Review: Diamond et al. The contribution of melt ponds to enhanced Arctic sea-ice melt during the last interglacial.

In this manuscript, Diamond et al conduct analysis of HadGEM3 last interglacial (LIG) and pre-industrial (PI) sea ice conditions, with a specific focus on the importance of melt ponds and the associated albedo-feedbacks for driving the difference in sea ice melt between the two time periods. Their study is important, given the need to understand sea-ice conditions in a previously warmer climate as a tool for predicting future conditions under climate change. Furthermore, their research contributes to the scientific understanding of sea ice simulations in global climate models (GCMs) and providing support for proxy estimates of LIG sea ice conditions. Their motivation for using HadGEM3 is clear and the additional comparison to other models in section 3.3 provides further justification and makes for an interesting discussion. They highlight the relevant research questions and aims at the start of each section, which is useful for a longer manuscript such as this. The structure is useful in guiding the reader through the steps the authors took in determining the importance of melt ponds in their study, but does at times become a little overly long or repetitive. The conclusions are succinct and summarise the manuscript and the main findings well, whilst again highlighting the relevance of the study.

The major concern lies in their reporting of previous studies with some very similar results and lack of 1) citations of such results and 2) discussion of how the two studies agree/disagree. There is some overlap or repetition with the Guarino et al. (2020) publication which is concerning and needs addressing. However, Diamond et al do provide a more thorough investigation into the melt ponds and their characteristics. They just need to make their novel results clearer and state explicitly where their results are not novel. There are also a number of minor concerns and inconsistencies throughout the manuscript, and it would benefit from re-wording and cutting down a number of sentences to make their point clearer. A more thorough explanation of a number of methods would also be useful to aid understanding of figures and results. Finally, a number of changes are required to the figures to ensure consistency. Therefore, I recommend major revisions before this article is published.

Major:

Whilst the Guarino et al (2020) study largely compares HadGEM3 with HadCM3, there are still some results concerning the PI and LIG sea ice differences and melt ponds. Some of your results are identical to those in Guarino et al. (2020), which reduces the novelty/originality aspect of the study. You need to focus on novel aspects of the study, of which there are plenty, and create a discussion section to compare your results with other studies. Where your results have been explicitly presented by Guarino et al., you should remove them, or at the least state that these results are from a different study and cite it. Results which were already presented by Guarino et al. (2020) require a discussion/comparison or citation to show that this isn't the first reporting. Specifical examples include:

Guarino: 'In the LIG simulation, the melt season starts earlier, with a max melt pond fraction reaching in mid-June (not shown)'.

Diamond: 'Maximum pond area at 2.67 million km2 is reached in mid-June'. Line 221. Peak sea ice fraction in LIG in early July, compared to peak in late July for PI. Line 225.

Diamond's discussion of spatial differences and sea ice onset/rate is more detailed than Guarino et al. (2020) so there are novel aspects, but some citing of the previous study is missing.

Guarino: 'This confirms that local thermodynamic processes are responsible for the difference between the two models and that melt pond formation plays a key role in determining how much of the additional TOA SW radiation during the LIG can be absorbed by the surface'. 'LIG TOA rad flux is 60-75Wm2 higher than during the PI in early summer. The crucial aspect is to what extent this increase causes additional melt of sea ice. The substantial increase of surface net short-wave flux (with max value of 70Wm-2 in July) is caused by a decrease of surface albedo.'

Diamond: 'we compare incoming SW radiation between LIG and PI, and use this to quantify to what extent ice-albedo feedbacks modify the surface net SW anomaly shown'. Line 246. 'We have demonstrated that the surface net SW anomaly in Fig. 6(a) was caused by the TOA SW anomaly, significantly amplified by stronger LIG than PI ice-albedo feedbacks' Line 252. 'This demonstrates the significant impact of melt ponds on the surface energy balance, and by extension their key role in enhancing LIG summer sea-ice loss.' Line 273.

Again, Diamond does include much more detailed analysis to support their conclusions, however there is no highlighting that some of these results aren't novel.

Guarino reports the total absence of sea ice in September and discusses the importance of this timing, similar to the results in Diamond.

Guarino: 'clouds over sea ice play little role in determining LIG-PI anomalies in SEB of Arctic.'

Diamond: 'Longwave forcings and feedbacks related to cloud cover are not a dominant contributor to the enhanced LIG sea-ice loss.' Line 173.

Guarino: 'The bias towards thick sea ice does not provide protection...LIG summer sea-ice free state in HadGEM3 takes around 5 model-years to complete.'

Diamond: 'An ice-free summer state is reached after only 4 years... preconditioning does not play the dominant role in enhanced melting of sea-ice during the LIG'. Line 157-159.

