-2} (~~6.4 – 16.~~±2 standard deviations~~9 W m^{-2}, henceforth bracketed values are 14 – 86 % range~~) whereas for the final 40 years where both simulations are ice free during September, the difference is 7.1~~6.8~~±6.6 ~~(4.9 – 10.2)~~ W m^{-2}. This difference could be related to Some combination ofchanges in cloud properties or precipitation phase in response to sea ice cover, such as the transition from snow to rain under warming, likely explain this difference; the CESM1-SoN simulation initially has a smaller sea ice extent but by the end of the simulation both CESM1-SoN and CESM1-NoS are largely ice free..

Taken together, the energy budget analysis of CESM1-CAM5 1pctCO2 simulations indicates that differences in flux trends due to FIRE do not drive the faster observed retreat, but instead the effect of stronger year-round $LW_{\downarrow}$ in the initial state is the most important radiative contribution. This supports our argument that the effective greenhouse effect from snowflakes results in a thinner pack whose retreat is more easily triggered by warming. This snowflake greenhouse effect is present year round and throughout the entire Arctic basin, leaving no safe spaces where the ice can fully recover.

4.2 CMIP5 Ensemble Results

The CESM1-CAM5 results show that snow radiative effects can substantially change simulated Arctic sea ice retreat under warming, which is consistent with the generally earlier disappearance of sea ice seen under historical-RCP8.5 simulations for the CMIP5-SoN sub-ensemble, compared with the CMIP5-NoS ensemble. To investigate this, we consider the CMIP5 1979—2005 mean annual cycle and the 2006—2035 linear regression trends for each calendar month for a variety of properties in Figure 7. Each simulation's line is coloured according to whether it includes FIRE (SoN, blue) or excludes FIRE (NoS, red).

The mean state period is the overlap between NSIDC passive microwave sea ice extent data and the historical simulations, and the trend period covers 30 years in which Figure 1 shows an apparent notable divergence in SIE between the CMIP5-SoN and CMIP5-NoS sub-ensembles.

Inspection of Figure 7 shows no clear support across the CMIP5 ensemble for the hypothesis we developed using the controlled CESM1-CAM5 simulations. In fact, Figure 7(d) shows that two models that include FIRE show substantially more summertime $SW_{\downarrow}$ (e.g. 45 W m^{-2} more than the median of all other CMIP5 models), which is the opposite of the direct effects we hypothesise are related to FIRE. These are the models are GISS-E2-H and GISS-E2-R, whose CMIP5 versions greatly underestimated mean Ice Water Path (IWP) poleward of 60—90 °N (Stanfield et al., 2014). This illustrates how other differences aside from FIRE may well have compensating effects, showing that FIRE alone is insufficient to explain differences in Arctic sea ice retreat among models.

5 Discussion and Conclusions

The apparent agreement in September sea ice retreat between CMIP5-SoN and CESM1-SoN seen in Figure 1 and Figure 2 appears supportive of a major role for falling ice radiative effects in reinforcing Arctic sea ice retreat. ~~However, analysis of the surface energy budget terms allowed us to identify a plausible physical mechanism in CESM1-CAM5, but revealed that~~However, the CMIP5 result was ~~fortuitous and~~ largely due to extremely early ice disappearance in the GISS-E2 models which accounted for two out of seven of the sub-ensemble members. These models have been shown to drastically underestimate total ice water path, resulting in too much ~~surface shortwave radiation~~$SW_{\downarrow}$ during summer and therefore likely a very strong surface albedo feedback. As detailed in Section 1, simulated sea ice is affected by many model design factors including the sea ice albedo scheme, ocean eddy heat transport and cloud simulations. Therefore, the

~~The~~ CMIP5 cross comparison simply shows that Arctic sea ice projections are at least as sensitive to other factors as to the inclusion or exclusion of FIRE and the faster September retreat of CMIP5-SoN in Figure 1 is likely due to the full combination of properties in these models and not directly due to FIRE. Nevertheless, the controlled CESM1-CAM5 simulations demonstrate that the inclusion of FIRE in this model results in a thinner sea ice pack and a faster retreat in extent over both 1979—2005 and in 1pctCO2 simulations. The difference between CESM1-SoN and CESM1-NoS in 1pctCO2 is larger than the range of values due to internal variability spanned by the CESM1 large ensemble so we conclude that we have detected a FIRE-driven difference in modelled Arctic sea ice retreat.

