Author's response to reviewer V Tsai.

An abbreviated version of the reviewer's original comments is given in standard font, and our reply in italics. The full review is linked here:
http://editor.copernicus.org/index.php/tc-2016-149-RC1.pdf?_mdl=msover_md&_jrl=25&_lcm=oc108lcm109w&_acm=get_comm_file&_ms=52435&c=110422&salt=2038061631095973250

Authors general comments. The review from Dr Tsai was useful and we'd like to thank him. In the cases where we do not fully agree with Dr Tsai's criticism, his review enabled us to modify the text to make clearer our arguments. In particular some of our justification for our methods was embedded in the discussion in a way that wasn't very obvious to the reader, and we have moved some of this to the methods sections and hopefully clarified our arguments.

Dr Tsai's general comments

Dr Tsai had two main concerns – one about the numerics, and another about the applicability of the results and the relevance of the sliding relations considered.
A third issue concerned the melt parameterization.

Numerics:
"On the numerical side, it seems impossible for the steady-state spin up simulations to achieve a resolution-independent grounding line whereas the steady-state after forcing is resolution-dependent since both are just steady-state solutions. This strange result brings into question the validity of the results and needs to be explained."

Dr Tsai is correct in his recognition that the uniqueness of steady state solutions does not hold here. This issue is not unique to this study. While the corresponding real (continuum) system (if there were a real world glacier on a linear down-sloping bed) would almost certainly have only one viable steady state, ice sheet models, especially at coarse resolution, typically feature a region of locally stable steady states for a given forcing, and this region shrinks as resolution is refined. There is a discussion of a region of locally stable grounding line positions in Gladstone 2010 (JGR). That 2010 paper uses a flowline SSA model, but this issue of advance and retreat experiments not matching has been seen also in other models, and we are not referring hysteresis in the presence of an overdeepened bed. For example see Fig 3 in the MISMIP paper (Pattyn 2012).

We've added a couple of lines about this where spinup is mentioned in the results section.

Applicability:
"The authors choose to test 3 sliding laws (SR2-4) that were proposed 30 years ago, where the effective pressure dependence is inserted in an ad hoc manner, despite the existence of at least 2 more recent parameterizations of basal sliding (Schoof 2005 and Tsai et al. 2015) that are more physically motivated."

To the best of our knowledge none of the sliding relations in our paper was actually in use 30 years ago, although they are certainly motivated by developments back then, which recognised the deficiencies of the simple Weertman sliding relation in describing fast sliding flow, particularly in marine ice sheets.

Several researchers explored the introduction of an effective pressure denominator into sliding relations (e.g. BIndschader 1983). In the context of the fast flowing ice streams and outlet glaciers of the marine West Antarctic ice sheet, this was a response to the character of those flows: increasing velocity towards the grounding line despite steadily decreasing surface slope and driving stress. The linear relation between basal shear stress and effective pressure of SR2 was suggested by McInnes and Budd 1984, specifically to treat sliding in the approach to the grounding lines in West Antarctica, again as an effort to reconcile velocities, driving stresses and the height above floatation (although their velocity exponent in equation (1) was linear!). SR3 and SR4 are modifications suggested in 2014 for this paper. The motivation of SR3 is to provide the same behaviour as SR2 near the grounding line but to interpolate to the standard Weertman type relation further from the grounding line, where the concept of connecting basal water pressure to the ocean conditions would seem less justified.

This suggestion of historical irrelevance is beside the point – publication year is not a strong indicator of scientific justification. Glen's flow law is older than these sliding laws, was empirically derived, and is widely used today although more complex alternatives are available.

Having said that, we turn to responding to the claim that more recent sliding laws, stemming from the work of Christian Schoof in 2005, are "more physically motivated". The Schoof (2005) work is indeed physically motivated. It is a theoretical derivation based on certain assumptions. Those assumptions include the bedrock being of hard rock rather than deformable sediment and considering "cavitation" (another "old" idea dating back to Lliboutry) where the ice base locally detaches from the bed in the lee of obstacles. Even if the work of Schoof and the assumptions it contains are perfect from there on in, these initial assumptions certainly do not hold for the whole of Antarctica. For example, both Schoof 2005 and Gagliardini 2007 (following Iken 1981) make the assumption that tangential stresses between the bed and the ice are zero, assuming that there is always a thin film of water present, and thus the stress parameterisation arises entirely from normal stresses (what is called form drag in other flow situations) - which is why the max slope of the cavities is so important in determining Iken's bound. It is not clear to us that these assumptions will always hold true.

This is perhaps an appropriate point to comment that the criticism in Schoof (2005) regarding unbounded flow relations (of the form of our equation 1) is not about the presence of the effective pressure per se – it is about the risk of unrealistically high shear stresses arising through power law relations. Indeed that paper is not concerned directly with the main issue at the grounding line – the vanishing of effective pressure. Furthermore, it is clear that any relation with positive powers of effective pressure in equation (1) has a far better chance of satisfying a Iken (1981) or Schoof (2005) boundedness requirement on τ_b/N for finite values.

Regarding the interesting parameterisation in Tsai et al 2015 - this certainly provides a "Coulomb" limiting cutoff, but apparently simply assumes that the Weertman sliding relation *is otherwise completely correct. Several researchers, such as the early researchers on Antarctic ice stream flow such as Bindschadler (1983), Budd and his group, and others, and more recently Schoof (e.g. Schoof (2011) Journal of Fluid Mechanics) seem less convinced that the general character of basal sliding is settled.*

The form of the effective pressure dependence in the Budd laws was motivated from the output of laboratory experiments of ice sliding (Budd et al 1979). This starting point was taken forward in various different parametrisations in that group's pioneering Antarctic modelling – informed by what the then available data showed about connections between velocities, surface slopes, ice thickness and bedrock geometry. We think it is much fairer to say "empirically derived" rather than "Ad Hoc" in this case (was Glen's law not also empirically derived based on laboratory experiments?).

