Review of, “Late Holocene Stabilization of Conway Ice Ridge”, by Hoffman et al.

Summary

In this paper by Hoffman et al., the authors use a combination of ice-penetrating radar data and numerical modeling to understand the ice dynamic history of the Conway Ice Ridge in West Antarctica over the last few thousand years. By identifying relic crevasses buried beneath smooth layers in the radar data, they are able to show that the ice streams bounding Conway Ice Ridge were once wider than their current extent, and that they narrowed to their current configuration over roughly the last 1000-3000 years. Because of uncertainties in the simple age-depth model they used to date the overlying layers, they are unable to date the timing of ice stream narrowing with much precision, but the qualitative pattern of ice dynamic changes is clear, and is also consistent with biogeochemical evidence indicating that the grounding line in this sector of Antarctica was retreated inland relative to its present position in the mid Holocene, and that it subsequently readvanced to its present position in the late Holocene. The authors then use a relatively simple stress-balance ice flow model to examine how prescribed changes in surface elevation, boundary flow speed, and basal strength affect the distribution of fast flow and shear margin locations in this region. While their model was unable to reproduce a wider configuration of Van der Veen Ice Stream, they did successfully model fast ice flow across the promontory at the downstream end of Conway Ice Ridge when they combined all three perturbations simultaneously.

Major Comments

This paper is a decent incremental step forward in the understanding of the Holocene dynamics of West Antarctica. It adds to a growing body of literature indicating that the Ross Ice Shelf grounding line retreated past its present-day position in the early-mid Holocene and then subsequently readvanced in the late Holocene to its present-day position. The radiostratigraphic evidence presented are a strong indicator of past ice dynamic changes and the modeling, while simple, supports the main conclusions and interpretation. I recommend publication.

My biggest request for changes to the paper is that I would like to see some more work fleshing out the uncertainty around the simple 1D age-depth model used to date the timing of ice dynamic changes. The authors used both a Nye model (constant vertical strain rate) and a Dansgaard-Johnsen model (piecewise linear distribution of strain rate) to date their layers. However, they did not vary the same parameters for both models: they varied surface accumulation rate and basal melt rate for the Nye model, and surface accumulation rate and shape factor for the DJ model. They presented both models as equally likely in Figure 7; however, the Nye model is a special case of the DJ model, corresponding to the limit where $h=0$ and $\varphi=1$. Finally, they did not include the effect of unsteady conditions in their age-depth modeling. Both age-depth models they used are steady state models, but this paper is explicitly focused on reconstructing unsteady dynamics. As a result, their age constraints are only “qualitative” (their word; L418) rather than quantitative.

I think that with a bit more work, they can transform their qualitative chronology into a quantitative one with defined uncertainty. In my opinion, the first step towards doing that would be to use a single age-depth model. As I mentioned, the Nye model is a special case of the DJ model corresponding to the limit where $h=0$ and $\varphi=1$, so it is not necessary to include it as a separate model. If the authors consolidate their age-depth modeling into a single DJ model, then it will be easier to translate uncertain distributions of the input parameters into a distribution of the output age. Second, this single DJ model will need to incorporate a basal melt rate. I’m not sure if the DJ model has an analytic solution with nonzero basal melting, but in any event it should be quite easy to compute a numerical solution if not. Finally, they need to account for the uncertainty in the average thinning or thickening rate since the layers were deposited.

This will produce a total of four free parameters: surface accumulation rate, basal melt rate, shape factor, and ice thickening rate. One way to display this parameter space would be to select 2D slices, as the authors did in their Figure 7. However, another option is to bootstrap the assumed distributions of the input parameters into a distribution of the layer ages. The authors have already made assumptions about the reasonable range of these parameters (other than thickening rate), and additionally made the implicit assumption that they are uniformly distributed (by presenting their 2D slices as though all regions of the slice are equally likely). They can keep the uniform distribution assumption or make other assumptions about these parameters (for instance, choose a central estimate with a Gaussian uncertainty). Either way, since the DJ model is very computationally cheap, it would be easy to sample the parameter space associated with the assumed distributions and produce probability distribution functions for layer age, thus transforming their qualitative chronology into a quantitative one with error bars.

To be clear, my above comments are a suggestion, not a command. The authors are free to choose different methods. They are also free to provide an argument as to why a more advanced treatment of layer ages is not appropriate or not possible. However, I think that some attempt to tackle this problem head on- either more rigorous age-depth modeling, or an explanation of why more rigorous age-depth modeling cannot be done- would go a long way towards completing this paper.

Minor Comments

L38-39: "...make the Siple and Gould Coast ice streams a compelling natural laboratory for understanding ice-stream evolution."