~~One example is the prevalence of mixed phase clouds with temperature: measurements with the lidar on the Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) satellite show that supercooled liquid occurs at much lower temperatures than simulated in many models (Cesana et al., 2012, 2015). Most CMIP5 models display a strong negative shortwave cloud feedback at mid-to-high~~

latitudes, some of which is related to melting of ice clouds into mixed-phase clouds, which are more effective reflectors of sunlight. The strength of this cooling feedback depends on the initial state, because one in which liquid clouds are already common means there will be less melting in future and therefore a weaker feedback. Controlled CESM1-CAM5 simulations shows differences in equilibrium climate sensitivity of greater than 1 °C per doubling of CO_2 when changing parameters that control these clouds (Tan et al., 2016). Such a large increase in warming would be expected to also change projected sea ice extent.

Furthermore, While we did not explore any dynamic changes in response to the inclusion of FIRE. The magnitude of the radiative effects are a credible candidate for explaining major differences in sea ice extent, with 11 W m^{-2} of downward longwave radiation over a year being sufficient to melt ~1 metre of ice over a yearannually following a simple energy budget equation and assuming that all of the heat goes into the ice (Kay et al., 2008). In reality this thinning is reduced by negative feedbacks, and Figure 4(b,c) show that in CESM1-CAM5 the net result is a thinning of approximately 30 cm of the interior ice pack, For a mean state case this thinning is nonsensical since the ice warms and leaks some of this heat through increased longwave radiation, but it is consistent with our hypothesis of a substantial role forthat FIRE thins the in Arctic ice pack and preconditions it for more rapid melt. Nevertheless, changes in dynamics that affect patterns of cloudiness, ice transport or ocean heat transport could reinforce or counteract our proposed changes and we have not investigated these.

In conclusion, we do not argue that the exclusion of FIRE in current models necessarily means that Arctic sea ice will retreat faster than simulated by the average CMIP5 model. CESM1-CAM5 might show a stronger sea ice response to FIRE than other models or, following inclusion of FIRE that, modellers might tune Inclusion of these effects followed by tuning other processes in a way which may lead to counteractsing processesFIRE-driven sea ice changes. Or a model may have a stronger summertime albedo feedback than longwave radiation-driven thinning effect, and show slower retreat once FIRE are included. However, our controlled experiments show a strong sensitivity of sea ice projections to FIRE in at least one model, with Figure 2(d) showing with September Arctic sea ice retreat being approximately twice as fast once atmospheric CO_2 concentrations are above 2017 levels under 1 % yr^{-1} CO2 growth.

Given that the snow radiative effect exists in reality, we encourage other modelling groups to include them in future cloud schemes to increase confidence in Arctic sea ice projections.

Data availability. The NSIDC [ftp://sidads.colorado.edu/DATASETS/NOAA/G02135/north/monthly/data/], CERES-EBAF [https://ceres.larc.nasa.gov/order_data.php] and CMIP5 data [https://cmip.llnl.gov/cmip5/data_portal.html] are available from public archives. The time series of CMIP5 and CESM1 sea ice and radiative fluxes are appended as supplementary information.

Author contributions. JLL led the research and performed ~~CMIP5 output &~~ the CESM1 sensitivity output processing ~~&~~ and analysis. WLL conducted CESM1 model sensitivity runs. MR performed the CMIP5 processing ~~& analysis~~ and the time series analysis. YHW, ~~&~~ YLH ~~&~~ and JYY provided discussion ~~&~~ and editing. EF, ~~&~~ JJ ~~&~~ and GS supported and offered comments~~/~~ and suggestions to the study. YL quality controlled the data.

