General comments

Alvarez-Solas et al. investigate the behaviour of the Eurasian Ice Sheet (EIS) during the Last Glacial Period (LGP), with a particular focus on the Marine Isotope Stage 3 (MIS3). They run a hybrid 3-D ice sheet model with explicit grounding line treatment and calving, using an offline climate forcing which separates orbital- and millennial-scale climate variability. After an initial control simulation providing the initial state for MIS3, the authors carry out a number of transient model experiments comparing the relative importance of the atmosphere and the ocean in driving ice sheet change over the MIS3 period. Particular attention is paid to the atmospheric and oceanic role in forcing ice sheet change during transitions from stadial (cold) to interstadial (warm) periods. Separate experiments are also carried out for how temperature changes in the surface and subsurface ocean affect the EIS.

The authors find a highly dynamic EIS during the LGP, and that ocean forcing dominates ice sheet mass loss and associated sea level rise during stadial-to-interstadial transitions. The imposed ocean forcing is able to force large-scale, abrupt grounding line retreat and associated high rates of ice discharge into the ocean. Conversely, atmospheric forcing (surface ablation) is not found to have a strong effect except in localized sectors, contributing little to overall ice discharge during abrupt climate transitions. They further find that temperature change in the surface ocean induces a stronger ice dynamic response in the ice sheet model than does the subsurface ocean, and that these change occur in an out-of-phase manner. They therefore suggest that ocean surface warming is the most effective forcing of EIS change during MIS3 stadial-to-interstadial transitions. Based on this and previous work (Alvarez-Solas et al. 2013), they argue that ocean-ice sheet interactions can account for "virtually all ice rafting events in the North Atlantic" during MIS3, as manifested in IRD records by Heinrich events from the Laurentide Ice Sheet during stadials, and by ice discharge from the EIS during interstadials.

Ice sheets are regarded as key players during abrupt climate change, but the underlying mechanisms, roles of oceanic versus atmospheric forcing, and involved ice sheet dynamics is far from resolved, as the authors rightly point out. This study is therefore a timely and exciting contribution to the community. The directed focus on MIS3 rather than the entire LGP allows for a more detailed comparison between different forcing, as well as some analysis of the transient ice dynamics using a 3-D ice sheet model, albeit with the model's inherent limitations in parameterizations and spatial resolution (see below). The manuscript is generally well written and nicely illustrated with figures. Some improvements can be made on the structure of the Results and Discussion sections since these are a little hard to follow, perhaps separating at least the Results into different subsections.

While the ice sheet model dynamics used is fairly standard (hybrid SSA-SIA), the way climate forcing is implemented is more novel (albeit offline). Further, applying the idea of the EIS as a contributor to the North Atlantic IRD record in the framework of a dynamic, transient ice sheet model has not been done in this manner before. The study tests relative contributions of ocean and atmospheric forcing, and further subdivides ocean forcing into surface and subsurface changes, which has not been done for the MIS3 and for the EIS.

Overall I am positive to the scientific focus and scope of the manuscript. I do however have a few major and a number of minor concerns that I'd like to see addressed. My concerns are mostly related to an incomplete description of the model dynamics and setup, and the need for a discussion of related uncertainties. I would like to point out that the authors should be able to address most of these concerns without the need for additional model simulations.

More substantial comments

Grid resolution and grounding line treatment

Given the conclusion that the ocean plays a major role during abrupt ice sheet changes, the model treatment of grounding line dynamics is key. Several studies have shown that for many applications, a resolution of around 1 km often is needed to accurately capture grounding line migration. In addition, it has been shown (e.g. Gladstone et al 2017) that grounding line behaviour is sensitive to the choice of friction law and the physics of subshelf-melting.

