Author's response to reviewer J Bassis.

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-RC2.pdf?_mdl=msover_md&_jrl=25&_lcm=oc108lcm109w&_acm=get_comm_file&_ms=52435&c=110576&salt=1162785007638623258

Authors general comments. The long review from Dr Bassis gave plenty of food for thought and we'd like to thank him. There were a few places where Dr Bassis clearly thought that we were arguing a stronger point than we had intended (sometimes not fully defensibly). This helped us modify the text to avoid our remarks being interpreted as implying stronger statements than we intend. Some of his other suggestions could be fruitfully pursued as research projects, but seem to range rather far from the focus of our current paper.

Dr Bassis' general comments

There were five major points of concern:

1.
Models usually display "grid scale dependence" for one of two reasons. The first reason is that the resolution of the model fails to appropriately resolve the fundamental scale in the physics. A second reason a model may display "grid scale dependence" occurs when the model results actually fail to converge (or even diverge) as resolution increases.

We are referring to the first kind of grid scale dependence referred to by Dr Bassis, in which the difficulties of resolving the physical problem cause strong resolution dependence, and this resolution dependence is expected to converge with resolution. We don't see how discussing the other kind of resolution dependence (in which convergence does not occur) can strengthen the paper. Clearly we needed to make the aims of our paper clearer, and we hope that revisions have achieved that, without specifically discussing the second type of resolution dependence. Our goal is to explore the degree of resolution refinement necessary to get consistent behaviour in our simulations – effectively convergence to required precision – for a variety of sliding relations (boundary conditions for basal traction under grounded ice), and for gradual or abrupt imposition of ice shelf basal melt rates. Both these constitute parameterisations - of unresolved small scale basal processes in ice sliding, and of the delivery and transfer of heat from the sub ice shelf ocean in the case of basal melting. Some of our variants of these parametrisations proved amenable to achieving sensible modelling outcomes at much coarser resolutions than usually considered necessary in such studies. In other cases we demonstrate that much finer resolution is required. Unsurprisingly the need for much finer resolution generally emerges when there are discontinuities in these two basal boundary conditions.

The lead author has worked with several ice sheet models, all of which do appear to converge, and the reason why some are worse than others at a given resolution is due to the physical problem itself, and the approaches taken to deal with the problem. Essentially, a *smooth change in a forcing field (be it basal drag or sub shelf melting) can be much better approximated at coarse resolution than an abrupt change.*

The lead author has some familiarity with Schoof's work, and the convergence problems described can indeed be viewed as a purely numerical issue. That does not mean that it is not also a physical issue. With a different choice of physics that same numerical issue becomes more tractable at a coarse resolution, which is one of the main points of this paper. Neither our work nor Schoof's work is advocating a particular sliding law as being "correct". Both are in agreement that a certain level of resolution is required to give a result that is close for the converged solution. The current work indicates that this required resolution is dependent on choice of physics. The lead author does not see any reason why there might be a need to pick either numerics OR physics to "blame" for the difficulties, as the two are clearly related.

The lead author does not see how consideration of these two types of (lack of) convergence lends any clarity to the paper. However, since I believe that this paper and all the studies relevant to this paper deal with the former type (essentially convergent behaviour), I have tried to clarify what we mean by grid scale dependence without giving any mention of the second type grid scale dependence. Specifically we've modified the introduction and the first part of the results section.

2.

I have no objection to picking a subset of the infinitely large number of possible sliding laws. However, what is missing is a discussion of why the authors think the various generalizations of Weertman sliding are more appropriate for more realistic ice sheet models. This is especially true when considering the simple assumption employed by the authors that effective pressure is a linear function of water depth **all the way to the ice divide**.

This question is very similar to a question from the other reviewer, and we refer readers to our response to that review. However, we should acknowledge that Dr Bassis touches on relevant issues (see below). We have added a brief comment in the paper to indicate that the sliding relations in our paper – which are specifically intended as physically motivated modifications of Weertman sliding – do have origins in the quest for better representation of flow in real ice sheets. We also wish to clarify that we are not claiming that these specific sliding relations are generally more appropriate than other sliding laws, but rather that they are worth exploring, since arguments can be made both ways and we don't wish to see the whole community using a single sliding law.

