Reply to comments:

Point by point reply to comments (reviewer comments in the larger font size):

Anonymous reviewer #1

1. The biggest issue for a modeller to swallow is clearly the use of steady-state equipotential surfaces. While I can appreciate this was done for simplicity, if this paper is to be published, this assumption needs to be accompanied by a much larger caveats section. The limitation is most severe in Antarctica, rather than Greenland. While the authors provide a reference for near-flotation englacial water pressures on the Antarctic Ice Streams, there have of course been numerous boreholes in the ice sheet interior that are effectively "dry" (i.e. F = 0 in the cold ice encasing ice cores). Unlike Greenland, where most englacial water originates as surface melt and likely travels through the englacial pseudo "aquifer" to the bed following Shreve (1972) gradients (and perhaps cite something like Colgan et al., 2011 for F = 1 in Greenland), in Antarctica most subglacial water is produced at the bed and routed at the bed, beneath the cold overlying ice. Shreve (1972) assumed temperate ice, with surface melt being the water source. The cited work of Fricker et al. (2007) indicates small-scale pipe flow, rather than large-scale Darcy flow, is a key mechanism in routing subglacial water in the Antarctic. Although they appear to work, it is not immediately clear to me why steady-state equipotential surfaces can be expected to describe subglacial water flow in a setting so different from Shreve (1972). A second issue with steady-state implementation (from my experience at least), is that "water" can collect at an initial sink point, rather that potentially overtopping and aggregating with a larger water mass (which leads to comment two...).

Although we agree that the Shreve equation is a simplification of reality, we have taken this simple approach with the aim of a first order exploration of potential subglacial lake locations, and testing this against the data we currently have available. We were remiss not to provide a full and complete description of the caveats using this approach so have added in another paragraph describing these. Clearly, the use of the terms ‘modelling’ and ‘prediction’ are not favourably viewed by the modelling community , so we have also exchanged these where appropriate for softer wording (e.g. potential subglacial lake locations), including changing the title so as not to mislead people into thinking this is a full modelling paper. The reviewers scepticism about the use of the Shreve equation in Antarctica is, we believe partly misfounded. Many other authors have used this method to investigate the subglacial hydrological system in Antarctica (e.g. Siegert et al. 2007; Wright et al. 2008; Carter et al. 2011), and we reference these where appropriate. Indeed, it probably comes down to the issue of scale. At the metre-scale, where local processes dominate and so fluctuations in water pressure are likely to be highly variable, the Shreve equation is unlikely to prove accurate. However, at the ice-sheet scale and on a kilometre-grid the effect of the ice-overburden pressure and bed topography surely have to be the dominant factors. However, we agree that there are a number of limitations with using the Shreve Equation and have outlined these in a comprehensive caveats section in Section 2.1. We also make the point about scale in this paragraph.

2a. After searching in the text, I cannot find the minimum lake area threshold employed. From Figure 4 it appears to 1 km^2, presumably because that is the pixel size? Using BEDMAP2 to infer 1 km2 features seems to be a case of "over-precision" to me. Given my familiarity with BEDMAP2, I would think 5 km is the minimum resolution which potential flowlines might be extracted. But perhaps more important than acknowledging the "effective" (rather than "nominal") horizontal resolution (and it was only in the most densely sampled areas that flight lines were 5 km spaced), is the vertical uncertainty... Figure 12 of Fretwell et al. (2012) has >50% of East Antarctica with a vertical uncertainty of >100 m. Is there any way to propagate this into lake distribution uncertainty (i.e. see which lakes persist through a large number of random perturbations in which each grid point is perturbed by, for example, a random distribution around 0 m with a standard deviation of +/- 100 m)? Talking about 1 km2 water bodies when the elevation uncertainty at that resolution is +/- 100 m seems to do a disservice to the work.

Sasha Carter makes a similar comment to this (pasted below), so we deal with both these comments as one.