Now, given the millennial-scale focus and large spatial scales involved in this study, I suspect that computational constraints do not allow for ice sheet flow to be resolved on such fine spatial scales, especially not with a 3-D finite difference model. Still, since changes to the marine boundary is an integral part of your conclusion, I feel that this point should be acknowledged and discussed in more detail; namely how your relatively coarse model resolution (40 km) affect your findings regarding the key role of the ocean and grounding line dynamics? Particular in light of Figure 9 where grounding line retreat is assessed in more detail.

The aims of the study are clearly described (response of the EIS to millennial-scale climate variability during MIS 3; ice sheet response to atmosphere vs ocean in abrupt glacial climate change). However, since quite some attention is given to stadial-to-interstadial conditions (i.e. abrupt glacial climate change), why not assess one specific DO event (for example DO 12 c. 47 ka, as shown in Figure 6) in more detail? One could for example do twin experiments over a particular DO event, with increased model grid resolution, to really pin down the conditions and dynamics involved, and assessing the uncertainty to model grid resolution in the process.

Calving

To me it's not entirely clear how calving is treated. Perhaps a naïve question, but since your grid resolution is 40 km, does this mean that blocks of ice 40x40 km are calved at once? If so, does this affect the ice dynamics in certain regions? Also, do you expect the model to be sensitive to the shelf thickness threshold H_calv you use? For example, in Banderas et al. 2018, where the climate forcing method used in the present study is explored, the same ice sheet model was used and H_calv = 200 m is employed. In contrast, the current study uses 150 m. Perhaps also give some references to the observational and/or theoretical basis of using such a threshold.

To be clear, I do not suggest you to switch to another calving law; they all have their inherent flaws and uncertain parameters, especially for paleo-applications. Still, we know that model behaviour differs with the choice of calving law, so I think a more detailed discussion is warranted, also since calving is a key element of the EIS ice discharge that supposedly produces IRD during the modelled period MIS3. Also see my comment on p4, l13-17 below.

Sensitivity to atmospheric forcing

The sensitivity to the PDD parameters are tested thoroughly as shown in the Supplementary. Though it's becoming increasingly outdated, I can accept the use of PDD in this study. However, I'm missing some discussion regarding the underlying assumptions of the PDD model. In light of your aims and experimental setup, what's the rationale for using PDD, and not another parameterization, for example including changes in insolation (e.g. Robinson and Goelzer, 2014)? Would the use of PDD put any biases to the SMB fields? If so, in what regions? How would this influence mass loss and would it change your conclusion regarding the ocean vs the atmosphere? I suspect it won't but if this is what you expect, this should nevertheless be pointed out.

Sensitivity to ocean forcing

You find that the ocean has an important role in rapid ice sheet changes, and suggest based on your comparison of OCNsrf and OCNsub that surface ocean temperature is a more important driver than subsurface temperature. This appears at first glance counterintuitive, given present-day evidence from Greenland and Antarctica, where warming subsurface waters are regarded most important, since subsurface waters reach grounding lines and induce basal melting, and the properties (temp, salinity) of these subsurface waters would therefore control mass loss from basal melt, as you also point out in p.6, l.8-9. Now, if I understand your model setup correctly, you are not comparing the effect of **concurrent** surface **warming** with subsurface **warming**, but surface **warming** with subsurface **cooling** (opposite sign of anomalies in Fig. 2c and d) in your experiments OCNsrf and OCNsub. Even so, you do get much smaller amplitude ice volume changes (except c. 44 ka) with subsurface warming than with surface warming, and out-of-phase ice volume variations, as nicely illustrated in Fig. 4. I think this could be made more clear.

An explanation for the stronger response for OCNsrf than OCNsub is presented (ice sheet configuration with extensive shallow grounding lines more sensitive to surface than subsurface warming) but relies heavily on the model representation of grounding line dynamics, basal melting and model resolution along marine margins. You touch on these aspects in p. 10, l.29-34, but I think your finding that the experiment OCNsrf gives higher amplitude changes for the EIS than OCNsub would need to be explained and discussed further.