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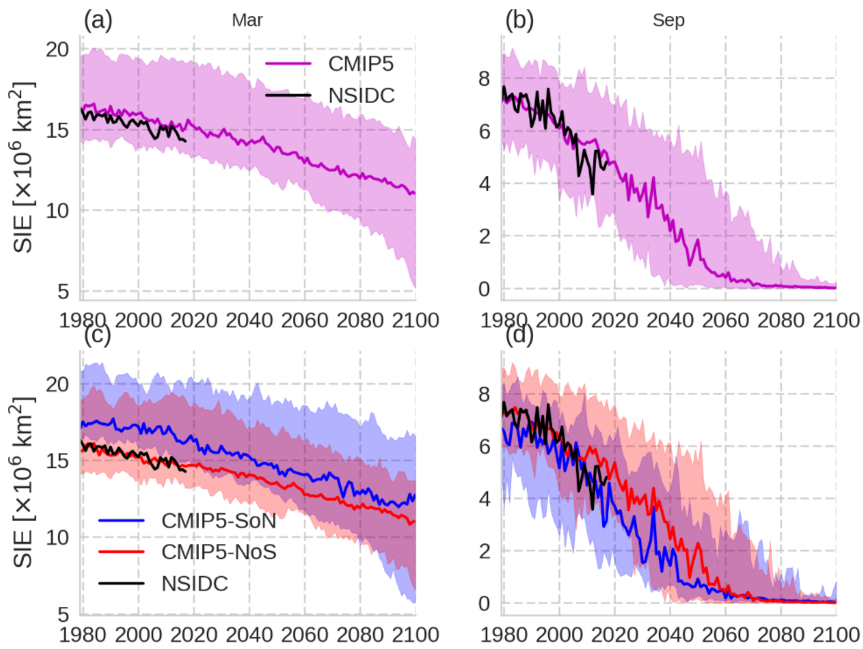

Figure 1: Arctic sea ice extent ~~during March (left) and September (right)~~ in NSIDC observations (black) and CMIP5 climate models (line is ensemble median, shading is~~ed~~ 10—90 % range). (a) full ensemble in March, (b) full ensemble in September, (c) CMIP5 split into sub-ensembles of models with FIRE (CMIP5-SoN) and those without (CMIP5-NoS) in March and (d) SoN and NoS in September.~~The upper row shows the full CMIP5 ensemble. The bottom row shows the ensemble split into those including snow radiative effects (blue) and those excluding snow radiative effects (red).~~

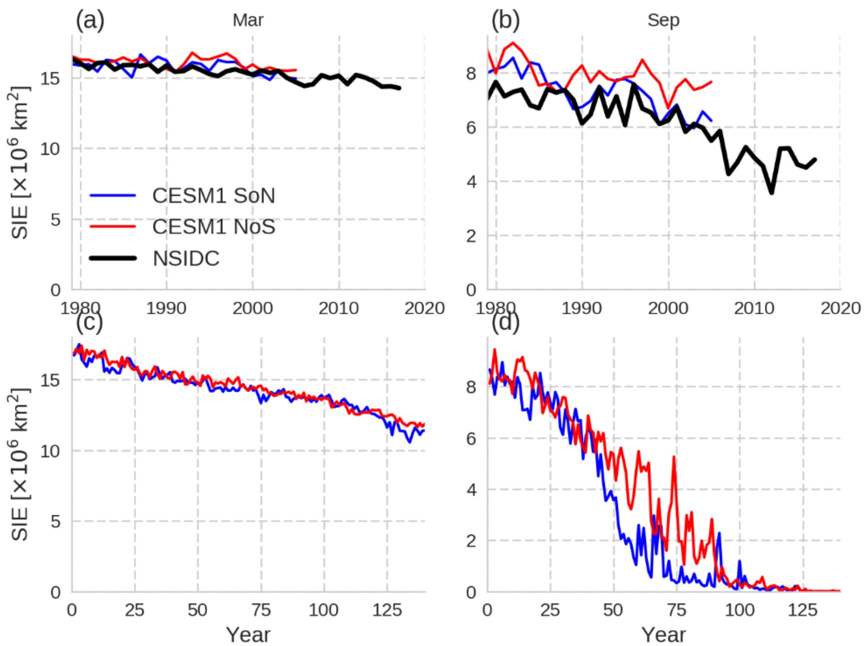

Figure 2: Observed (black) CESM1-CAM5 simulated Arctic sea ice extent in (a) March ~~March~~ in historical, (~~left~~b) ~~and~~ September in historical, (c) March in 1pctCO2 and (d) September in 1pctCO2.~~(right).~~ Blue lines are with snow radiative effects (SoN) and red without (NoS). ~~The upper panels are historical, the lower panels from 1pctCO2.~~

