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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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In this study the authors explore how square obstacles modify the flow of floating ice in a channel with the objective of quantifying thin ice regions or ice shadows in which life could have persisted during snow ball Earth. The scientific question is and interesting one and I believe this study has a potential to give useful insight to the proposed theories of how life could have persisted when oceans were covered by glaciers. Nevertheless, as it is, this manuscript appears to be only a small extension to Campbell et al., 2014. The methods and model used here are identical to Campbell et al., 2014 and the setup for the modeled domain is very similar as well. Specifically, the only modification to the previously studied setup is that before the upstream inflow boundary condition of constant thickness was applied where the channel is narrowed and so its interpretation was a narrow entrance to an embayment. In the current setup the upstream inflow boundary condition is moved further up past the narrowing and so the interpretation of the narrowing inside of the channel is that it is a promontory. This shift of a narrowing further inland and shift of the inflow boundary condition further upstream is what supposedly allows the current ice shadows to possibly persist unlike before. But it seems that it really just comes down to how far away from the narrowing the ocean body lies, if that is the case, it seems that such conclusion could have been reached without further modeling (especially since the ocean is not included in the model). I think for this study to provide some new insight that has not been shown in Campbell et al., 2014 yet, further extension should be included possibly focusing on one of the following questions mentioned in the manuscript, but not elaborated on:

1) It is unclear why authors chose to use a model which has such high minimum thickness requirement, given their main motivation is to answer the question weather life could have persisted in ice shadows of much smaller thickness than allowed by this particular model. There are many other models available that solve the shallow shelf approximation and that allow for much smaller thickness. Using a more suitable model thus could make it possible to answer the question of the aerial extent of zones with light transmission, which the authors express the pity not to be able to answer. Further, it may be worth validating the model with the examples of current climate that are pro- vided in the manuscript. Applying the validated model to known specific embayments from the past and evaluating where life could have persisted would be of great use, thought the later is probably past the scope of this study. **The model we use here has solver stability issues at thicknesses less than 50m. We have spent significant time trying lower this limitation. Because of the nature of the Shallow Shelf Equations (SSA), instability is introduced whenever thickness is allowed to become negative (or zero). We deal with this by setting thickness back up to a reasonable value. With this problem ice is being sublimated and stretches downstream, both of these conditions ensure that ice will thin to zero with a suf-**

ficiently long channel. Any solver attempting to solve the SSA will have stability issues at some thickness with this problem setup.

2) In case the main motivation is not to exhaustively answer the questions regarding ice shadows as refugee for life, but to study the flow of floating ice past obstacles, this study should be a bit more comprehensive and less hand wavy. For example, it would be useful and interesting to analyze for which relative size of obstacle to channel width can a model which uses floating ice only be used as an upper bound for sea glacier penetration length (see specific comments). The authors claim but do not show that this model can always be used as an upper bound. Also the discussion of promontory efficiency should be more elaborate and the speculation that series of small promontories could be more efficient than a large one should be modeled and analyzed rather than speculated about. **As a first study in this topic, we agree that we have not exhaustively examined the effect of size, geometry, position, and number of obstacles. We agree with you about steep-walled promontories being an upper bound and have deleted this claim (see comment below). We are clear that the claim of the series of small promontories being more efficient than a large one is a speculation.**

2 Specific comments P2 L21-23: A finding from previous work of the authors (Campbell et al., 2014) is used as a motivation for further work. These ice free zones formed on the sides at narrow entrances to embayments were however concluded not to be suitable as refugee: Sea- glacier-free zones near the channel entrance would grow thick ice locally if they are located in a cold region of the inland sea. We conclude that none of the sea-glacier- free zones observed in our models near the channel entrance would act as refugia, because they are only observed with colder temperatures that would locally generate thick sea-ice. Is moving the obstacles away from the open ocean rather than keeping them at the entrance where they would not persist because of the closeness of the ocean body the only thing that distinguishes this work from the previous article? If not, some discussion would be useful regarding of why the obsta-

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Discussion paper

cles considered in this paper could persist while those in the previous one could not. How far in the embayment does the promontory need to be located and is x = 0.85L far in enough to be shielded from the main body of the ocean? For what ratio of L to W is x = 0.85L a suitable choice? I would suggest elaborating on and justifying some of these choices. **There are two reasons we use a promontory in the channel that differ from the previous study with a restricted entrance, 1) we wanted to capture both the upstream and downstream effects of the promontory, the previous study only captured downstream effects, 2) ice thickness near the ocean side of the channel was too large to thin ice sufficiently to allow transmission of light except in very cold cases. We wanted to know if there was a promontory in the channel, could ice thin sufficiently at warmer temperatures.** "We found ice shadows could only exist near the entrance of the channel at very cold temperatures. In this study, we examine promontories along the channel sidewalls that are far from the entrance of the channel in order to capture both upstream and downstream effects of promontories and to determine if ice shadows can form downstream at warmer temperatures."

