

Review of Climate of the Past manuscript egosphere-2026-1385:

‘The climate-ice sheet interactions at the Late Ordovician glaciation onset revealed by numerical simulations’

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General evaluation

Sun et al. use CESM 1.2.2 and the ice-sheet model ISSM to simulate Late Ordovician land ice under an atmospheric CO₂ concentration of 6 times the preindustrial value. They run different experiments: forcing ISSM with the CESM monthly output in an offline setup, leading to an ice sheet of small extent; the same simulation but using an asynchronous coupling method allowing this time the ice sheet to reach ~40°S, plus two additional simulations where the maximum ice-sheet extent is imposed in CESM with fixed sea-surface temperatures (SST) one the one hand, and with a slab mixed-layer ocean on the other hand.

In terms of land-ice extent and volume, the results are mostly similar to the previous attempt of Pohl et al. (2016). However, the authors focus here on the impact of the ice sheet on climate. They show that an ice sheet reaching the middle latitudes induces an overall warming over most of the ocean. Warming in the Southern Hemisphere arises from changes in atmospheric circulation and radiative budget (as demonstrated in the slab simulations, which include no [changes in] ocean dynamics); warming in the Northern Hemisphere arises from changes in the deep ocean-circulation and ensuing modifications in the oceanic heat transport.

I think this manuscript constitutes a very welcome contribution. Attempts to simulate Late Ordovician land ice are few, and I think our 2016 paper was the only one so far including climate-ice-sheet feedbacks. Therefore, it is excellent to see these results obtained independently using other climate and ice-sheet models. The asynchronous coupling method, involving no less than 80 steps, is impressive, and the additional simulations using the slab model and the prescribed SST are clever and helpful. The focus on the impact of land ice on climate is interesting. The paper is also very well written and I had great pleasure reading it. Overall, I strongly encourage publication of the study by Sun et al., after revisions. To complement my evaluation, I would suggest sending the manuscript to an expert in atmospheric science, since I am not competent to properly evaluate this aspect.

Major points

- **Impact of albedo.** Section 3.3 insists a lot on the role of katabatic winds (see also the abstract, lines 12–14, lines 384–387). While I did find this section interesting, I think that the primary role of albedo change in driving land-ice growth is critically missed. My expectation is that changes in albedo are the primary driver of land-ice expansion in the asynchronously coupled simulations. I think that demonstrating the opposite would imply repeating the asynchronously coupled simulation without albedo change. As far as I’m concerned, I think it is not worth the effort considering the amount of work involved and expected outcome (but will happily leave this decision to the authors), and would instead revise the manuscript to insist more on the first-order importance of albedo. I also think that, as we proposed in 2016, the strong temperature gradient at the middle latitudes is key in stabilizing the land-ice front there.

- **Equilibrium of the deep-ocean circulation under full glacial conditions?** The authors discuss the changes in the deep-ocean circulation. This is interesting, but I'm not convinced that the latter reached a steady-state following land-ice growth. My understanding is that CESM was integrated for 800 years after land ice *started* to grow – would it change the results to integrate CESM for another, say, 1000 years once the ice sheet reaches its final steady state? The response to this question may largely impact the simulated SST change.
- **Further discussion of the impact of land ice on the low-latitude geochemical record.** The authors demonstrate a small change in tropical SST in response to land-ice growth (Fig. 5i). I think it would be useful to discuss the expected imprint on the geochemical record; for instance, the authors could extract the SST change at Anticosti Island, and combine this number with the expected global ocean salinity change arising from land-ice growth (+ regional simulated pattern), to convert these numbers into a $\delta^{18}\text{O}_{\text{apatite}}$ change, and ensuing apparent SST imprint of land-ice growth. I think this could be a very short yet extremely interesting discussion for the community, the details of which may be provided in the supplement.
- **Discussing land-ice extent.** The sedimentary record of the glaciation suggests that the ice sheet may have reached $\sim 30^\circ\text{S}$ during the Hirnantian (see Fig. 2c), while the ice sheet simulated at 6 PAL $p\text{CO}_2$ reached $\sim 40^\circ\text{S}$. This difference should be described and it should be clearly stated that the conclusions of the paper may be different with an ice sheet reaching lower latitudes. In this regard I disagree with lines 184–185 stating that 'result closely matches the distribution of glacial tillites in the geological record'. Please revise. The references for the paleogeographical reconstructions and tillite database should also be provided in Fig. 2. I also disagree with lines 374–376 (also lines 399–402): modeling results would need to be ground-truthed with Middle Ordovician data for these statements to be supported.
- **Sensitivity to orbital changes.** I strongly disagree with lines 196–197. The sensitivity of the ice sheet to orbital forcing was not quantified. Please delete. Accordingly, future work should also consider orbital changes (lines 199–200).
- **Impact of topography vs. albedo.** I wonder about the contribution of land-ice topography vs. albedo to simulated southern hemisphere warming. Additional slab-ocean simulations could be used to quantify these respective contributions. This is clearly just a suggestion to further improve the manuscript, nothing mandatory of course.

Other points

- **Title.** What about revising the title to insist on the main message rather than on the methods? For instance, 'Limited impact of Late Ordovician ice sheet on tropical ocean temperatures' or similar?
- **The Ordovician mystery.** The authors refer to an Ordovician mystery (line 23). I would suggest avoiding such phrasing, since I think there is not such mystery anymore (the growth of an ice sheet under a high $p\text{CO}_2$ was resolved many years ago by considering the lower solar luminosity).
- **Anchoring the simulations in the geochemical record.** I think it would be good to give rapidly in the manuscript the tropical SST, and compare it with geochemical records, to give the reader an idea of what kind of climatic state is illustrated in the simulations. Otherwise,

statements like ‘the climate condition is comparable to that of reconstruction’ (lines 168–169) are poorly supported. An overview of sea-ice extent may also help (as for now, statements like the one of lines 387–388 are unsupported).

