Review of, “Sensitivity of the Ross Ice Shelf to environmental and glaciological controls”, by Baldacchino et al.

Summary

In this article, the authors report on a sensitivity study of the short-term (20 years) mass balance of the ice sheet catchments surrounding the Ross Ice Shelf to perturbations in surface and basal mass balance, basal drag, and ice rheology. They use the automatic differentiation capability within the Ice Sheet System Model to evaluate the linear sensitivity of the 20-year Volume Above Flotation (VAF) to each of those four spatially distributed parameters- that is, the derivative of VAF (evaluated after 20 years) with respect to localized perturbations in those four spatially distributed parameters. With respect to the two force-balance variables they tested, they found high sensitivity of VAF to basal drag within the fast-flowing but low-drag downstream regions of the Siple Coast ice streams, as well as high sensitivity within the main trunk of Byrd Glacier, while the sensitivity of VAF to ice rigidity was highest in shear margins and around the perimeter Roosevelt Island. With respect to the two mass-balance variables they tested, they found near uniform sensitivity of VAF to SMB within the majority of the grounded ice sheet, no doubt reflecting the short 20-year timepan of their simulation, while the sensitivity to BMB was greatest near the grounding line.

The method used by the authors is sensible, the results are well presented and well discussed, and the conclusions are supported by the results presented. This paper describes the application of an existing technique to a new area of the ice sheet, and thus represents a solid incremental advance in our understanding of the Antarctic Ice Sheet. This paper deserves to be published in *The Cryosphere*. However, before publication, I have some concerns that I would like to see addressed around the analysis of basal drag. I describe these concerns next, and then move on to more minor comments. Overall, my concerns can be addressed with additional figures (or additional subplots in the existing figures) and a bit more analysis.

Major Comment: Basal Drag vs Basal Drag Coefficient

My biggest concern with this paper is with the use of a linear sliding law, in which basal drag is directly proportional to sliding velocity. Simply put, a linear sliding law is not a realistic or physically defensible representation of dynamics at the ice base. Even in areas of the ice sheet underlain by hard bedrock, the sliding relationship is expected to be multi-valued with a maximum basal drag given by Iken’s bound (Iken, 1981; Schoof, 2005; Gagliardini et al., 2007), a relationship which is quite different from a linear one (especially for fast sliding, like the ice streams where the authors found high sensitivity). Moreover, the Siple Coast ice streams, where the authors find the highest sensitivity to basal drag, are known to be underlain not by hard bedrock but by soft subglacial till that obeys a Coulomb plastic friction law (Tulaczyk et al., 2000). The linear sliding rule used by the authors corresponds to power-law sliding with an exponent equal to one, while a more realistic Coulomb plastic sliding law corresponds to the limit where the exponent approaches infinity. Thus, the sliding law used in this paper is the exact opposite of a realistic one for the region of interest. Therefore, it is critical that the authors present results that are robust to the choice of sliding law.

My recommendation is that the authors show maps of the sensitivity of VAF to basal drag (τ_b) alongside their maps of sensitivity to drag coefficient. The drag coefficient (C_b) is an artifact of the choice of a linear sliding law, while in a more realistic Coulomb plastic sliding law, the basal yield stress would be the quantity which describes the state of the bed, and the basal yield stress would be equal to the basal drag in areas that are actively sliding. Furthermore, while the coefficient (C_b) will change with different sliding laws, the basal drag (τ_b) is likely to be broadly similar for all sliding laws for two reasons: 1) all potential inversions with all possible sliding laws would be run within the same ice sheet geometry, and thus experience the same gravitational driving stress; and, 2) all potential inversions with all possible sliding laws would be constrained to match the same observed surface velocities, and thus all inversions will converge on similar patterns of englacial stress transmission. When identical patterns of driving stress are combined with similar patterns of englacial stress transmission, the result is that momentum conservation will dictate similar basal drag patterns regardless of the sliding law.

Fortunately, it should be possible to compute the sensitivity of VAF to τ from their existing results on the sensitivity to C, without the need to recompute the entire model. Starting with the linear sliding relationship (Eq. 2 in the manuscript), we have,

$$\tau_b = C_b^2 N v_b \quad , \tag{1}$$

where N is effective pressure and v_b is the basal sliding velocity. We can then compute the derivative of drag with respect to drag coefficient,

$$\frac{d\tau_b}{dC_b} = 2 C_b N v_b \quad . \tag{2}$$

Once we have the derivative of drag with respect to drag coefficient, it is easy to convert the sensitivity estimate the authors already have (dV/dC) to one that would be more robust to the choice of sliding law (dV/dτ), like so,

$$\frac{dV}{d\tau_b} = \frac{dV}{dC_b}\frac{dC_b}{d\tau_b} = \frac{1}{2 C_b N v_b}\frac{dV}{dC_b} \quad . \tag{3}$$

This quantity, the sensitivity of ice sheet mass balance to changes in basal drag, is likely to be far more representative of the soft sediments that compose the true ice sheet bed than the sensitivity to drag coefficient. A map showing this quantity should be added to figure 2, and this quantity should also replace the sensitivity to drag coefficient in the top rows of figures 3 and 4. Conclusions drawn based on the sensitivity to basal drag are more likely to be robust to changes in the sliding law than conclusions drawn based on the sensitivity to drag coefficient.