B. Although hinted at in the Author comment the paper would be improved if the authors took a more critical eye to the input data used. Although the Bedmap-2 ice thickness model and bedrock DEM (Fretwell et al., 2013) represents a substantial advance over its predecessors, it still suffers in a number of regions from insufficient input data and interpolation strategies that prioritized continuity and ease of reproducibility over precision. Of course work must go on despite this, but I encourage the authors to review the Fretwell et al., 2013 paper and figure out which of their results might be an artifact of limited data availability or the interpolation algorithm. Although Bedmap-2 was constructed at a 1 km resolution, but in reality the smallest feature that it resolves is actually 5 km. The reason that the modal size of many of the lakes Wright and Siegert., 2012 inventory was 5 km was more related to resolution issues than actual lake sizes. Indeed Carter et al., (2011) showed that though careful analysis of the line data that many apparent enclosed basins in the hydropotential were actually gridding artifacts, which ultimately did have an outlet. Also it is generally understood that surface altimetry south of 86S is of lower quality do to what is known as the “pole hole” south of the southern most limit of ICESat coverage.

Both these comments are very valid points and we have corrected the paper as follows.

1. We have re-calculated all our predictions using a threshold lake value of 5 km^2 as suggested by both reviewers, and discarding anything less than this as spurious given the resolution at which the data was collected.
2. We have taken on the first reviewers suggestion and explored the sensitivity of our potential subglacial lake locations to vertical uncertainty in the bedrock elevation by carrying out 50 random perturbations (on both the Greenland and Antarctic ice sheets) of the bed elevation DEMs, whereby each grid point was randomly perturbed using a normal distribution with a standard deviation equal to the elevation uncertainty (given by Fretwell et al. 2013 and Bamber et al. 2013). This has allowed us to explore whether the potential lake locations are robust features or likely to be artefacts of the bed uncertainty. Relating back to Sasha Carter’s point this has allowed us to be a bit more critical about which lakes are robust and which are likely to be artefact. In addition, we devote more effort to describing the regions where the results are probably flawed due to the lack of input data, such as the region between Support and Recovery glaciers and south of 86S. We present two new figures (1c and 5c) with these results and discuss their implications in depth.

2b. I would be interested in knowing the smallest subglacial lake area observed in the Wright and Siegert (2011) dataset. Presumably there is a physical lower limit for subglacial lakes, below which the body of water is "unstable" and will prefer to migrate to a larger body of water (e.g. analogous to the "smaller conduits flowing normal to the equipotential surfaces will thus be deflected toward existing larger conduits" of Hooke, 1989). With this in mind, I would suggest that the observed minimum lake area be used as a threshold, with the analysis restricted to only simulated lakes of greater than minimum area. That of course raises the question of what to do with the discarded "water" of the micro-lakes, which presumably re-organize themselves into more substantial meso-lakes in real life... a key process that a steady-state equipotential surfaces cannot capture, and a likely reason why the authors are finding two orders of magnitude more "unconsolidated" lakes than observed "consolidated" lakes.

The first part of this comment refers to the threshold value we should use to identify subglacial lakes, and is answered in the comment above.

It is certainly an interesting idea that small subglacial lakes are unstable and will prefer to migrate, although we have not come across this before and wonder how useful the analogy with conduits is. However, this is really beyond the scope of our study as it requires a model, which can account for dynamic changes in the basal water pressure. Having said this, another simpler explanation is that many of the smaller lakes haven't been found because they are beyond the resolution of RES. We make this point in the text.

3. A cold ice mask is used to discard lakes that form in areas of cold-based ice. Presumably, it is assumed that any water flowing into areas of cold-based ice refreezes? While zeroth order, that is certainly a fair representation of the effect of cold versus warm ice on the output side of the authors' water budget. What about on the input side of the equation? I do not see it stated in the text, but I would think that the same cold-based ice mask should be used to remove grid cells from the pool of "source" cells. When "cells that have more than 5000 cells flowing into them were used to arbitrarily define networks of meltwater flow concentration", what happens if 4000 of those cells were in cold based areas? At present, the paper reads as though all cells are possible water sources, but only a subset are possible water sinks, resulting in an apparent mass conservation disconnect. It should be clearly implemented and articulated that the only sources and sinks for subglacial water are the subset of warm-based ice nodes.