I agree with you that detailed assessments of the mechanisms of abrupt climate change is beyond the scope of this paper (as you point out at the end of the Discussion), and perhaps requires online coupled climate-ice sheet models. Nevertheless, I think you could briefly mention (in Discussion) what potential implications your found ocean-dominated regime have for abrupt climate change in general and MIS3 in particular.

You have nicely illustrated that whether the surface or subsurface dominates may be a question of the ice sheet configuration (e.g. p12, l20-22). Not only that, but you have attempted to link the rate of temperature change (e.g. p10, l29-30) to the question whether surface or subsurface ocean heat matter for the ice sheet, and also compared the impact on different regions. These are exciting findings and could be made even more visible than in the present manuscript. In this aspect, a more in-depth discussion of how you represent grounding line dynamics (see above) and basal melt (see specific comments) seem all the more important.

Contribution from different sectors

The role of grounding line retreat and associated dynamic mass loss from Bjørnøyrenna ice stream is highlighted, along with a description of changes in other sectors (e.g. p9, l30-35). Perhaps some rough numbers could be given for mass fluxes for the different sectors. This would also be helpful for both future model and observational studies building on your study. See detailed comment in Results below.

Ice sheets' role in abrupt climate events

Are the time scales of modelled ice sheet change correct for the D-O type abrupt events? (decades from cold to warm). Does the ice sheet change fast enough in your model? Perhaps briefly comment on this in the Discussion.

Specific comments (mostly minor)

Title

The title is fine, but I'm not sure it gives enough credit to your finding that ocean forcing drives EIS change during MIS3. As it stands, the title could be interpreted as a study which only tests the influence of the ocean on the EIS (which I assume is not what you want). Also as it stands, we have no idea that this is a model study, but including this is personal preference.

Abstract

l8. Unclear what "its" refers to

l12. "provides a more realistic treatment of millennial-scale climatic variability than conventional methods" Not clear from the context what conventional methods you refer to here, and therefore why your model approach therefore is "novel"? Try to very briefly clarify this.

Introduction

p2, l.10. "its" – awkward phrasing given that you talk about both LIS and EIS in previous sentence

p.2, l19. Please state and provide a reference for why BKSIS is "often considered an analog" for the current WAIS.

p.2, l20-21. "Understanding the underlying mechanisms" **[of what?]** would provide insight into future evolution of the WAIS?

p.2, l23-24. This is true and important, but not unique for the EIS – other ice sheets advancing during the LGM would also have destroyed older evidence. Please rephrase.

p.2, l26. A detail but Finland would perhaps not be considered western Scandinavia, rather use just "Scandinavia".

p.2, l31. "The results" – imprecise wording; what results are you referring to?

p.2, l32-33. high co-variability of the BIIS volume, extent, ice discharge? Not clear what property of the BIIS that co-vary with ocean SSTs, without looking into the underlying reference.

p2-3, l35-1. Please specify that it is sediment cores/records that you refer to here.
p.3, l1. …was identified in [**records from**] the Irminger Sea…
p.3, l4. "as well" – awkward wording
p.3, l5 "just before interstadial transition" – do you mean "just before stadial-to-interstadial transitions"?
p.3, l17. part of **the** LGP. (missing "the"). Also a bit vague, maybe specify which part of the LGP that was modelled in detail in this study.
p3, l15-19. Recent studies by Patton et al 2017 QSR and Åkesson et al 2018 QSR may also be relevant in this literature overview (cf. l23-24 and l28-30).
p3, l20-23. Bassis et al 2017 Nature perhaps relevant.
p3, l.33. Nice overview of the paper – but what's in Section 4?