In addition, it might also be interesting to look at maps of the sensitivity to normalized perturbations in basal drag, beyond merely the sensitivity to drag perturbations. That is to say, it might be interesting to look at the sensitivity to dτ/τ, rather than merely dτ. That is because there is a very large range in basal drag within the domain: within the very low-friction ice plains in the downstream regions of the Siple Coast ice streams, an increase in τ by a few kPa could easily represent a doubling of the local basal drag, whereas in other regions that same perturbation might be an increase of only a few percent. Using Equations 1 and 3, the sensitivity to a relative drag perturbation can be computed by,

$$\frac{dV}{d\tau_b/\tau_b} = \tau_b \frac{dV}{d\tau_b} = \frac{C_b}{2}\frac{dV}{dC_b} \quad . \tag{4}$$

However, I view the sensitivity to relative drag perturbations (4) to be less important than the basic sensitivity to drag perturbations (3). If the authors want to omit the sensitivity to relative drag perturbations, that is fine. It is only important that the sensitivity analysis by done with respect to drag, rather than drag coefficient, to ensure that the results of this study are robust to the choice of sliding law. Of course, I do not expect the results to be hugely different. It is likely that the map of sensitivity to drag will be similar in spatial structure to the map of sensitivity to drag coefficient, so the authors probably will not need to change their conclusions or rewrite much of the manuscript. However, until the map is actually made, we cannot know for sure. An analysis of the sensitivity of the model to basal drag will be much more physically defensible and more robust to changes in sliding law than an analysis of the sensitivity of the model to the coefficient of a linear sliding law.

Other Comments

L29-30: “Through the ice shelf restraining this ice in the catchment, it has a total potential contribution to sea level rise of 11.6 m (Tinto et al., 2019).”

This is very awkwardly worded. Perhaps rephrase to something like, “The grounded ice in its catchment has the potential to raise sea level by up to 11.6 m (Tinto et al., 2019).”

Figure 1

Perhaps it would be better to show velocity with a logarithmic color scale? Velocity varies by several orders of magnitude over the ice sheet. Presenting it on a linear scale as in this figure has the effect of emphasizing fast-flowing areas, especially the ice shelf. Perhaps this was the intention; however, a side effect of this is that the structure of ice streams in the grounded part of the domain is faded and difficult to see in many places. In addition, it would be helpful to show some aspects of the ice sheet geometry in additional plots of Figure 1, such as the bed elevation, ice thickness, or surface slope.

L68: “...ice viscosity depending on the ice temperature…”

Where do you get the ice temperature from?

L72-83: Model setup

What does your inverted drag coefficient look like? What about the other forcing fields? It would be useful to have a figure showing maps of the background fields used to force the model, so that we can put the sensitivity maps in context. I would be interested in seeing a figure that showed the inverted basal drag coefficient, the actual basal drag, the ice rigidity, surface mass balance, and basal mass balance. Without being able to see these fields, it is hard to put the sensitivity maps in context. Remember, the quantity being computed by the AD procedure is the linear sensitivity- that is, the derivative of the output quantity with respect to small perturbations in the input quantity, *evaluated at the given value of the input quantity*. If the background ice sheet configuration changes, the sensitivity will change as well.

L102: Equation 4, compared to Figures 2 and 5

This equation implies that you have the units wrong in the labels of Figures 2 and 5. In order to get from the sensitivity to a parameter, $\mathcal{D}V(P)$ and the perturbation in that parameter, δP, to an estimate in the volume change (V), it is necessary to perform a spatial integral over the model domain, with differential $d\Omega$. Thus, the sensitivities $\mathcal{D}V(P)$ should have units of meters per (parameter units), rather than m^3 per (parameter units), as you have given in the figure titles. The extra m^2 necessary to form a volume comes from the $d\Omega$ in the integral.

L107: “These sensitivity maps show that the vast majority of the grounded ice is not sensitive to changes in friction or rheology…”

This wording isn’t quite right. Presumably, the local flow in the vast majority of the grounded ice is going to be sensitive to local changes in friction or rheology. What you mean here is what you explained in the rest of the sentence, namely, that the overall mass balance of the ice sheet (after 20 years) is not sensitive to local changes in friction or rheology in the vast majority of the grounded domain. Perhaps a better wording would be, “These maps show that the sensitivity to friction and rheology is low in the vast majority of the grounded domain…”

L118-120: “Kamb Ice Stream on the Siple Coast shows low to no sensitivity to changes in the basal friction or ice rigidity highlighting that the Kamb Ice Stream is currently stagnant and thus changing the friction or ice rigidity will not change the ice discharge significantly.”