P3 L2: Section 2 does not provide theoretical framework, it only provides intuitive explanation of the mechanisms involved. **Changed for clarity** "In Section 2, we provide an explanation for why ice shadows exist and provide examples of ice shadows on the modern Earth. "

P3 L20-21 Fig 2a: It would be useful to show thickness contours superimposed on top of or even instead of the satellite image. From the simple photograph it is not obvious where exactly is the ice shadow and how significant it is. **I modified the figure to show figure direction and magnitude, which I think demonstrates with point.**

P3 L26-28 Fig 2b: This example doesn't seem relevant to this study which only considers floating ice, while this is a grounded ice example. **Indeed this study does only consider floating ice, and this is a grounded example. This example in Fig 2b still has merit because it shows clearly ice thinning as it moves around an obstruction. I have changed the wording to make that fact that Turnabout Glacier is**

grounded more clear. “The grounded Taylor Glacier flowing around Finger Mountain (77.8 °S, 161.3 °E) and incompletely penetrating Turnabout Valley. Arrows indicate the ice-flow direction.”

P4 L17 P4 L17: For consistency, give values of W and L used in the model and/or express Lp considered in terms of W or L. **The term L does not explicitly enter into our model and is only used to illustrate that open water conditions exist past the end of the sea glacier. Lg is solved for in our model. We have specified W.** “The geometry of our experiments consisted of an idealized rectangular channel with width W and length L, here W is 200 km and L is long enough to prevent the sea glacier from contacting the end of the channel.”

P4 L17 P7 L30-32: Can you show that the following statements are always true? If not, then you cannot speak of an upper bound or conservative approach to be guaranteed by this model unless specifying when exactly it is true:

P4 L17: This approach is conservative because other wall configurations could increase drag and reduce sea-glacier penetration.

P7 L30-32: However, a grounded sea glacier flowing over a promontory would still tend to slow because of the additional basal resistance, and the penetration length L would be decreased, increasing the probability of a refugium farther along the channel. I suspect that as Lp $\rightarrow$ W there could be a point when Lg reached by flow through the narrow opening will be smaller than Lg reached by flow over the promontory. **You are absolutely correct, our usage of upper bound was not accurate. We have changed this text on page 4.** “This approach is an idealization, if the sea glacier were allowed to move onto the promontory, it could increase or decrease sea-glacier penetration.”

P4 L24-26: This sentence is a bit strange, just say that you solve the Shallow Shelf Approximation in steady state, later you repeat what it is anyways. **Changed for clarity** "To simulate the behavior of a sea glacier flowing in a channel containing a promontory, we used an ice-flow model solving an approximation to the Stokes momentum-balance equations (called the Shallow Shelf Approximation [Morland, 1987; MacAyeal et al., 1996]) in steady state."

P5 L1: By no-flow conditions do the authors mean no-slip? How do you justify this choice in the context of wanting to be conservative in computing Lg as was emphasized before? **Changed no-flow to no-slip. This is justified because the ice is well below the melting point.** "Along the sidewalls and along the promontory, no-slip conditions are applied, a suitable condition for ice below the melting point."

P5 L4: Penetration length was previously called Lg not L **Corrected Typo. Thank you.**

P5 L9: Why is the minimum thickness requirement so high? It looks like in your previous study 5 m thick ice was possible. Why not use then a realistic thickness that allows for sufficient light penetration? **We have issues with solver stability with ice thickness less than 50m. We changed the wording to me this more clear.** “For our purposes, we define thin sea ice to be less than or equal to 50 m, because that is the ice-flow model’s solver stability limit for thin ice.”

P5 L13: typo - through in stead of tjhrough **Corrected Typo. Thank you.**

P5 L13: keep consistent terminology, keep clear when referring to sea ice and when to sea glacier. **Done**

P5 L22-26: The discussion on albedo seems a bit out of place given solar radiation is not included. **We wanted to be clear how this is different from Warren 2002**

P5 L28-29: Reformulating the first two sentences may be helpful. First statement is completely generic. Second sentence is just a fragment and it is unclear what it connects to. **I deleted those awkward sentences and changed the following one.** “Far upstream of the promontory, ice flow is fastest along the center of the channel, and the pattern of ice flow is indistinguishable from ice in flow an unobstructed channel (Figure 4 for an representative case);”

P6 L2-3: Does this length scale come out of the equations or is it just by eye comparison? **This comes from a scaling argument from Kamb and Echelmeyer 1986. I've added the reference.**