Regarding the specific point about hydrologic assumptions and connectivity of all basal water to the ocean… actually we think arguments CAN be made for why this terrible assumption is actually better than assuming that basal drag is entirely independent of basal water, though clearly both assumptions are wrong. Traditional Weertman sliding is clearly wrong. Some of the other approaches and assumptions could be argued to be better, but none of them are great. And none of this is really the point of the current study. The point we want to make is that there is a relationship between model physics and resolution

requirements. We are not trying to say that everyone should use the same physics as we use in this study. Other researchers have to make their own decisions. But they may find it useful to think about basal physics of potential study areas and the implications for their study before picking a study area and before picking appropriate sliding laws and melt parameterisations. We do NOT want to advocate particular sliding laws or melt parameterisations, we just want to make the point that there are reasons why different forms of both may be relevant in different physical situations, and let others decide for themselves what to do with that information.
In short, we prefer not to enter into an argument about whether it is better to assume full hydrologic connectivity to the ocean or no connectivity at all. If reviewers/editor still think it essential then we will add such an argument, but we fear it could distract from the main point of the paper.
The importance of effective pressure in this study is near the grounding line. We think the reviewer agrees that the sub-glacial hydrologic system is more strongly connected to the ocean near the grounding line. Defining "near" in this context is both difficult and important, but well beyond the scope of this study.
The SR3 sliding relation can be regarded as progressively decreasing the influence of the ocean connection on effective pressure as the ice sheet becomes more firmly grounded inland.

The fact that one sliding law is more convenient for numerical modelers doesn't necessarily make it the most physically appropriate sliding law.

Exactly. We completely agree and we can't find anywhere in the paper where we claim the opposite. However, we have added text to the discussion to clarify that we do not advocate a particular sliding law or melt parameterisation.

For instance, the 4 sliding laws predict very different ice sheet profiles with different divide thickness, slopes and even concavity of the profile. Can we say anything about the reasonableness of the sliding law assumptions based on observed ice sheet profiles (Pine Island?, Thwaites?, Lambert Basin, Siple Coast Ice Streams?). My guess is that one might find different sliding laws appear more appropriate for different portions of the ice sheet?

This is exactly what motivated the researchers when modelling of the Antarctic ice sheet developed in the 1980's. We have added brief remarks about this to the paper. The concavity of the surface of fast flowing ice streams in West Antarctica highlights the deficiency of the Weertman relation (SR1) – ice stream velocities increase even though surface gradients and hence gravitational driving stresses are decreasing. The idea of a role for effective pressure (already introduced into sliding relations – e.g. by Lliboutry on theoretical grounds) and its identification with the "height about buoyancy" was considered a better representation (e.g. Bindschadler 1983) while Budd and his co-workers explored a variety of parametrisations to the limited data available. Dr Bassis also correctly observes that there are a variety of profiles associated with different fast flowing glacial systems. The Siple Coast ice streams *have very low profiles and a marked concavity compared to some relatively steep East Antarctic outlet glaciers.*

In this paper our concern is with marine based ice sheets.

3.

Selection of numerical parameter: Another question along a similar line of reasoning is how did the authors select the numerical "C" coefficients in the sliding laws?

They were chosen to give approximately similar grounding line positions after the spin up. This results in very different thicknesses to the grounded ice sheet. We've added this comment to the methods section where the sliding relations are introduced.
Yes, the size of the drag coefficient can make a difference, as shown in Gladstone 2012 Annals paper, but by far the biggest difference is the change across the grounding line, and this is dominated by the choice of sliding relation. We've added a comment to this effect in the discussion.

4.

Getting into the actual experiments, I'm puzzled by the fact that the authors spin up the models and find a steady-state that is independent of resolution, but then find that some of the forcing experiments do yield a dependence on grid resolution.

This is only really puzzling if you expect a single steady state for a given forcing (see also our response to the other reviewer to a very similar question). In brief, some models tend to give better convergence in advance and some in retreat. This is likely due to choice of discretization and special treatments of the grounding line. Our model seems to give better results in advance than in retreat. It is plausible that a change in the way forcing is applied could cause this to change, and indeed in the melt experiments our model does better in retreat than advance. This is already mentioned in the results section.

5.

Similarly, I would urge the authors to dig a bit deeper into the physics behind the results.

Actually, resolving the transition zone adequately is only part of the problem, possibly less than half the problem. There is a discrete feedback between the forcing and the state of the system. In a continuous system, advance of the grounding line is incremental with forcing. You can have a tiny change in forcing and this can cause a tiny change in grounding line position. This in itself results in a tiny change in forcing because of the increase in total basal drag (if we're talking about grounding line advance), which in itself causes further advance. Hence there is a positive feedback here between state and forcing. In the model a forcing perturbation of a certain size is needed in order to cause the grounding line to advance by one element or grid cell. A very small change in forcing SHOULD cause an increase in total basal drag because it SHOULD make the grounding line advance a bit. But it doesn't because the grounding line hasn't moved at all. This discretisation of what should be a *continuous positive feedback between model state and model forcing causes artificial stickiness in the model which is, to my mind, at the heart of the resolution problems. However, it is very difficult to separate this argument out from the simple issue of having sufficient resolution to resolve the transition zone itself, and I fear presenting this argument in its current form may not be helpful. We could put this in if reviewer and editor feel it adds value to the study.*

We've added some discussion to the discussion about the relevance of our study to using a real hydrology model.