Fig 1 in Guarino is similar to Fig 2 in Diamond. March, July and September SIC are plotted for both PI and LIG in both publications. Changing the colour bar alone and including other months is not enough. Perhaps you could include an anomaly plot, so that readers can better determine the similarities and differences between the two periods in all months and not just when there is a large difference.

The Guarino et al. (2020) study is not mentioned at all in the results (which also serves as a discussion, as no separate discussion is provided), except for Figure 6 caption.

I suggest that you include a discussion. Section 3.3 could also be included in the discussion.

Minor:

Throughout

Check your tenses throughout the manuscript. Typically, when talking about previous studies or work of others, past tense is used. Some sentences have a combination of past and present. Similarly, be careful of words like 'remarkable', 'simple' etc. These are too subjective.

Introduction

You need to include some connection between the motivation for using HadGEM3 and the melt pond section. Until line 54, you didn't mention the importance of melt ponds or whether anyone else has researched their impact on sea ice loss in GCMs, despite this being

the most crucial aspect of your motivation. I would link melt ponds into the section where you state the mechanisms driving Arctic sea ice change.

Ln 55: Melt ponds 'seem to be critical' for sea ice loss simulations. What evidence do you have of this? As this is partly the motivation for your study, it needs to be clearer. Why did you decide to investigate the impacts of melt ponds?

Ln 68-74: This paragraph is simply a list of previous findings, but with no context or link to your study. I suggest you remove the first few sentences and focus on which melt pond models are included in GCMs, specifically HadGEM3, and simply say that the development of such models comes from a series of studies and improvements. Then simply cite the relevant studies.

Methods:

How is sea ice treated or represented in the PI and LIG simulations? You point the reader to the Ridley et al. paper, but as sea ice is a crucial aspect of your publication, a short paragraph describing some of the key characteristics is required. For instance, melt pond information from which thickness category of sea ice is used? Melt pond fraction and depth are calculated separately for each ice thickness category (Ridley et al. 2018b)

You use the low resolution HadGEM3. How does the model resolution potentially influence your results, especially when looking into melt ponds which are considerably smaller?

I suggest that you move more of your description throughout the results into the methods. For example, include definitions of 'ice covered' and 'open water' grid cells, as these are used throughout.

Results:

Ln 132: Could you show the sea ice thickness, perhaps as a panel next to sea ice area. You mention sea ice thickness at least 4 or 5 times throughout the manuscript, so it would aid the readers understanding.

Ln 142-144: Sentence not clear. Specifically, which season you are talking about.

Ln 145: Your motivation is wanting to know what causes the spring melting. You mention ocean, clouds, albedo, wind, ice distribution- but don't mention melt ponds, despite the introduction and title. Similarly, you don't do any analysis related to ocean heat transport, but only discuss previous studies (line 158-161), so including ocean heat transport in line 145 onwards is misleading. As ocean heat transport is not important nor a result that you found, I would move this to either the introduction or discussion.

Ln 153 onwards: You state that you don't use the spin up years in your methods (Ln 109), but here you look at them. You need to make it clearer in your methods that you don't discard them entirely.

Ln 157: How does the August decline show that preconditioning isn't playing a role? Is this winter preconditioning that you are talking about, as in line 148? In which case, should you not focus on the winter conditions during spin up?

Ln 176: change to 'This suggests..'. You are concluding quite strongly that thermodynamic processes are predominantly from SW anomalies, but you are looking at spin-up and only 1 year. Which I wouldn't say is totally conclusive.

Ln 187: Are ice volume tendencies split by dynamics and thermodynamics directly output by HadGEM3? Or have you calculated these and how? This should be made clear here, and if necessary, included in the methods.

Ln 213: what do you mean by distant from sea ice edge? Aren't areas south of 70°N closer to the sea ice edge?

Ln 219: what do you mean by stronger? Is the rate of change faster? Stronger isn't the right word here.

Ln 236: Where did you get these albedo values from? In the introduction you provide a range for melt ponds from 0.1-0.5, so why use 0.27?

Ln 253: You don't discuss the ice-albedo feedbacks in the above paragraph. You talk about the anomalies in downwelling and total absorbed SW, but you don't separate the total absorbed SW into ice-albedo feedbacks. Are you including absorption by ocean and melt ponds into this statement?

Ln255 section: Are there other things that might have changed the albedo- such as snowfall changes? I am not suggesting that you look into them, but is it possible that albedo changes on the sea ice are not only related to melt ponds?

Ln 288: explain what is meant by thin-ice fraction here.

Ln 285 to 292: this section should be made shorter and clearer, there's a lot of repetition.

Ln 294-300: lots of new acronyms. Are they all necessary? If they're only used in this section, just write them out.

Ln 295-300: There is no discussion of the April corelations with thin-ice being statistically relevant. I would also mention the importance of this variable for prediction, along with where you state the difference in April PI and LIG. I think you try to explain this in line 298-300, but it is not clear, as despite the difference in SIA and thickness between the two time periods, the April correlation is similar in both periods. Perhaps a summary sentence is needed here too- do you see the predictability of SIA from melt ponds changing as the climate warms?