Model and experimental setup
p4, l5-6. Please mention briefly what the underlying assumptions of the SIA and SSA are. Any modeller will know this, but non-modellers might need a reminder.
p4, l7-8. Given the importance and uncertainty of basal drag on ice sheet dynamics, I think it would be helpful to briefly elaborate on how you represent basal drag and what the underlying assumptions are, e.g. type of sliding law, any non-linearity, treatment of sediments if any, spatial distribution of basal drag coefficient, if used, etc.
p4, l11-12. Are these arbitrary numbers or do they have some physical meaning? The criterion where you "activate" SSA could have an impact on your modelled ice velocities, grounding line retreat and ice discharge and should therefore be discussed.
p4, l14. criterium -> criterion
p4, l14. "its thickness" slightly awkward here; use "shelf thickness" to be precise
p4, l15. Please provide a reference for "typical thickness of observed ice-shelf fronts"
p4, l13-17. Please explain briefly the rationale behind using this double criterion, as opposed to, for example, a single ice thickness criterion, or using the Levermann calving law on its own. Also, what happens if there is no shelf in the model (e.g. vertical calving face) – is the calving rate in that case zero? What happens then to the basal melt rate? Given that many vertical termini we know from the present-day are grounded in fjords several hundred meters deep, the thickness criterion would not be reached in this case. Would this have any effect on EIS evolution? (you may include part of this in the Discussion if you wish to keep the model description short)
p4, l22. I know that your focus is not Greenland so this would not affect your conclusions at all, but I don't see the advantage of using the Bamber dataset from 2001, when more recent, more accurate datasets are available (e.g. Bamber et al 2013, Morlighem et al 2014; 2017). On this note, you do include Greenland in your model domain, which I think is indeed interesting and could've been a paper on its own. However, the modelled evolution of the Greenland Ice Sheet is not mentioned in the paper, except being shown in Fig. 1 and 6 and in the supplementary animation. What's the rationale of including Greenland, when the focus of the paper is the EIS? Is there a scientific motive or just a technical reason?
p4, l33. "inland" – would rather use "for grounded ice"
p5, l2. the abbreviation SMB has not been introduced yet (should be done at p4, l22)

Misc. regarding the model

- Please provide the model time step, both for ice flow and PDD. A table of model and forcing parameters along with their values/ranges would be useful.

- You mention that GRISLI-UCM is a thermomechanical model (p4, l4), but I can't find any information of the thermal part of model. Are thermomechanical feedbacks involved over the millennial time scales you focus on?
- Is Glacial Isostatic Adjustment included in the model, and if so, how is it accounted for?
- How is the calving rate defined (as plotted in Fig. 5) and how do you separate this from direct basal melt (also in Fig. 5)?

p5, l19-24. Are you here describing characteristics of the CLIMBER modelled climate in the North Atlantic, or reconstructions, or a combination? Please clarify.
p5, l4. parts -> ice
p5, l14-15. Note sure what you mean here by "purely floating ice shelves", please clarify.
p5, l15-19. You assume 10 times lower melt for "purely floating ice shelves" than at the grounding zone (what do you consider as the "grounding zone"?) and justify this with qualitative agreement with "some Greenland glaciers". OK, but given your previously claimed analogue between the Bjørnøyrenna basin (where most of the action in your model happens) and the WAIS, would it be more appropriate to compare your imposed melt rates with Antarctic melt rates? Also gamma = 0.1 seems a bit arbitrary as it stands; did you explore any other values for gamma and found 0.1 to be the "best" one, or did you settle on this directly based on present-day observations in the studies you cite? Note also that the cited Rignot and Jacobs (2002) covers basal melt in Antarctica, and not Greenland. In addition, the studies cited are for Greenland glaciers with floating tongues (Petermann and 79N), which is indeed more relevant than if you were referring to glaciers in Greenland with grounded termini; this should be mentioned.

p7, l14. Great that you're comparing with ice sheet reconstructions. I know that you're not trying to fit the model perfectly to reconstructions but rather to investigate the relative roles of forcings. Personally, I think that aggressive tuning of climate and model parameters to (over)fit the data perfectly will weaken the value of this kind of study, so I applaud you for not going too much down this route. Still, for transparency and to assess your slightly vague "to an extent that satisfactorily agrees with previous reconstructions", I think including a figure comparing with one or two ice sheet reconstructions (e.g. DATED-1 and ICE-5G) would be valuable. Perhaps you could add these reconstructed ice sheet margins in Fig. 1, or if this becomes too messy, add another figure.

p8, l5. applying (missing p)
p8, l10. You give a nice overview of your experiments. Would also be valuable with a table summarizing the experiments and their differences for easy reference (control run, constant vs time-varying atm forcing, surface vs subsurface ocean, sea level etc.)
p8, l13. Please specify that it is refreezing under ice shelves you're talking about here, since you do include refreezing in your SMB model.