We agree that there is a mass conservation disconnect for the experiment where we include the basal thermal regime of Pattyn (2010) and so have re-calculated the subglacial meltwater routing using just the subset of warm-based nodes as per the reviewers suggestion. This is shown in Fig. 1b and the method is clearly described in the caption. The results of this are compared with the standard experiment and the implications of cold-bedded zones discussed (i.e. reducing the effective drainage basin and causing water to pond or freeze-on behind them)

4. I think the authors should use conventional terminology of "false-positives", "truepositives" etc, in their accuracy description. Continual use of phrases like "predictions that correspond to a known subglacial lake location" are just plain clunky. Liang et al. (2012) provide a fairly comprehensive statistical accuracy summary of lake hits and misses, with mathematically defined quantities like "precision" and "recall" that seem to be useful to the reader. Table 1 would definitely benefit from some of this clean language, as well as discarding the two 1 km columns and possibly the ALBMAP columns as well. It is not entirely clear to me what purpose the ALBMAP comparison serves, as it receives no mention beyond P1185 L22. I would think sufficient competing lake number estimates are contained within the paper without further confusion from the foray into ALBMAP.

Again we agree with the reviewer and have changed the 'clunky phrases' in the paper to reflect conventional statistical terminology. This includes use of the terms 'recall' and 'precision' as used in Liang et al. (2012), which was referred to by the reviewer. With regards to Table 1, we have changed the terminology and discarded all the 1 km^2 columns (due to the change in threshold to 5 km^2), the Albmap column (we agree that is not does add anything) and the 10 km column (which similarly does not add much to the discussion or key conclusions). We have also deleted the 'densely-surveyed block' rows as this has been superseded by our analysis of the vertical uncertainty, while having a separate row for the 'simulated lakes >= 5 km^{2}' is also superfluous because of the change in lake threshold. We have kept the smoothed BEDMAP2 column however, and added the 0 ka modelled time-slice on (see reply to comments by anonymous reviewer 2 and point 5 below). We agree with the reviewer that this has really helped to simplify the table, making it easier to refer to from the text.

5. I found the entire thread dealing with lake genesis during deglaciation to be somewhat tenuous. My impression is that the authors are using the observed DEMs of Greenland and Antarctica for the "present day" snapshot (Table 2), and then modeled ice geometry for the historical epochs. The direct comparison of modeled and observed ice sheet geometries is not permissible for these purposes. Observed ice sheet geometries have significantly more structure at the ice sheet surface than modeled ice sheet geometries, as models have a tendency to exchange bumps and valleys in the ice sheet surface with relatively clean parabolic profiles. As a result, there effectively is far less surface structure in the historical epochs than the present-day epoch. Given that equipotential sink points are sensitive to surface structure, it is therefore not entirely unexpected that your observed DEM produces more lakes than all of our modelled DEMs. I suppose to make the comparison entirely fair, the present-day DEMs should also be modelled (i.e. arbitrarily "smoothing" the BEDMAP2 DEM is not the same). But deglaciation DEM aside, what the deglaciation cold-based ice mask employed? Presumably epoch specific masks from Pattyn (2010) rather than assuming the ice temperature distribution has not changed in the past 20 ka?

We agree that a direct comparison of the present day and deglacial ice-surfaces is not valid, but I think this actually misses the point of what we are trying to achieve and misinterprets our analysis. We actually restrict our analysis of the change in subglacial lake formation to just the deglacial time-slices, i.e. purely an intercomparison exercise (the gross values themselves are not really relevant). The deglacial time-slices are only talked about in the same context as the present-day DEMs with respect to the ice-surface flattening feedback, and in this case the lower recalls and frequencies are not explained away as indicating fewer lakes during deglaciation. It is instead the lakes that do not result from surface irregularities, which we are interested in here. So it is a relative comparison – i.e. despite being effectively smoothed the deglacial time-slices still predict a better than random number of present-day subglacial lakes, and that the jump in recall is much greater with the smoothed present-day ice-surface than the standard experiment. Indeed, the reviewer suggests the only way to make a fair comparison would be to model the ice-surface. Actually we have the 0 ka modelled output from Whitehouse et al. (2012), so have included this in our analysis. To help the reader fully understand our sections on the deglacial evolution of subglacial lakes we have extended the methods section (section 2.1) to more clearly explicate the methods used to derive deglacial hydraulic potential surfaces and the caveats associated with it.