Ln 311: SIE not explained previously, and only used in this section- write out the abbreviation.

Ln 313: What are the other models? Or link to a paper which explains the CMIP6-PMIP4 experiment so readers can see all model options.

Ln 325: melt pond schemes should 'matter' all of the time if we are attempting to simulate the most accurate conditions. I would rephrase this sentence.

Ln 336 (and in other locations): if the grid cell is classed as 'ice covered', how is open water involved? How do you calculate open water? Just where sea ice is less than 0.15? Why is there a difference in open water absorption between the two periods? If the same sea ice area is used? If necessary, I would include some explanation in the methods.

Ln 348: I would say May-June melt pond area is a good predictor, as April isn't.

Ln 350: watch your tenses here and throughout the manuscript. LIG is in the past, as is the simulation, and condense. 'which has ice that is thinner' could be 'which had thinner ice and was thus…'

Figures:

There seems to be inconsistency in your evaluations. Figure 10 is first 50 years of simulations- but in other areas you use 200 years. Why? Figure 12 is 200-year average. Figure 13: 50 years again.

Figure 5: The legend covers too much of the results from PI.

Figure 6: no a and b labels. Caption is not clear. Reword to: Anomalies (LIG-PI) of the components of the surface energy budget from a) the LIG simulation (adapted..) and b) first year of LIG spin-up. What does 'anomalies are found from long-term means' mean? If figure b is the first year of spin up, what long-term mean can be used? Again, the label is covering your results.

Figure 10: What is your scale in Figure 10? Day of the year? So 90 refers to approx. start of April? I would include some more information in the label or caption.

Figure 12: change to c) shows melt pond fraction of sea ice.

Figure 13 c: What is meant by 'up to this day'? The caption and label are not clear here. Alter y axis labels on a and b to put units in brackets, to be consistent with figure 13c. Figure c needs more ticks and labels for x axis, as you talk about specific dates in the text, which are currently hard to see.

Figure 14: Does ASO mean august, September, October? Only the red square is April, May, June, and the other symbols are August, September, October? Or I the 'for comparison' sentence only for reference and not actually plotted? In that case, remove it from the caption, or add panels to show this. Similarly, as there is plenty of room, label your x axis with the months rather than letters. This would be consistent with figure 13 and others. Full stop missing from last sentence. Explain the three dotted lines in the caption.

Technical:

Ln 27+30: 'find' should be 'found'

Ln 29: 'explore' should be 'explored'

Ln 32-33: reword sentence 'This makes it difficult to determine the mechanisms or distribution of sea ice loss during the LIG from these preserved biological data.'

Ln 46: remove 'however', it is not needed with a sentence starting with 'whilst'

Ln 50: remove 'apparently'

Ln 59: remove extra parenthesis

Ln 96: Above you say CICE5.1 by Hunke et al. 2015 is used, but here it is just called CICE. Is there a difference? If not, include 'hereafter called CICE' in line 87.

Ln 109: Documentation from UKESM states 615 years of spin up, not 700 for HadGEM3 GC3.1 N96ORCA1 (https://ukesm.ac.uk/cmip6-simulations-hadgem3-gc3-1/)

Ln 134: include 'for temperature and salinity respectively' after 1-2ppt.

Ln 134: SST not yet explained.

Ln 141: citation for this?

Ln 141: SSS not explained. This is the only time you use this abbreviation, so I would just use the full words, as you have many abbreviations already in use.

Ln 141: Combine the two sentences: 'Thus, it is additionally useful to consider mean sea surface salinity changes (Fig. 4), which have a clearer signature than SST.'

Ln 145: reword 'we of course wish to know' to 'we aim to understand'

Ln 151: reword to 'The process which is most significant for the simulated enhanced LIG sea ice loss, is currently unknown'.

Ln 169: bracket around a not needed in Figure citation.

Ln 174: include 'likely' or 'possibly' in this sentence, as you have only shown they are not important in the spin-up, not for all sea-ice processes.

Ln 208: 'that' should be 'than'

Ln 209: reword for clarity. Only after a number of reads through this section did I understand this point. Perhaps to: 'As melt onset is not provided as a variable by the models, we use the mean first day each grid-cell has pond fraction greater than 1% as a proxy for this'.

Ln 217: remove simply.

Ln 220: Make clear that this sentence refers to LIG conditions

Ln 247: 'Outlined in 1'- what does that mean? What is 1?

Ln 268: you shouldn't be computing something from a figure… perhaps highlighted or presented?

Ln 270: missing full stop after (c).

Ln 328/341: Lead should be led.

Ln 332: re-amplifies should be amplifies