This is true, but the problem with the fact that the glaciological community has relied so heavily on this region as a natural laboratory is that it may have biased our understanding towards the type of ice streams found here. The Siple Coast ice streams are low-bed-strength, low-surface-gradient, topographically-unconfined ice streams. In a way, this makes them valuable to study as "pure" ice streams mostly uninfluenced by other processes, but it also means that insights gained from studying them may not apply to other ice streams where these conditions do not hold. There are very few other ice streams in the world where those conditions all apply. For instance, the most important glaciers for projecting future sea level rise are Pine Island and Thwaites, which are quite different from the Siple Coast (for instance, PIG is topographically confined in its main trunk, and Thwaites has both high drag and high surface slopes in its near-grounding region). Perhaps some of the effort spent studying and modeling the Siple Coast would have been better spent studying a more diverse selection of glaciers? This point is admittedly irrelevant to your paper, but still, it is something that comes to my mind whenever I hear the Siple Coast discussed as a natural laboratory.

Figure 1.

It would be nice to show the velocity direction vectors either here or in a later figure, especially given the discussion about divides or the lack thereof, and the discussion later on about flow across the promontory.

L90: "field seasons conducted in the austral summers of 2001-2002 and 2003-2004"

It's wonderful that we're still getting use out of data that's more than twenty years old.

L121-122: "The corrected bed-returned power and depth-averaged and depth-variable englacial attenuation rates for individual traces were calculated for multiple reflectors in each trace"

Why mention this in the main text, when the appendix reveals that the results were unreliable and you don't actually use these results in your analysis at all? For that matter, why bother even including this in the appendix?

L132-134: “We choose to apply two relatively simple depth-age models due to spatiotemporal uncertainty in input parameters (accumulation rate, basal-melt rate, and ice-flow parameters) and vary these parameters over physically plausible ranges to estimate the age of englacial layers.”

What about the ice thickness change rate? The age-depth models you use are steady-state models, but you are investigating non-steady processes of ice stream slowdown and shear margin migration. What is the influence of non-steady ice thickness changes on the age-depth relationship?

Figure 2.

It can be hard to interpret some of these plots. It looks like you have used both hillshading and contours for all four subplots, is that correct? Hillshading makes sense for bed elevation (plot b), but for the other three, including hillshading makes it harder to interpret the data. For instance, it looks to me like plot c is saying that some areas of Conway Ice Ridge have sped up by 5-10 m/yr in just the last two decades. That seems somewhat implausible to me, and I suspect that I am misinterpreting the figure. Removing hillshading guarantees that brightness variations are due to changes in the underlying data value, not changes in the spatial gradient of the data. The use of a background image (MODIS? SAR?) also makes it harder to interpret the data. I would recommend simplifying these maps to make them easier to read. In addition, you can crop the color scale to values that are relavent to your region of interest. There is no need to represent bed elevation values up to +1500 m, for example. The colorcale on the elevation change map can also be tightened a lot.

L167-168: “This diagnostic approach allows us to solve for ice-flow speeds without prescribing accumulation and ocean melt-rate forcing”

In other words, your model is only solving for stress-balance snapshots. This is a reasonable approach given how you interpret the results, but it is a limitation of the model. It is unclear, for instance, if dH/dt fields computed from the your model velocity fields would be consistent with the surface elevation changes used to force the model in the first place.

L175, Equation 1

This is the Shallow Shelf Approximation or Shelfy-Stream Approximation, SSA. Cite MacAyeal, 1989 here (DOI:10.1029/JB094iB04p04071).

L178-179: “We include thermal softening by coupling the 1D thermal model of Meyer and Minchew (2018) to solve for the average ice temperature at every grid point.”

Do you take the column average of temperature before computing the rheology, or compute the rheology first and then take the column-average? Because of the nonlinearity of the relationship between temperature and rheology, you will not get the same results both ways. The correct way is to first compute vertically variable rheological stiffness, B, and then take the column average of B. Note that you should use stiffness B, not softness A, for this calculation, because stiffness is linear in the stress balance equations (effective viscosity η is directly proportional to B) but softness is not.

From this description, it sounds like you did this the wrong way, by first computing the column-average of temperature, then using that to compute rheology. If that’s the case, then I don’t want to say that you should throw out all of your results and completely redo the model, but you should at least do a sensitivity test where you compute the column-average rheology correctly to see how much of a difference it makes to your results.

Figure 3

It doesn’t make much sense to have a color scale of ±30 m in plot (b) when you are only showing changes of -3 m. I realize that this color scale aligns with the one used in Figure D3, but many readers will not bother with the appendices, and I think it is fine to use a different color scale in the main text.