In terms of the cold-bedded ice mask – we only use a mask of the thermal regime for the present-day ice-sheet in one experiment to see how this affects the precision (i.e. we know that in reality some of the lakes will be in regions of the bed where meltwater is not produced so we thought it would be useful to offer a first order approximation of this). The bed of each of the deglacial time-slices are treated as uniformly warm-bedded. As we are only comparing these deglacial time-slices we believe this is a reasonable assumption at this simple level of analysis.

6. The apparent lack of any validation for the Greenland modelled supraglacial lake locations is somewhat surprising (galling even). While a subglacial lake inventory does exist for Antarctica, in many ways Greenland is a better validation target as it is smaller and better observed, and its glaciological setting is more closely aligned with the assumptions unpinning Shreve (1972), and hence the steady-state model employed in this study. While the observed proglacial hydrological outlets of Lewis and Smith (2009) are a charming addition, the authors do not provide any insight where subglacial water has been observed in the interior (aside from citing Oswald and Gogineni (2008)). There is abundant radar data available for Greenland which has the potential to constrain the modeled lake inventory... why not at least try to demonstrate the existence of the inferred 250 km2 Greenland subglacial lake that will be new to science?

It would be great to be able to try and validate our results against radar data, but it is beyond the scope of this paper. The aim of this paper is to outline the potential subglacial lake locations and discuss the implications of this. Certainly in the future we aim to go back and see if we can find any of these lakes – indeed we state this in the paper. Having said that, since this paper has been published in TCD we have had a lot of interest from other glaciologists keen to try and test our predictions, including a paper currently under consideration in Science, whilst we have found one paper that does infer a subglacial lake location beneath the Greenland Ice Sheet (Ekholm et al. 1998), and this coincides with one of our potential subglacial lake locations. This match is now described in the text.

The fact that a full comparison of our Greenland results to geophysical data is beyond our scope here, and considering the current length and complexity of the paper, we have not attempted to include such a comparison. We hope that the editor agrees with our justifications for this.

Specific Comments
Title – "Predict" implies the subglacial pathways of the "future" to me. Perhaps plain old "modelling" is more appropriate?

We have changed the title to “Potential subglacial lake locations and…”

P1178 L6 – This "two orders" of magnitude discrepancy with observations throws up a flags at the very start of your writing.

This is true, but even with the 5 km^2 threshold this result remains. And indeed, even with the improvements of BEDMAP2, huge regions of the bed remain unsurveyed so we do not think this is necessarily a sign that our analysis is wrong – it merely reflects the huge amount of work still to be done. However, we have removed this sentence from the abstract, and simplified it to bring out the key points of our analysis.

P1181 L13 – Pw and Pi reversal?

This sentence has been deleted from the revised methods section as part of our attempts to simplify the methods so that the reader can clearly how we have produced the results.

P1182 L24 – Some description is needed of this smoothing (i.e. filter type / width)

We have included details about the type of filter and the width of the window used. We have also included a present-day modelled ice-surface as an alternative to the smoothing algorithm (as ice-models tend to produce very smooth surfaces).

Section 2.3 – Was there no thermal mask employed for Greenland? I see the authors have posted a comment on replacing the (now seriously) outdated Bamber et al. (2001) DEM with the new Ice2Sea DEM (Bamber et al., 2013) instead, which has been available to the community in alpha version for two years. This appears to change their results and discussion of the Greenland Ice Sheet significantly.

As far as we are aware there is no thermal mask comparable to the Pattyn (2010) output for the Greenland Ice Sheet, so do not include one. As the reviewer notes, we have updated the results of the present day Greenland Ice Sheet with the latest DEM. Although it changes the results (i.e. we now get many more lakes) it does not actually change the key discussion points, such as, that there are still less lakes per unit area under the Greenland Ice Sheet, compared to the Antarctic Ice Sheet. Certainly the Bamber (2013) DEM is much improved, which we think justifies updating the results with the latest dataset.

The use of non-scientific terms like "success rate" (P 1185 L26), "hit"/"miss" (Figure 1) "cut" (Figure 4) could really be phased out in place for proper statistical terminology.