Results
First off, I think this section would benefit from division into subsections.

p8, l22. "internal ice-sheet variability" – what is exactly in the ice sheet causes this internal variability?

p8, l23. “slight response” – please more specific, how many % variability or ice volume/sea level equivalent? Is this subdued response to sea level forcing what you expect, or surprising (you may link this to previous literature in the discussion)? Do you think your coarse model resolution dampens the response, making it “harder” for grounding lines to retreat, but once they retreat, the response is large since you “instantaneously” remove a big 40x40 km chunk of ice? Or is it something inherent to the sea level forcing? Is the subdued response to sea level the same everywhere, or does sea level forcing induce grounding line retreat in some sectors, related to the particular ice sheet configuration (e.g. deep vs shallow grounding lines)?
p8, l30. This is an exciting result. The anti-phase relationship is not perfectly in phase throughout the LGP, which perhaps should be mentioned. Given that your SST and subsurface anomalies (Fig. 2cd) are of opposite sign, though not with same spatial distribution, I don’t think it’s too surprising that the ice sheet responds in this anti-phase manner. Still I do think it’s in interesting result with relevance both for abrupt climate change during the LGP and for present-day/future, but it requires a more thorough discussion. See also major comment above on ocean forcing.
p9, l5-13. Please check the manuscript to be consistent with the use of yr^{-1} and a^{-1} (as used at p7, l11).
p9, l20. mid panel -> b
p9, l20-35. A very interesting and nice paragraph where you break down EIS change into sectors and try to explain why. I think an additional figure (if feasible) showing ice volume through time for the different sectors you refer to (e.g. SW vs NE) for one or two forcings (for example ALLsub and ALLsrf), would be of great interest and also illustrate the spatial contrasts and their relation to the forcing you outline.
p10, l23. …are representative of [**the ice sheet response**] during all other stadial-to-interstadial transitions.

p11, l8-12. Great that you’re trying to quantify the grounding line retreat, I think this analysis strengthens the paper. Firstly, over what “fixed area” (line 11) do you define mikro? Is it the square highlighting the Bjørnøyrenna basin shown in Fig. 1c? Secondly, your definition of mikro appears to represent the percentage of non-grounded grid points in the Bjørnøyrenna basin, so that increasing mikro (more non-grounded grid points) corresponds to grounding line retreat. While there is nothing formally wrong with this definition, I wonder if it would be clearer to just use the grounded ice sheet area as your metric for grounding line retreat. Grounded ice area could be shown in Fig. 9 on two different y-axis, one in (%) and one in (km^2). See also comments below on Fig 9.

p11, l18-19. I think this an interesting point. For your experiment OCNsrf, you’ve found a quite close relation between ice thickness H and the number of non-grounded points in the Bjørnøyrenna basin (right panel in Fig. 9). Is this the same as saying that the grounded area and ice thickness in this basin scales linearly? I.e. that the more extensive grounding line retreat (higher mikro), the thinner ice sheet (lower H)? And conversely, a thickening ice sheet translates linearly into grounding line advance? Is this what we expect? Does this mean that ice sheet thinning and grounding line retreat occurs more or less at the same rate, i.e. are tightly coupled? There is also an “anomalous” branch of your H vs mikro plot, where grounding line retreat and thickness temporarily are decoupled. What stage of ice sheet change is this (stadial or interstadial)? What occurs first, grounding line retreat or thinning? Is this what you expect, or counterintuitive? Just adding a brief discussion on this would be relevant both for both paleo-ice sheet changes and people working with present-day changes in Greenland and Antarctica.