- Line 29. The sedimentary record suggests that the ice sheet may have approached 30°S.
- Lines 31–32: I do not think that the fact that SST approach modern levels can be used to infer icehouse conditions when the configuration of the continents was so different. I think this is misleading.
- Line 37 (and elsewhere, e.g., line 176): what is a ‘super ice sheet’? I suggest to rephrase throughout.
- Lines 82–83: ‘achieving a high level of agreement with geological records’. I do not think this statement is supported.
- **Dome-like geometry.** The simulated ice sheet seems to have a maximum thickness in its center, hence aligning with our 2016 results but contrasting with the alternative modeling results of Lowry et al. (2014). Since there is no general consensus, it may be interesting to rapidly discuss the geometry of the ice sheet in light of the results obtained here?
- **Lines 192–193.** Please provide the land-ice volume and extent as numerical values in addition to the calculated sea-level drop for purposes of future inter-model comparison (+ does the calculated sea-level drop account for isostatic adjustments?); please also provide a detailed comparison with ‘previous reconstructions’ (line 193) or at least provide references to support this statement.
- **Line 207.** The statements of a ‘much lower coupling frequency (200 ka)’ is not correct (the coupling frequency is much less than the one used here, but difficult to estimate due to the modeling setup we used). The ‘coarser resolution’ statement referring to the FOAM atmospheric resolution is also erroneous, since we forced our ice-sheet model with the higher-resolution LMDZ output; please consider deleting as well to avoid misleading the reader.
- **Fig. 3.** Please clearly state that the black contour represents the ice-sheet edge, and that bottom row represents the ice-sheet surface elevation. I think it may be instructive to also include an additional figure showing land-ice thickness in the supplement.
- **Line 264.** ‘warm air started to blow towards the ice sheet’. I cannot see this in Fig. 4I. Is the 0°C isotherm also lacking in Fig. 4, upper row?
- **Line 276.** Throughout (and notably in figure captions), I suggest to avoid using the term ‘control’ for the ‘offline’ simulation. For me, ‘control’ would be a land-ice-free run.
- **Fig. 5.** I think that averaging upper-ocean currents over 200 m rather than 50 m would be more instructive (since it would better account for Ekman deviation).
- **Line 296.** What is a ‘wavenumber-1 structure’? See also line 391.
- **Caption of Fig. 8.** ‘The differences in (a-c) net surface *shortwave* radiation’?
- **Line 335.** I am not totally sure that air coming from the ocean would warm low-latitude continental masses. Isn’t land warming rather driven by changes in the hydrological regime (precipitation minus evaporation)?
- **Fig. 9.** A very short comparison of the global pattern of the global ocean circulation in light of previous modeling results would be a good addition. I also suggest to add contours and contour labels for clarity. I am also surprised to see that waters seem to sink at the middle latitudes in the southern hemisphere – please show mixed-layer depth maps in the

supplement, this would be instructive (and make sure to update all .nc files in the repository to reflect the data plotted in the manuscript for easy re-use by the community later, in line with FAIR principles; from this point of view, the README file would deserve being revised for typos notably). The authors may also want to stress that the simulated decrease in deep-water formation in the SH provide no explanation to regional ocean warming (which would require an increase) – further pointing towards a driven role of atmospheric processes in the coupled model – hence further supporting their analysis.

- **Lines 366–367.** There is also geological data supporting a Darriwilian onset of the glaciation (see references cited in our 2016 paper, and maybe more recent ones). I suggest rapidly stressing this point for completeness.
- **Lines 368–370.** Although I overall get the authors' point, I think the current phrasing looks contradictory – suggesting a limited impact and then a stabilization role. Please revise to better convey the message.
- **Lines 370–372.** Again, I get the point but the phrasing is misleading – the curve reflects tropical SST. Please revise. See also lines 398–399.
- **Line 379.** Please stay consistent with the terms 'control', 'offline' or 'uncoupled'. I suggest to use offline throughout, see previous comment.
- **Line 380.** How is 'realism' evaluated here? This is usually bold to state that deep-time simulations are realistic.
- **Line 382.** Please define the geological record. Please note that the Hirnantian sedimentological record cannot be used to document the Middle Ordovician land-ice state.
- **Lines 403–409.** Another limitation is the use of the simple PDD scheme for ablation.
- **Lines 444–445.** Please provide a reference to support this statement. This is very important.
- **Fig. B3.** What are the 3 columns?

Technical points

- **Line 63: 'and warmer the subpolar'.** Please revise.
- **Caption of Fig. 2.** 'ice sheet surface' > add 'elevation' for clarity? I also suggest to clearly label the 60°S and 30°S parallels.
- **Lines 276–277.** Please revise to properly match Fig. 5 content.
- **Line 277.** 'mostly bare'. Please revise/clarify the meaning.
- **Line 280.** 'The majority landmass'. Please revise.
- **Line 310.** 'shifts'.
- **Line 345.** Exponent formatting missing.
- **Line 396.** 'Response'
- **Fig. B4.** 'triangle' (singular)

References cited

- Lowry, D.P., Poulsen, C.J., Horton, D.E., Torsvik, T.H., Pollard, D., 2014. Thresholds for Paleozoic ice sheet initiation. *Geology* 42, 627–630.
- Pohl, A., Donnadieu, Y., Le Hir, G., Ladant, J.B., Dumas, C., Alvarez-Solas, J., Vandenbroucke, T.R.A., 2016. Glacial onset predated Late Ordovician climate cooling. *Paleoceanography* 31, 800–821.