We have phased out the non-scientific language for proper statistical terminology as discussed above.

P1186 L7 – I think this pseudo Monte Carlo needs to be bolstered into a real Monte Carlo that also takes uncertainty in bedrock elevation into account during your random simulations.

As explained above, we think this is a great idea and have fully implemented this.

P1190 L11 – Or perhaps the lack of difference whether or not the depth of the lake is included in the input DEM merely confirms that the horizontal dimension of subglacial lakes far exceeds the vertical dimension (i.e. they are wide and flat, so it doesn't matter much whether or not DEMs are corrected for them).

This is an interesting point, which we have included a sentence about in the paper (section 3.1, last paragraph). Indeed, we find that the more robust lakes are also the largest, which confirms the reviewers suspicions here.

Is my interpretation of Figure 4 correct if you are saying there are >5 undiscovered subglacial lakes that are _100 km2, and one 250 km2 subglacial lake, beneath the Greenland Ice Sheet? I find it difficult to accept that researchers have so far failed to find a 10 by 25 km water body beneath the Greenland Ice Sheet...

This may be the case, but we feel obliged to publish the entire suite of results. We hope this work will then stimulate research to try and validate or dismiss our predictions. This does not mean to say that all our potential subglacial lake locations are correct. In fact there will almost certainly be errors. However, we have added more discussion on those lakes which we think are robust and those that may be artefacts of the bed grid. This has been helped enormously by the inclusion of the random bed perturbation experiments, which have allowed us to dismiss many of the potential lake locations as spurious (including the largest lake predicted in Greenland).

Table 1 presents seven estimates for the number of lakes beneath the AIS and obliges the reader to wade through the text to figure out which DEM/resolution/temperature mask is most suitable. I think those assumptions should be clearly articulated in the text and a more concise package of data presented to the reader here.

We agree that this table is confusing. We have therefore cut-down the text to display only the most suitable results as suggested by the reviewer in the main comments above.

Section 6 – Going into bullet form gives this final portion of the manuscript an unfinished appearance. I believe the convention in TC is to use paragraph structure.

We have changed the bullet points into paragraphs as suggested.

Figures – The structure, quality and ease of reading are quite variable.

We have updated many of the figures commensurate with revisions in the text.

References – Why do so many references have a string of hyperlinked page numbers following them? Is this a new TC feature?

Anonymous reviewer #2

1. Subglacial drainage cannot be decoupled from the ice flow model: GIS is not a modelling technique. Bringing data together from different sources and doing an overlay analysis is not the same thing as a dynamic model that solves for ice flow dynamics, thermodynamics and basal hydrology based on conservation laws. For instance, using model data from a thermodynamical model (using different initial datasets) cannot be simply applied as an overlay operation with other modeled properties; best would be to use a thermomechanical model and calculate the full flow field, temperature field, hydrology). The same applies with the use of paleo model results that are not in full agreement with the datasets used.

This is very similar to point #1 from the first reviewer (also see points above). However, we strongly argue that this is missing the point of our paper. The coupled ice-modelling suggested by the reviewer is still in its infancy: to our knowledge the community is a long way away from models that could be used to investigate the large-scale controls on subglacial lake in the way that we have been able to use our simple routing model. The long-term aim of incorporating physics-based hydrological models into large-scale ice-sheets model is a worthwhile one, but it is not our aim here. In fact, it should be remembered that all mathematical models are crude approximations of the complexity of reality. We chose the simplest possible model for our investigation and, given our broadly qualitative analysis of our results, we feel that this choice was appropriate. The advantage of our approach is we can quickly (and at high resolutions) test the results against known subglacial lake locations in Antarctica and then apply the model to other ice sheets. We hope this will stimulate researchers to then go and try and find these lakes (and we have had a lot of interest in our results already), to collect data that can be used for more advanced modelling and to stimulate theoretical advancements. As stated above, our results are not comprehensive predictions, rather potential subglacial lake predictions derived from 1st order approximations. We accept however that there are limitations of the Shreve equation and have therefore included a comprehensive caveats section to indicate to the reader that ours is a first approximation approach with a number of potential error terms.