It is worth pointing out that this result is a byproduct of the assumptions inherent in a linear perturbation analysis, and thus this negative result for Kamb ice stream can potentially be misleading. The sensitivity maps produced by this method basically answer the question: 'if we changed the friction coefficient at this particular location by an infinitessimal amount, what would be the marginal change in VAF?' In the continuous limit, each perturbation would only be to a single point with vanishing area, while in the numerical implementation, the perturbation resides at a single mesh node.

Thus, the method cannot capture the sensitivity of VAF to a general reactivation of Kamb ice stream, which would involve a spatially correlated large-amplitude reduction in basal friction across the entire bed of the former ice stream. Clearly, a general reactivation of Kamb ice stream would reduce the grounded volume of the ice sheet. However, a reduction in basal friction at one location within the former ice stream would not have much effect if friction remained high across the rest of Kamb. Thus, the result of a low sensitivity within Kamb Ice Stream is accurate within the assumptions of this method, but it can be misleading in the sense that this method doesn't test for the possibility of spatially correlated perturbations to model parameters, and a general ice stream reactivation would be a spatially correlated perturbation.

As I mentioned above, this method tests the sensitivity of VAF to perturbations in the forcing parameters, *assuming that the general ice sheet configuration does not change*. The reactivation of a dormant ice stream is a big enough perturbation that it would constitute a change in the general ice sheet configuration.

Figure 3:

Are the negative values in the bottom row (sensitivity to rheology B) real, or do these simply represent numerical artifacts? Put another way, do you actually believe that there are places where strengthening the ice will actually cause *more* of it to flow into the ocean? There are also some negative values in the top row of this figure and in figure 4, but those negative excursions are much smaller than the negative excursions in the bottom row of figure 3.

Figure 5, L127-149, L192-201: Sensitivity to SMB and discussion of sensitivity to SMB

I would be very interested in seeing a map of the sensitivity to SMB normalized by the stagnant ice sensitivity- that is, the sensitivity that you would get assuming that the ice did not move and the perturbation to SMB simply piled up mass at the location of the perturbation. That "stagnant ice" sensitivity is simply given by the time period of the simulation, in this case 20 years (by the way, I believe that you have made an additional units error in Figure 5, beyond the units error I pointed out earlier: the sensitivities to surface and basal mass balance probably should have units of m/(m/s), rather than m/(m/a) as you put in Figure 5, since otherwise values on the order of 6-7x10^8 are far too large. However, if ISSM is producing sensitivities in terms of m/s instead of m/a, then those magnitudes are exactly what one would expect from the stagnant-ice sensitivity with a 20 year model runtime). Normalizing the SMB sensitivity by the stagnant-ice sensitivity should help to put the SMB sensitivity in context. If there are any areas with sensitivity greater than the stagnant-ice value, then you could say definitively that SMB is vital to keeping those areas grounded and buttressing the rest of the ice sheet. Areas below the stagnant-ice value probably reflect regions where ice flux evolves very quickly (within the 20 year runtime) to export additional mass.

I also would expect the results for SMB sensitivity to drop below the stagnant-ice sensitivity as model run time is increased beyond 20 years. In a steady state, the sensitivity of ice volume to SMB is quite weak, as increases in SMB are mostly balanced by increases in flow rate into the ocean, with only small increases in surface slope (and thus inland ice thickness) necessary to produce those increased flow rates. A classic scaling analysis from John Nye suggests that the steady-state thickness of an ice sheet scales with accumulation rate to the power of $1/(2m+1)$, where m is the sliding exponent (Nye, 1959). Thus, even with a linear sliding law ($m=1$), thickness would only increase with the cube root of accumulation rate, while for more realistic sliding laws the dependence would be weaker still, and for a Coulomb plastic bed where $m \rightarrow \infty$, such as the bed found in the Siple Coast ice streams, the steady-state sensitivity should approach zero. But with only a 20 year model runtime, I would expect the vast majority of the inland ice to have a constant SMB sensitivity given by the stagnant-ice value.

L157-158: “Our results show that the Bindschadler and MacAyeal Ice Streams have high sensitivities to changes in basal friction at their grounding zones due to these ’sticky spots’ being key in controlling the ice discharge”

Do your results in fact show high sensitivity at the sticky spots, as opposed to high sensitivity in the ice plains generally? This is where it would be helpful to have a map of the inversion results to compare your sensitivity maps to. In addition, this is where it would be helpful to distinguish between the sensitivity to perturbations in drag and the sensitivity to relative perturbations in drag, as I discussed earlier.

References:

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