P6 L12: The naming of this metric as thickness drop is a bit deceiving as it includes both thickness increase and thickness decrease so it is counting the same thing twice. Perhaps a better way to quantify thickness drop would be to compare the thickness past the obstacle to the thickness in the control run. With the metric as it is a hypothetic situation of ice thickness increasing upstream from the obstacle and no change of the flow showing downstream would show as a thickness drop. Using a more general metric would be useful for example to compare thickness drop for the case of narrow entrances to an embayment as in Campbell et al., 2014. **I do agree that this metric includes both upstream thickening and downstream thinning. However it is not intuitive to compare to case with no promontory. The no promontory case has a different overall glacier length; so comparing the same distance along the x-axis would not be a genuine comparison either.**

P6 L26-30 Fig 5b: This paragraph suggest there are two regimes, 10-60 km and 70-100 km, however the figure shows rather smooth transition. **There is a regime where TIP in Figure 5b is close to 0 and a rather smooth increase after that. I have reworded this for clarity.** "For promontories with Lp of 10-30 km, centered at 0.85 Lg, where Lg is the penetration distance in the control run, typically 25

P7Ł9: What happens to the efficiency of promontories as defined here when Lp $\rightarrow$ 0 and does it make sense? **Certainly a zero length promontory would not reduce sea-glacier penetration in the way I have discussed. There likely exists some limit to this efficiency, as suggested by the change in inflection of the blue line on Figure 5c.** "Furthermore there is likely some limit to this effect at sufficiently small promontory sizes. "

P7 L11-14: I would suggest this effect of an array of promontories and the effect of spacing between them to be included in this paper for completeness, rather then hypothesizing about it. **We agree that would be a good addition to the paper. However the work presented here has sufficient merit as a first study.**

P7 L16-18: It seems that this study would be a right place to include this generalization to rectangular geometries. **We agree that would be a good addition to the paper. However the work presented here has sufficient merit as a first study.**

P8 L2-5: Perhaps mention earlier the reason you keep using 50 m for minimum thickness - this choice was not justified earlier and seemed strange. Also give reasonable values of ice thickness for light transmission to be sufficient for life to persist. **We have issues with solver stability with ice thickness less than 50m. We changed the wording to me this more clear.** "For our purposes, we define thin sea ice to be less than or equal to 50 m, because that is the ice-flow model's solver stability limit for thin ice."

P8 L14: Modern examples and analogues can be found of both grounded and floating ice that thins as it flows around obstructions - is not really a 'conclusion' of this study. **True. Removed**

Fig 4: flow speed in stead of flow rate **done**

Fig 5: Why is there a sudden drop for the 75m thin ice percentage for Lp = 100km? Does the trend continue for higher Lp and if so what is the reason for the reversal of the trend? **We only explored Lp up 100 km. I imagine there will some limits to how well the model work at larger Lp. We find Lg so that the sublimation over the channel balances ice inflow into the channel. However this there is an asymmetry to ice flow generated by the promontory. This makes it so that the edge of the sea glacier will not be perfectly oriented with this model domain. I believe this problem gets worse as Lp becomes larger.**

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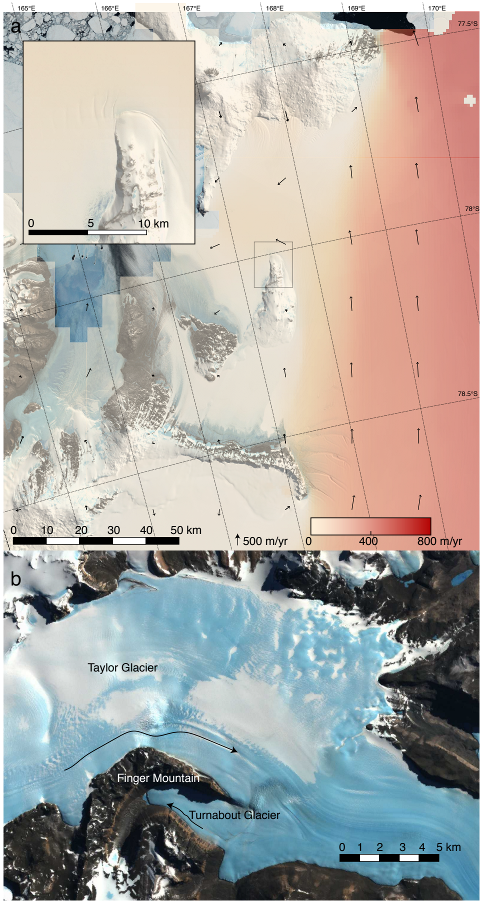

Fig. 1. Figure 2. a) Satellite image of Ross ice shelf flowing around White Island (78.1 °S, 167.4°E) forming an ice shadow on the northwest side of White Island where a tidal crack can form (inset); colorba