We find unhelpful, the reviewers comments regarding what and what is not a model. A GIS, being capable of executing simple calculations, such as evaluating the hydraulic potential of an ice sheet, can play a useful part in a modelling approach. However, not wishing to confuse the reader, we need to make clear from the start that we are not solving a dynamic model that solves for ice flow dynamics. Hence, we have softened the language, clearly explicated this fact and included a comprehensive caveats section.

2. The way smoothing is introduced is very unclear: the authors apply smoothing of the surface to get rid of known positions of subglacial lakes (which normally flat out the surface because of the vanishing friction at the ice-water interface). However, such smoothing will be a function of the size of the lakes. In order to get rid of the flatness of subglacial Lake Vostok, a substantial amount of smoothing is necessary. Maybe this is why Lake Vostok can be easily retrieved. The authors should clearly explain what the smoothing effect is on the routing of the subglacial water.

This is a good point and one we had not considered. To rectify this we now clearly explain in the methods section the choice of smoothing and the windows used to carry this out. We also explain the potential bias described by the reviewer above and refer back to it in the discussion. To try and counteract this though we also use a modelled 0 ka time-slice. Because models tend to create very smooth ice-surfaces this offers a useful comparison to check for any bias. We have also included a section describing the effect that each of the smoothing methods has on the routing of subglacial water.

3. In fact the whole methodology section is not well worked out. For instance, what is the rationale behind the choice of convergence of 5000 grid cells to define a subglacial lake position? What is the sensitivity of this choice?

We have made significant changes to the methods section, including, the addition of some caveats, a more up-to-date statistical terminology, a revision of the way our approach is outlined, a detailed description of how we implement the deglacial time-slices, a clear justification of our method, and thorough descriptions of the methods used in ArcGIS. In terms of the rationale behind the convergence of grid cells – we have deleted this from the methods and instead included a description of the number of grid cells we choose in the captions. The choice is purely arbitrary (i.e. at what point does a brook become a stream become a river?) and just for visualization purposes. However, we wanted to keep this constant where possible.

4. The routing algorithm stems from GIS software, and no further details are given. In fact, there are many ways by which routing can be defined. See therefore discussion in papers by Wright et al., as well as LeBrocq et al (2006). While the latter is on balance flux algorithms, the flow concentrations algorithm is basically the same. Results can be quite diverging depending on the method used. Very little (or no) information on this is found in the paper.

We have added in a section in the methods clearly describing the steps by which we derived the hydraulic potential surface and routed and ponded water using a GIS – i.e. we simply routed water down the largest hydraulic potential gradients, and then calculated drainage pathways by defining the cumulative number of all cells that flow into each downslope cell. We have also referred to other papers which have taken similar approaches.

5. The fact that the Greenland ice sheet has lost subglacial lakes through drainage since LGM has already been pointed out by Pattyn (2008, JGLAC), where a full-Stokes model including basal hydrology and subglacial lake discharge was used to test the sensitivity of ice sheet geometry on subglacial lake drainage. It was shown in that paper that the mean surface slope of an ice sheet (or region of an ice sheet) is a decisive factor in subglacial lake stability, and therefore argued that the Greenland ice sheet lacks an extensive network of subglacial lakes because of drainage due to deglaciation (hence larger surface slopes).

We have cited the paper by Pattyn (2008) and discuss his findings in the context of ours, both in terms of the difference between the present day Greenland and Antarctic ice sheets and also in terms of the deglaciation of the Greenland Ice Sheet.

6. Only 36% of BEDMAP2 grid cells is covered by a measurement. However, the major effect on subglacial lake position and basal hydrology is the surface topography. The bed is known to a much lower resolution. Of the 36% coverage, most subglacial lakes lie outside of these regions, mostly in the interior of the ice sheet, which is either harder to reach or not so interesting as fast-flowing ice streams. In reality, the ice sheet surface reacts to the bed conditions (and not the other way around). This means that any paleao reconstruction or other model run output deals with a completely different spatial resolution, depending on both the bed resolution and the spatial resolution of the model through which the calculation is done. this could introduce a serious bias that has not been discussed.