Comments on presentation
The writing in the manuscript is generally clear, but stylistically, the manuscript contains many paragraphs that consist of one or two sentences. The abstract, for example, contains 4 distinct paragraphs! Normally, abstracts are a single paragraph. Traditionally, at least in the US, paragraphs are units that contain coherent ideas. Paragraphs depend on each other, but paragraphs themselves should be coherent. I found it jarring to have indentation that has little relation to the organization of ideas. For example, on Page 4 line 9 contains a paragraph with the single sentence starting with "This parameterisation is similar to that used in the . . ." The next paragraph begins with a definition of a variable. Neither of these are coherent or can stand on their own. This peculiar isolation of sentences into incoherent paragraphs was jarring to my American sensibilities, but this is perhaps a European or Cryosphere Discussion stylistic preference. I leave it at the editors and/or authors discretion as to whether this is stylistically appropriate.

We aim for our manuscript to be readable to all, even Americans. We're happy with the current multi-paragraph abstract (but willing to merge all the paragraphs into one if the reviewer and editor still feel strongly about this), but we've merged paragraphs in several other places, including the specific example mentioned by Dr Bassis. From a purely aesthetic point of view, note that the paragraphs will look longer when in double column format…

I was also flummoxed by the bestiary of acronyms. I was confronted with ISM, MIP, MIS all in the first several paragraphs and later hard to deconstruct SR1 and SR2, ALMW, ALMN amongst what seemed like many others. I have strong personal preferences to avoid all unnecessary acronyms and, unless the authors are forced to pay by the word, I urge the authors to use complete words instead of acronyms whenever possible. If this is inconvenient or impossible, then I would suggest a table that readers can easily refer to that provides us with a dictionary of acronyms that we can conveniently look up.

Surely ISM, MIS and MIP are very commonly used abbreviations? Also, they are all introduced together in the first paragraph, which is pretty convenient. Do we need a table for just these three? Or should we spell them out everywhere? We don't have a fundamental objection to either but don't fully understand the need. Does the editor have a recommendation here?

Some of the experiment names were indeed rather beastly. We've renamed most of these, mostly with shorter abbreviations now, and capitalised the relevant letters in their description in the table, so hopefully now the experiment names will make more sense. After revision we more seldom insert acronyms beyond the experiment labels, and we tried to accompany these by reminders which simulations we are discussing.

My last comments on presentation concerns the figures, which although physically appealing, were difficult for me to parse. For example, I struggled to understand Figure 1. According to the caption, panels show the steady and final states for the basal melting experiment. However, the top panel doesn't have the gray shaded profile. Why not? Moreover, given that the shaded gray profiles obscure parts of the initial profile, I would prefer to see this figure broken into two figures. The first would show the initial, post spin-up profiles. The second would show the final, post simulation profiles (with velocities). This would allow readers to more easily be able to see the differences in the shapes of the initial and final profiles and contemplate the role that the sliding law in modifying the morphology of the profiles and the transition zone. This figure also needs a scale bar to show horizontal distance (and possibly vertical elevation).

For Figure 2, why are only 2 steady states shown in Figure 2? Why not show all of them (as in Figure 1)?

Note that the top panel of Fig 1 has no grey profile because (as mentioned in the text) SR1 was not run in retreat due to melting. Fig 1 has been modified so that the grey profiles are now only outlines. Given that Fig 1 shows the whole domain for all four sliding relations, distance labels make the plot cluttered and we feel are not helpful. However, these could still be added if the editor and reviewers still think they are needed.

Fig 2 is intended to give a more detailed view of the two types of stress regime. The other simulations are not qualitatively different from the two shown. We prefer to show just what we feel supports our key points. However, as above, if the editor and reviewers still feel showing all four would be beneficial this could be done.

Figure 3 was difficult for me to parse. I would urge the authors to considering using color schemes that are more appropriate for the color blind and/or line types that don't depend as much on color.

We have reordered the legend to indicate the general progression of curves down the plot, and drawn attention to this in the caption. An alternative and easier to view colour scheme was not obvious.

Typographical comments:
page 1 near line 10: even with –> even with

You mean page 2? Changed, thanks

page 3 above line 10: incomplete sentence "These values for p and q are chosen for simplicity, and deviate" ????

Thanks, completed sentence.

From a physical perspective, Equation 2 is problematic. It assumes that the ice sheet has a basal hydrology sentence that is perfectly connected with the ocean all the way to the ice divide.