A related line or two about why v and mikro do not follow such close relationship would also improve the manuscript.

Discussion

p12, l2. “some authors” - need reference

p12. l2-3. I would like to congratulate the authors by making the link between the EIS during the LGP and the present-day/future of contemporary ice sheets. However, it’s not entirely clear to me from this paragraph whether the authors’ findings support or contradict the Kara-Barents complex as a “WAIS analogue”. Here I think the relevance of the EIS for present/future changes of Greenland/Antarctica could be strengthened.

p12, l23. grounding line**s**

p12, l23-31. A very important paragraph where the authors outline uncertainties associated with linking calving (flux) and IRD. These uncertainties are outlined nicely, but presently they are not discussed in light of the findings in this study. I also feel that this paragraph would benefit from one or two additional references.

p13, l4. regarding initial ice sheet size – how does your initial ice sheet state entering MIS3 affect subsequent evolution? I don’t expect any new simulations in this regard but a brief comment what you expect, particularly since you tuned your basal melt rates at 40 ka to obtain an ice sheet in reasonable agreement with reconstructions.

p13, l14. Rignot et al. 2002 -> Rignot and Jacobs, 2002. See also comment above (Section 2.2) on justifying your magnitudes of basal melt against data from Antarctica vs Greenland.

Conclusions

Well written. Consider including your finding about surface vs subsurface ocean. A brief statement on uncertainties in ice sheet dynamics/grounding line dynamics could also be included. I think you may also mention that you explicitly include calving in your model, and very briefly how oceanic basal melt is parameterized.

Misc.

- check consistency of Bjørnøyrenna vs Bjørnøyrenn throughout text and figure captions.

Figures

Generally nice and clear figures. Some panels within the figures are missing abcd labels (Fig. 2, 7, 8, 9). To help the reader, make sure you make according changes in places within the text where you refer to different panels of these figures.

Figure 6. previous -> prior.

Figure 7b. ice velocities in the Bjørnøyrenna basin – how are these defined? Mean velocities over the entire basin? (same in Fig 8b)

Figure 7c. I like that you plot the calving rate in (Sv) for oceanographic relevance – also consider adding a second axis in mass loss per year (Gt/a) for the glaciologists reading this. (same in 8c)

Figure 7d. ice shelf extension – would rather use “ice shelf area” to emphasize you’re showing area, not length. Check in text to be consistent. (same in Fig 8d)

Figure 9. A nicely plotted interesting figure. I would put "grounding line index \mikro (%)" as ylabel instead of just mikro (%) to help the reader, unless you follow my suggestion above to use the grounded area as a metric instead. In the caption, please also cross-reference where in the text the index mikro(t) is defined (Eq. 18). For ice thickness H, is this the mean ice thickness in the square shown in Fig. 1c? Ice stream velocities v, over what region are they defined? Finally, I would label this figure with abc, to more clearly refer to each panel in the text (e.g. p11, l13-21).

Supplementary
Fig. S1 and S2. Though it should be obvious to most readers, please spell out "S.l." in the yaxis label, as you've done in Fig. 3.

Animation. Should the units of time in the animation be changed ka -> a?
Also, unless I'm misinterpreting something, the model seems completely off when it comes to getting rid of ice in the Holocene (see screen dump from your animations below). You're modelling the evolution all the way to the present-day but northern Europe is still under ice in your model at 0.0 ka BP, so is northern Russia. Do you have an idea why? I know this is not the period you focus on, but people seeing the animation may take this large disagreement as a sign of something completely missing in your model. Given the severe mismatch, I think an explanation should be included in the manuscript.