This is true and is a limitation of our model, not discussed before. We have therefore added a description of this limitation in the caveats paragraph in the methods Section 2.1. However, as previously mentioned, we deal with time-slices from each palaeo-reconstruction in isolation (it is the relative change we are interested in), which therefore helps us to circumvent this bias to some extent. To try and reduce this further we use the old Bamber et al. (2001) bed topography when calculating the potential surfaces as this was the grid used to run the Simpson model.

7. The GIS technique is capable of retrieving 70% of the known lakes. However, the more lakes you predict, the higher the probability that a predicted lake coincides with an existing one. This sensitivity should be tested.

We already test to see whether the large numbers of lakes are able to produce similar recall percentages when the known lakes are randomly distributed. Our findings suggest a large reduction in recall, which gives us confidence that our results are robust and the lakes are being found for the right reasons, and not just because of the large number of predictions! The precision also deals with this (i.e. the number of false-positives), although this is muddied as we don’t have a complete inventory of known lakes so is not a clear-cut test.

Smaller remarks

P1186: i wonder why the model is better at predicting subglacial lakes underneath ice streams. Is it because those are susceptible to discharge and are detected by IceSat changes at the surface (not necessarily detected via radar) and many more could potentially exist, while for the interior ones, the majority has been detected via radar, which in some areas has been done on a quite detailed scale.

This is an interesting question. We think this recall rates are largely based on the accuracy of the dataset, and indeed many ice streams are surveyed with airborne grids. We have included a sentence discussing this possibility in Section 5.1. Certainly there are differences (bias) in the observations as well, although these are not mentioned in the paper and therefore probably slightly outside the scope of this paper.

P1187: The pathways of the deglacial time slices are very model-outcome dependent; the problem is that the pathways do not interact actively with the ice sheet during the deglaciation and melt patterns provoked by hydrological changes may influence the surface topography at any stage as well. Therefore, the description of the results given is very outcome dependent and lacks any broader discussion.

We agree that the time-slices are very model dependent, but our model is not capable of describing the coupling between hydrology and ice dynamics. To mitigate this effect we have only compared model time-slices with other model time-slices and only discussing the results qualitatively. We have also toned down the way we present the results (i.e. using phrases such as 'likely' and 'suggests that' As mentioned before, we have added the caveat that the calculations are not dynamically coupled to ice behaviour.

P1190: The authors should question the reason why Lake Vostok can be reconstructed in the experiments based on the lake/ice reflector: this surface is in hydrostatic equilibrium, for which the hydraulic potential reaches a minimum. I find their explanation quite difficult to understand and slightly outside the scope. One thing has not been mentioned: smoothing will change this hydrostatic equilibrium ad hoc, and probably only the larger lakes will come out, while smaller ones are automatically removed by that procedure.

Just because the ice over Subglacial Lake Vostok is in hydrostatic equilibrium does not mean it will not show up in the calculations when we use the lake surface as the bed elevation. It just means that we will be mapping the lake's current 'freeboard', i.e. how much space left it has to fill up before it 'over-flows' its hydropotential surface lip. This is the very point we are trying to make in this paragraph.

We have responded to the second part of this comment in the discussion above.

<u>Comments by Sasha Carter</u>

A. Overall, the authors generally explain everything they are doing, which is great, but at times they make it seem like they are doing more than they actually are (i.e. streamflow routing does not coupled to numerical ice sheet model for either AIS or GrIS). Indeed Johnson and Fastook 2002 showed the importance of coupling between these two for reproducing the inferred evolution of the Laurentide Ice Sheet.

We agree that we have maybe mislead the reviewers (see also replies to other reviewer comments), in terms of what we are actually doing. As well as toning down the language used we have also added a comprehensive caveats section in the methods section (section 2.1) where we clearly state our analysis is not coupled to numerical ice-sheet models. We have also explained more clearly the routing method implemented in Arc to derive the drainage pathways.

Indeed Carter et al., (2011) showed that though careful analysis of the line data that many apparent enclosed basins in the hydropotential were actually gridding artifacts, which ultimately did have an outlet. Also it is generally understood that surface altimetry south of 86S is of lower quality do to what is known as the “pole hole” south of the southern most limit of ICESat coverage.