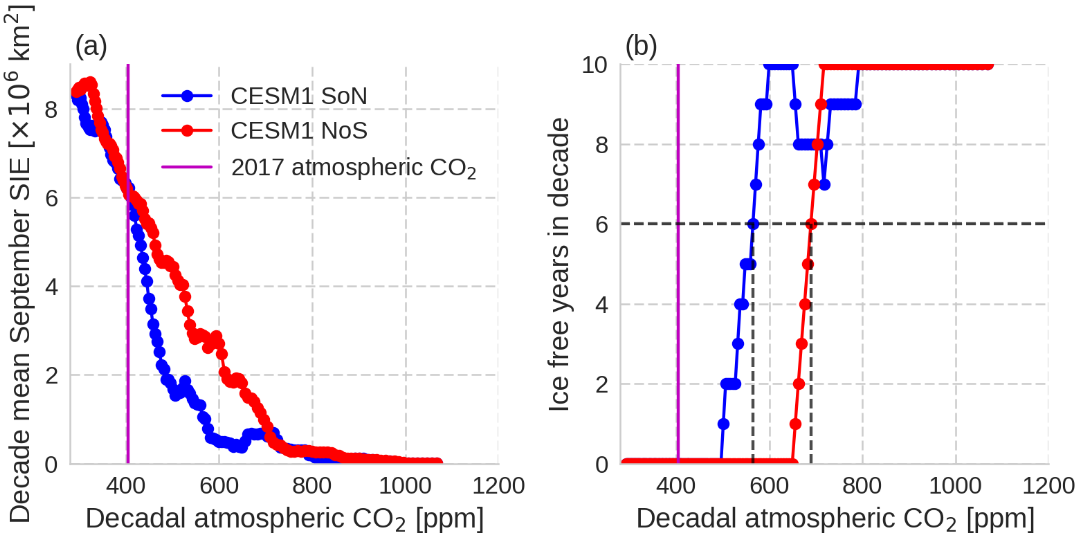

Figure 3: Changes in September Arctic sea ice under 1 % yr^{-1} CO_2 increases for CESM1 SoN (blue) and CESM1 NoS (red) as a function of decade-mean atmospheric CO_2. ~~Left~~ (a) ~~shows~~ the decadal mean sea ice extent and (b) ~~right shows~~ the number of years within that decade for which SIE < 1×10^6 km^2, commonly taken as representative of an ice-free Arctic Ocean basin. The atmospheric CO_2 concentration in 2017 is shown as a vertical ~~black~~ magenta line ~~in each case, but any comparisons must be carefully made since the real world includes changes in non-CO_2 forcing~~ and the dashed lines in (b) identify the decade-mean atmospheric CO_2 level at which the majority of simulated years have SIE < 1×10^6 km^2.~~.~~