So on the one hand we have an empirically derived law and on the other we have a theoretically derived law. It seems clear to us that arguments could be made both ways. It also seems likely that the cavitation sliding law is not always going to hold true, and although Schoof's contribution to sliding relations is excellent work, we don't think it benefits the community as a whole for everyone to always use the cavitation law. At least not until there is a lot more direct evidence from real world applications to support it.

Having said all that, the choice is to some extent arbitrary, since the differences between these laws are smaller than the differences between parameter choices for a given law, i.e. I can choose parameters such that both laws look pretty similar, or I can make two different parameter choices within one law that give massive differences. Also the two laws are closer to each other than they are to Weertman sliding, and the choice of SR2-4 here give us control over the way basal drag is smoothed across the grounding line.
Given the weak dependence on sliding velocities in our various sliding relations, their local behaviour at the grounding line may not be that dissimilar to a Coulomb relation there.

In summary, it is not at all clear to us that any one of these sliding laws is "right" and another "wrong", and there are other sliding laws that we haven't considered at all here, but we don't want to see everyone in the community using the same law until we're really sure that it is always "right". We think that this discussion is far too long to put into the paper. We modified the paper to briefly discuss other sliding relations, to try and give perspective to refute the suggestion that 30 years ago people just made things up "ad hoc" and also to make the point that we are not advocating any particular sliding relation as being "better" than others just because we are using it, in the basal sliding methods section.

Basal melt parameterization:
"For the melt parameterization, the authors also seem to arbitrarily choose a smoothly decreasing melt rate for no apparent reason."

We consider it is quite clear why we present simulations both with and without the smooth decrease in melting near the grounding line. A smooth decrease is an opposite member to abrupt change, and appropriate for what is essentially a sensitivity study. There is of course a physical justification for high variation in amount of melting near the grounding line: varying strength of sub-glacial outflow, and this is already mentioned in the discussion.

However, it seems from Dr Tsai's comment that it would be appropriate to put some justification in the methods section where the basal melting is described, so we have done this.

We hope it was clear that the melting parametrisation gives greater melting at deeper ice shelf drafts via the "thermal driving" , to which we added a physically motivated optional factor that might reflect reduced heat transport in a sub-ice shelf cavity of limited water column thickness.

We also changed the MISOMIP reference from the GMD discussion paper to the peer reviewed one, since this is now published.

Response to line by line comments.

P2L17: Feldmann et al. 2014 should be cited in this paragraph as well.

Why? The context here is effective pressure dependency in sliding relations. I like the Feldman 2014 paper, but it is not about pressure dependency (or were there 2 Feldman 2014 papers?).

P3L1: "The starting point" could be rephrased for clarity.

We changed the wording here.

P3L9: Missing half sentence.

Oops! Thanks, we finished the sentence.

P3L16: Since the C's in the different equations are actually different, different letters (subscripts?) should be used. Otherwise it is confusing.

We've made this change, but have minor reservations because it doesn't really help with Table 1.

P5L8: Sentence wording is confusing. Sounds like a given model's horizontal resolution is spatially variable, rather than what is intended with 3 different models with different resolution.

Dr Tsai is right, thanks. Wording improved.

P5L21: Should this be 1000km rather than 150km?

Good point, fixed.

P6L12: There are multiple shear stresses. Need to specify which one.

This is sigma_xz, and this should be clearer now after minor changes to the text.

P6L15: Throughout this section, it would be useful if the authors made it easier for the reader to follow which simulation is referred to. For example, the acronyms could be spelled out once in the main text, the main text could refer to the colors in the figures, and also refer specifically to the figures and figure panels in which the results are shown.

We've changed the experiment acronyms to make them easier to remember and more intuitive. We've also introduced most of them in the text (experiment design section). We also refer to Fig 2 line colours in the text (results section).

P6L15: Whether or not the simulations (particularly SR2 and SR3) actually achieve resolution independence is difficult to see in Figure 3, partly because it is hard to see the step size. Since the steady-state solutions are the only ones that need to be compared, and contain the useful quantitative information, I would suggest making subplots for each SR# experiment that plot zoomed-in steady-state grounding line location (on y-axis) vs. mesh resolution (on x-axis). It would be preferable if there were more than 3 points, but if that is all that is computationally feasible, that is understandable.

A new figure has been created as suggested.

P8L12: Sentence is confusing. In fact, much of this page (L3-L20) is not coherent, and I would suggest the authors reword for clarity and flow.

Re-reading this it seemed fairly clear. We've made some modifications to this section, including removing some bits that were not essential to the argument we are trying to make. Hopefully this is now clearer.

P8L29: As commented earlier, since the sliding laws used are not physical, it is not clear that the results are easier to interpret than those of the Gagliardini study.

We don't understand this comment. We don't see where in that paragraph we claimed that our results are easier to interpret.

Fig.1: The point of Fig.1 is not entirely clear, and the gray overlay makes it impossible to see what is stated about there being little vertical shear.

Figure 1 seems to generate mixed opinions. It aims to show the different profile shapes due to different sliding relations. We have changed the grey to only outlines, and added the bedrock.