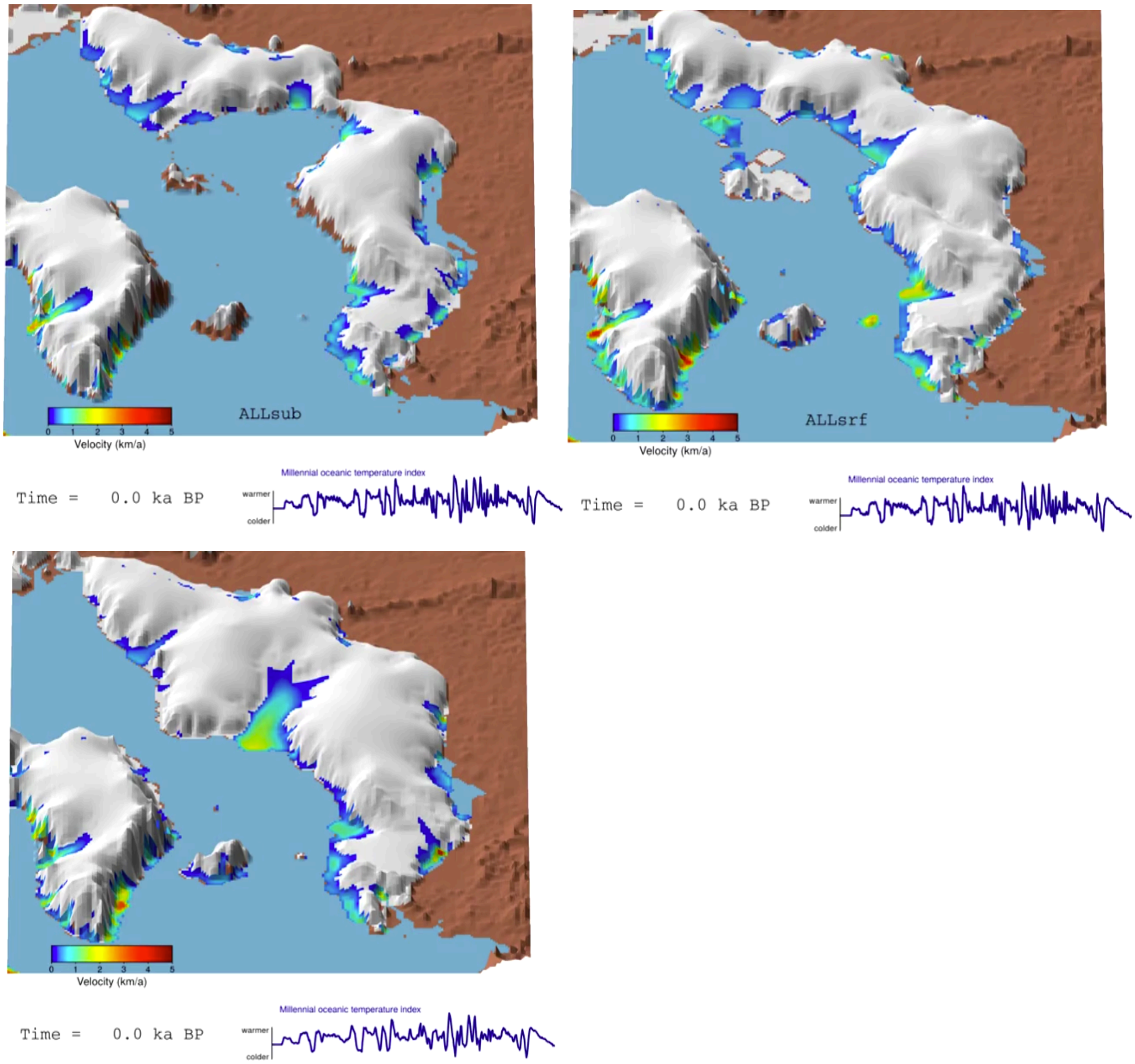

Figure. Screen dumps from supplementary animation of modelled ice sheet state at 0 ka BP (present-day), for experiments ALLsub, ALLsrf and ATM.

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