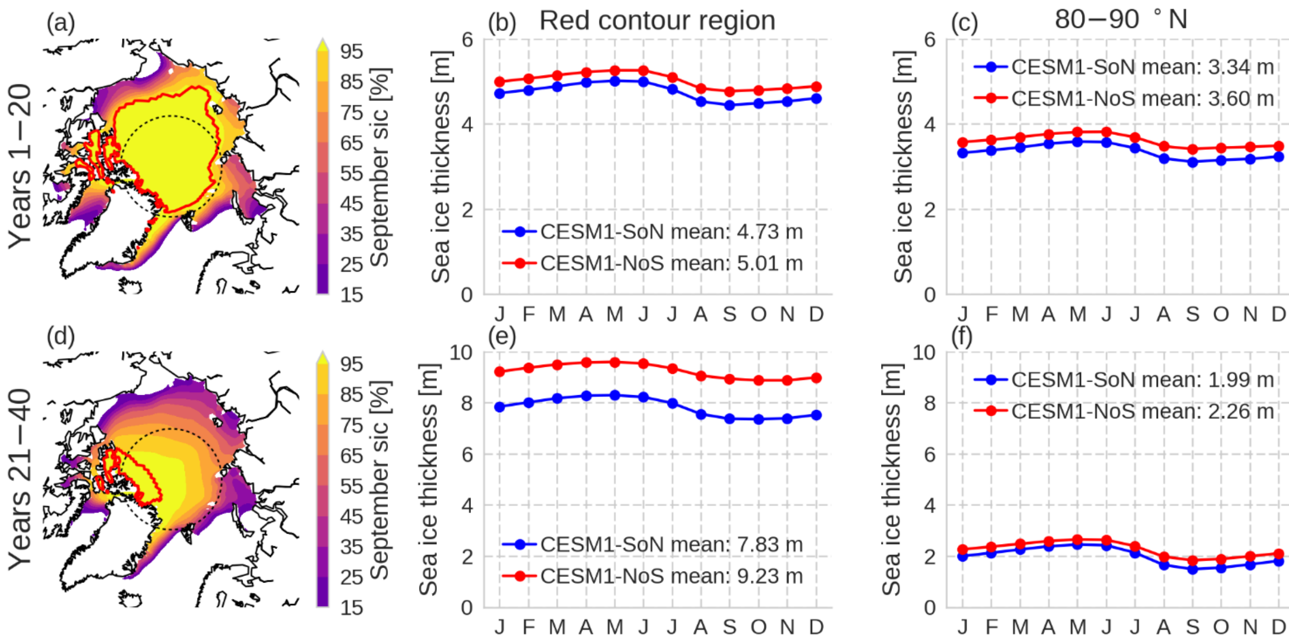

Figure 4: (a) CESM1-SoN September mean sea ice concentration over years 1—20 in 1pct CO2, the black dashed line is 80 °N and the red contour encloses the region within which the mean sea ice concentration exceeds 80 % in all calendar months for both CESM1-SoN and CESM1-NoS. (b) mean thickness within red contour for years 1—20, (c) mean thickness poleward of 80 °N for years 1—20. (d—f) like (a—c) but for years 21—40.

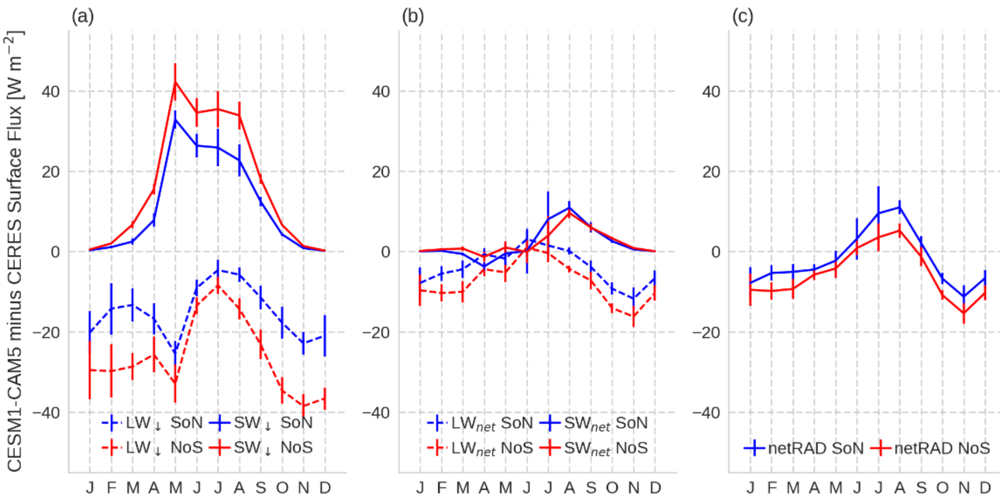

Figure 5: CESM1 minus CERES 60—90 °N ocean differences in mean surface fluxes for each calendar month over 2001—2005. ~~The d~~Differences are shown for both ~~using~~ CESM1-SoN (blue) and CESM1-NoS (red) ~~are both shown,~~ . ~~E~~~~and e~~Error bars are estimates of internal variability only, based on standard deviations of non-overlapping 5-year periods in each series after detrending the annual data~~estimates of uncertainty due to internal variability from selecting the four year overlap period, based on the spread compared with other four-year periods in both CERES (post-2005) and CESM1 (pre-2001)~~. ~~Left panel:~~ (a) ~~differences~~ Differences in downward longwave (dashed) and downward shortwave (solid). ~~Centre panel:~~ (b) difference in net longwave (dashed, positive downward) and net shortwave (solid). ~~Right panel:~~ (c) net downward radiation sum. All values are defined such that positive indicates a case where the model value shows greater net downward flux than the CERES value.