A basal hydrology scheme? Yes, this is physically problematic. So is the assumption that basal water pressure is constant all the way to the ice divide, which is implied in SR1. We don't think it is obvious which is worse (though we would argue the latter is worse), but we're not going to resolve this question here. If it were obviously worse than the alternative then we would agree that it would need some justification.
As already indicated SR3 can be regarded as representing a modification which effectively increases effective pressure faster inland, in as much as it approaches the Weertman result for firmly grounded ice.

The constant "C" in Equations (3)-(6) has different units in the suite of basal drag parameterizations. To avoid confusion, I recommend using a different symbol or appending subscripts to more clearly indicate that the numerical value of C (and its units) are not identical in all experiments.

They have subscripts now.

Line 4, line 15 and elsewhere: need a space between the number and unit "100m" –> "100 m"
After section 2.3 "1800km" –> "1800 km"

Hopefully we've caught all these now, thanks.

Accumulation rate: What is the motivation for the accumulation rate defined by Equation (11). This appears to predict linearly increasing accumulation with zero accumulation at the ice divide? Why not use a constant accumulation, which would seem to be more realistic for much of Antarctica?

It makes the ice divide boundary condition easier and it is more like the real Antarctica. Ok, linearity is not correct, but there is certainly more accumulation near the margins than in the interior.

Spin-up is independent of resolution. This seems to indicate that resolution only matters in some experiment types???

This is discussed above already.

Page 6, line 6 "focusses" –> "focuses"

Fixed, thanks!

Page 6, line 7 missing commas –> “The spinup simulations do however vary” –>The spinup simulations do, however, vary”

Fixed, thanks!

Page 6, line 21: “But even SR4b still shows significant resolution dependency.” –> sentence fragment, consider revising.

Revised!

Page line 23: “Since” indicates time (e.g., I haven’t slept since yesterday). I think the authors want “Because” (Because SR1 and SR4a . . .)

Yes, that is probably better, revised.

Page 7, line 4 missing space: “20ka” –> “20 ka”

Fixed, thanks.

Page 7, line 29, “Any resolution dependence in a model is inevitably non-physical. Ideally model behavior should converge with finer resolution.” This line is perplexing and indicates my fundamental misconception. Are the authors arguing that grounding zone position fails to converge with increasing resolution and is, hence, resolution dependent or are they arguing that grounding line position requires finer resolution than they have available. There is no particular guarantee that a numerical model with a priori specified resolution will be an accurate representation of a given set of partial differential equations. This is especially the case when the resolution of the model is more coarse than the fundamental scale of the system.

At this particular point we’re not arguing either, rather we’re discussing desirable properties of a modelled system. At no point do we attempt to argue for failure to converge in general, though we do not go to sufficiently fine resolution in some cases to demonstrate convergence. The main point of our paper is the exploration of situations where convergence – in our rather operational sense – is achieved with relatively coarse resolution, compared to previous studies – that typically use SR1. The cases that don’t converge across the range of our coarse resolutions simply identify conditions that are not amenable to the coarser resolution modelling.
This particular couple of sentences are not essential and we’ve simply removed them to avoid confusion.

Page 8, Line 10-15: I don’t think it is true that basal melting *requires* subglacial discharge. Because the ice near the grounding line is (usually) located at a depth that is much greater than the melting point of sea water, one expects melt near the grounding line of ice shelves with deep grounding lines, even in cold cavity ice shelves. In the traditional ice-pump theory, the cold melt water mixes with sea water and forms a buoyant plume. I’m puzzled by the argument that subglacial discharge is required to initiate the process? Are the authors arguing for massive super cooling near grounding lines? Is this usually observed? Models of submarine melt under ice shelves rarely include grounding discharge and yet predict realistic patterns of basal melt. Why is this if submarine discharge is a crucial component of the process? It also seems like the relevant length scale over which basal melt must go to zero is going to related to the characteristic width of the buoyant plume. Can this be estimated and used to better constrain the parameterization of basal melt?

Having re-read the original text we do not see where in these lines we claim that sub glacial outflow is required in order to get basal melting. It is a factor that influences basal melting close to the grounding line, which is important. We are not arguing for massive super cooling near grounding lines. We've modified the wording in this section. Hopefully it will be less prone to misinterpretation, but we're not very confident about this as we can't see where in the original text we claimed that subglacial outflow was strictly required.
We have revised the discussion of subglacial meltwater input, and clarified the observational point that there are high basal melt rates are often inferred near deep grounding lines (Rignot and Jacobs 2002). We have also included reference to Jenkins (2011) which explores the capability of subglacial melt at the grounding line to generate high melt rates close to the grounding line. Models of submarine ice melt are increasingly considering subglacial melt water discharge at the grounding line.