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Interactive comment on "Could promontories have restricted sea-glacier penetration into marine embayments during Snow ball Earth events?" *by* Adam J. Campbell et al.

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I have to confess to not knowing much about Snowball Earth events. That said, the introduction makes it clear what the paper is about: if colonies of photosynthetic organisms survived these events, then they must have been exposed to sufficient light, so that sea glacier coverage must have not been complete. That idea has been considered before (by the same authors, and by Pollard 2005): the innovation here is the a look at the inclusion of partial obstacles in channels that would otherwise be covered with thick ice.

The term 'sea glacier' was one I did not know, but a quick look at Pollard 2005 (also about the survival of photosynthetic organisms) credits the term to one of the authors

here (Warren) and refers to ice that, although floating and formed by freezing of the sea, looks more like modern land ice in terms of thickness and salinity. Like Pollard 2005, this paper then assumes dynamics similar to modern ice shelves and tongues. It proposes that rocky promontories, by modifying the flow field create an ice shadow, ie a region of thin ice downstream from the promontory, thin enough for photosynthesis to take place beneath the ice.

This is a modelling study, which is looking at the thickness of ice in the shadow with a numerical model based on the Shallow Shelf ice flow approximation (conventionally used to study ice shelves and tongues), and concludes that the mechanism is plausible. It does make reference to some modern ice shadows, just to be clear that we a are talking about a genuine physics phenomenon (if not necessarily a genuine biology phenomenon)

Coming from a numerical modelling perspective largely centred on contemporary ice dynamics, I enjoyed this paper, but thought it was too short. I would have like to have seen some fleshing out of the 'more than one promontory' discussion with model results, though I don't imagine the conclusion would be very different. Likewise, I think some runs exploring the boundary conditions would be instructive - what if the promontory does not impose zero tangential flow (Dirichlett BC), but finite draq (Robin BC)? Does the resulting variation in ice shadow suggest that refugia would be common or rare?

That aside, it is interesting to see the present interest in flow fields with lateral variation having an impact in thinking about the distant past. I suppose some might describe the paper as a bit speculative, but the dynamical model is well founded and the discussion is clear enough for the most part.

Specific Comments ——————————

P1, L21 : Neoproterozoic – how about adding a time period?

P3, L9: It is not just surface gradients around the obstacle that changes the flow. Even a flat ice mass would see its flow deformed, through the interaction between viscous stress in the ice and the no-normal flow (and no/reduced tangential flow) at the obstacle wall.

P3,L10. This sentence assumes x,y-incompressible flow, which is not quite correct. The flow is incompressible but you must take the z-component into account, so e.g the same volume of ice can be moved through a constriction at the same speed as up and down stream if is is thicker within the constriction.

P4,L15: 'This approach is conservative...' : I don't see what you mean here.Are you just saying that the wall geometry is a sensible idealized case, or something else?

P4:L29 '...iteratively… produced local sea ice that was 50m thick (fig 3). This is confusing. I think you are picking points on the h = 50 m contour from eq 1 (so choosing pairs of surface temperature Ts and sublimation rate b), then computing flow model solutions Lg, u(x,y), h(x,y) for a variety. The iteration is just how the ice flow model solves its PDEs? But I had to read to sec 3.2.1 to realize this.

P4: L25. Normally just an approximation to the Stokes equations (but OK, the Stokes are an approximation to the Navier-Stokes)

P5, L3: 'Integrated hydrostatic equilibrium… ' This is the normal shelf front boundary condition, yes? In which case it includes the sea pressure.

P5,L9: 'thin ice < 50 m' . This isn't really a resolution limit, because the model doesn't have a vertical resolution. Presumably, it is related to solver stability (e.g a region of thick ice surrounded by thin ice starts to look like an elliptic PDE with Neumann conditions an all boundaries)

P6,L2: slower, given the same channel width outside the promontory? i.e having the promontory just makes the channel look narrower far upstream.

P6,L4: 'thickness gradient… directs'. Not entirely - the stress balance and BC's alone

would produce this deflection for uniform h(x,y) (see earlier comment). The thickness is a result of the flow as much as the other way round.

P6: Fig 5c is not discussed. What does it add?

P7, L10 (promontory efficiency paragraph). Seems a bit too vague. Why not do some runs that explore this idea, if you are determined to discuss it. I'm sure it is true that a modulated wall exerts more net drag coefficient than a straight one.

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