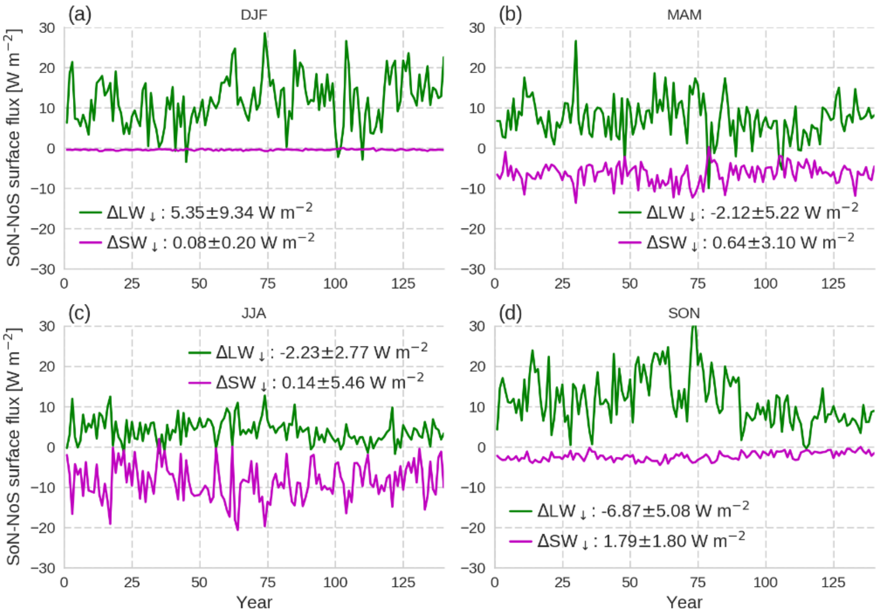

Figure 6: 1pctCO2 CESM1-SoN minus CESM1-NoS season differences in downward surface fluxes over 60—90 °N oceans. The legend reports the estimate of the 140-year change in this difference by multiplying the linear regression trend coefficient by 140, with ±2σ uncertainties. (a) December-January-February, (b) March-April-May, (c) June-July-August and (d) September-October-November.

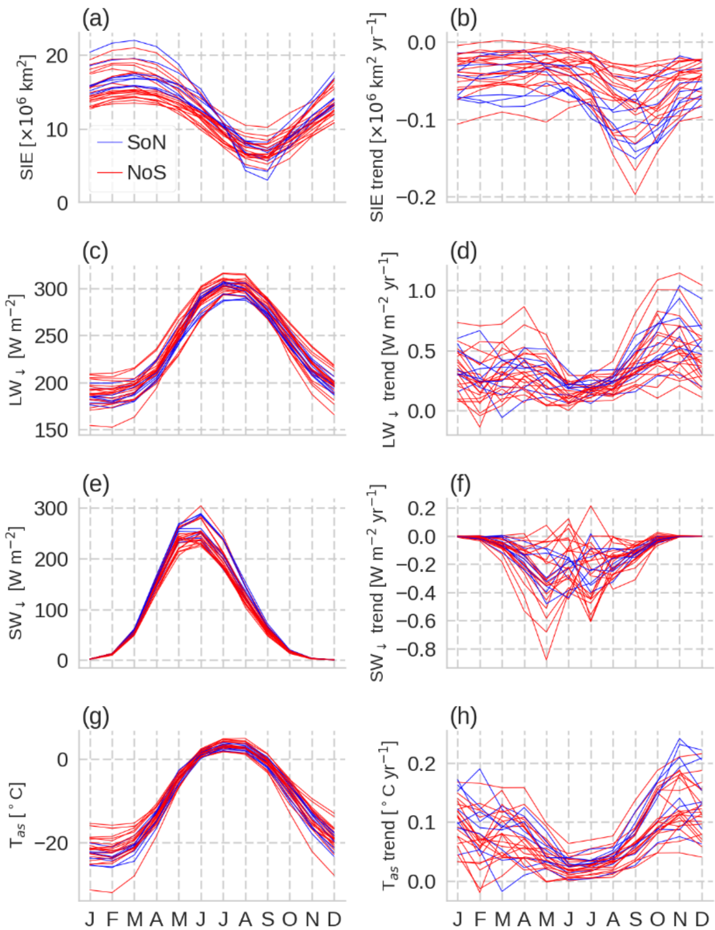

Figure 7: Output over 60—90 °N oceans from individual CMIP5 historical-RCP8.5 simulations according to whether the simulation includes FIRE (blue) or excludes them (red). Left panels show annual cycles of mean properties from 1979—2005 and right panels show the trend for each calendar month over 2006—2035. (a—b) ~~From top to bottom the properties are:~~ sea ice extent ~~(poleward of 30°),~~ , (c—d) downward longwave radiation at the surface, (e—f) downward shortwave radiation at the surface and (g—h) near-surface air temperature.