L189-190: “We interpolated the basal strength field from estimates of the basal resistance inferred using the Ice-sheet and Sea-level System Model”

The use of an inverted friction field means that you are, to some extent, enforcing the locations of shear margins and fast flow because the inverted friction field is constrained by the observed present-day velocity. This somewhat undercuts the claim that your model can be used to solve for the location of the shear margins. It also goes a long way towards explaining why your model was incapable of simulating the narrowing of van der Veen ice stream. However, it also means that when your model did simulate a change in shear margin location (at the promontory) that that result is more likely to be robust, because a model using a present-day inverted friction field is always going to be biased towards reproducing the present-day flow pattern.

L252-253: “Near the confluence of Mercer and Whillans Ice Streams, there is also a promontory where ice flows slowly from Mercer Ice Stream towards Whillans Ice Stream.”

This is the sort of thing which would be easier to visualize if you included velocity vectors in one of your figures.

Figure 4

The caption should specify that the color scale representing basal roughness is only displayed along the flight line. When I first saw this picture I thought that the main map was showing two color scales simultaneously and I was confused. In addition, the background map should be simplified. It looks as though you plot the background SAR imagery and the velocity colors on top of each other, but the brightness variations from the SAR background make it harder to interpret the velocity data. I would say to either plot one or the other, don’t plot both of them in the same location. The same comment applies to figures 5 and 6 as well.

Figure 8

Would it be an accurate interpretation of this figure to say that the model can reproduce small changes in shear margin position at the promontory, but not the larger changes observed elsewhere in the domain? As I mentioned above, this is probably related to the fact that the model is forced by inverted basal drag, which is constrained by the present-day velocity field.

Also, what do the contours represent in plot a?

L391-392: “Our simulations show little additional inland migration when we prescribe additional surface thinning”

A weakness of your argument here is that you only tested surface thinning fields obtained by extrapolating the pattern of present-day thinning into the past. However, the spatial pattern of thinning could have been different if the ice sheet configuration was different. In particular, the present-day pattern has thinning mostly co-located with fast flow, and if the old area of fast flow was more extensive, then the region of thinning might have been as well.

L406-413: Paragraph about the model not being able to reproduce the changes in the northern van der Veen Shear Margin.

I am glad that you included this discussion here. It would have been a major omission if you left it out. I would suggest adding two things to this discussion: 1) the fact that the model uses a basal drag field tuned to the present-day velocity field, and 2) the fact that the spatial pattern of thinning used in the perturbation experiments is also largely confined to the present-day fast flow.

Figure 9

Is this schematic supposed to show a narrowing of van der Veen? It looks the same width to me.

Appendix A

What is even the point of including the reflectivity analysis here? It turned out to be unreliable and you didn't actually use the results for anything. Is the point merely to document everything you tried, regardless of whether or not it worked?

L511-512: "For both methods, we define the full thickness as ∼100 m below the surface to 85% of the ice thickness to avoid the direct arrival and low signal-to-noise deep in the ice column."

This is a fundamental problem with any attempt to use englacial reflectors to generate an attenuation correction for the bed, and probably contributed to the unreliability of your results. The problem is that, for typical ice sheet thermal profiles, the warmest ice (and hence the most powerful attenuation) is found near the bed. So an analysis using only englacial reflectors can't estimate the attenuation rate of the most important part of the ice column! This is in addition to the other problems you mentioned (such as layer slopes).

Appendix C

Both age models assume steady state, however, a key point of this paper is that this region is no in steady state. You should consider running some tests to see what the effect of unsteady age-depth profiles might be. In addition, the Dansgaard-Johnson model does not include a basal melt rate, which doesn't really make sense considering that the basal melt rate was one of the two key parameters you tested for the Nye model. See my major comments above for more thoughts about the age-depth modeling.

Appendix D

What is the point of discussing the constant basal drag model? As with the reflectivity analysis in Appendix A, it did not end up being reliable and you didn't end up doing anything with it. Again, is the point merely to document everything you tried, regardless of whether or not it worked? To my mind there is no point including the constant basal drag model in this paper.

L603-604: "All of the simulations presented in the main text are steady-state solutions that solve for the velocity associated with different assumptions for the ice thickness, boundary velocity, and basal strength."

I suppose that the phrase "stress-balance snapshots" might be a better description than "steady-state solutions", but this isn't really so important.

Figure D3: "Black lines in elevation perturbation experiment show changes in height above flotation relative no change in height above flotation."

I am confused by this sentence, please clarify.

Technical Corrections

L31-33: Names of projects and references

It feels weird to have both the names of the projects and the literature references grouped together in the same parentheses. In particular, it is jarring that there is no break after SWAIS2c to separate the project names from the author names. I don't know what the right style guideline is here, but consider changing this so that it reads easier.

L41: (+/-0.5° C)

Remove the parentheses and use a real ± sign.

L130: "traced traced"

L404: "as ice sheets widen"

Should be "as ice streams widen".