We agree that many of our potential subglacial lakes probably only form due to gridding artefacts. Although going through every single lake would be a mammoth task at the ice-sheet scale, the use of the bed uncertainty analysis has helped us to weed out those lakes that we think are artefacts. For instance, we suggest that all lakes that occur for <50% of the bed elevation uncertainty experiments should be looked upon with scepticism. Likewise, we have also devoted more effort in the results and methods section to outlining those areas where the data quality is poor and therefore where we have less confidence in our lake predictions. This now includes discussion of the “pole hole”.

C. It would be useful for the authors to think a bit more deeply as to why lakes form where they. The model may not be able to address all of these issues and if so it would be useful to know what might be misses. Tabacco et al., 2006 provides a nice review on several classes of physiographic settings in which subglacial lakes form. A review of Fricker et al., 2010 and Sergienko et al., 2011 would show that an entire class of lakes can also form in the lee of areas of high basal traction or sticky spots, and originate entirely from ice dynamics.

This is a good point. In the caveats section we now include a discussion of those subglacial lakes that the Shreve equation would not be able to identify, including the one mentioned above, and also those associated with differences in the basal thermal regime.

D. Obviously the choice of F=1 or 0.75 seems a bit too arbitrary and could either use some more justification or find a way to show how varying the parameter matters over a larger spectrum of values.

We agree that the choice of F=0.75 is quite arbitrary and does not add much. And as we do not offer a comprehensive analysis of the F-values either we have deleted the data pertaining to F=0.75, and actually removed F altogether, instead choosing to focus on the effective pressure, which is easier to explain in terms of the limitations of the Shreve Equation.

Specific comments:
P1179 Line 26: What do you mean by conceptual breakthroughs?

We have changed this to theoretical advances, which we hope is clearer.

P 1181 Line 5: a number of references predate this (Alley et al., 1989; Tulaczyk et al., 2000)

This is slightly confusing as the Shreve reference predates both the references given by Sasha Carter. We have therefore left it as it is.

P 1181 Line 12: make sure you emphasize the sensitivity of water is 10 times more sensitive to surface slope, but bed slopes can in places be 10 times greater than the surface slope.

We actually make this very point in the next paragraph.

P 1182: The case made for hydraulic minima equally lakes is oversimplified. They should expand more on this matter, specially the implementation in ArcGIS.

We have added in an extra couple of sentences expanding upon the method by which the hydraulic minima were used to derive lakes, and also the implementation of the routing method used in ArcGIS (see comments by reviewer #2).

P1182 Line 27: Glimmer (Rutt et al., 2009) should be cited

We have cited Rutt et al. (2009)

Page 1183, the wrong figure of Pattyn, 2010 cited; they probably meant Figure 2 instead of 1b.

We did not intend to cite a figure in Pattyn (2010); we were actually referring to Fig. 1b in the paper.

Page 1185: Where is the justification for the cold-bedded ice masks? Also on this page, why is 2500m of ice the delineator between ice streams and divides? Please provide citations for these seemingly arbitrary physical definitions.

We include a justification for the cold-bedded mask in the methods section. The arbitrary choice of numbers to delineate between ice streams and divides has been removed. Instead we now refer to specific regions of the Antarctic Ice Sheet – for instance, the Siple Coast subglacial lakes vs. the Recovery Glacier subglacial lakes. We also qualitatively assess the differences between the recall of known subglacial lakes beneath ice streams and elsewhere and try and explain this (see also point and reply by reviewer above).

Pages 1186-1187: We again stress that the ice models used and the interface flow are not coupled processes so it's hard to take what they say here as seriously as they intend.

We agree there are limitations to our approach, and we now include a full summary of the caveats in the methods section (2.1). However, the reviewer I think is misreading our intentions – we are not outlining a definitive series of maps of subglacial lake locations during deglaciation, rather we are exploring the relative evolution of subglacial lakes and their sensitivities. This is certainly the tone of the discussion section, which makes no definitive statements about subglacial lake locations and drainage pathways apart from where they coincide with palaeo-studies, such as in Palmer Deep. Related to this, some regions of the bed are likely to be more susceptible to drainage switches than others, and this can be explored in our model. We therefore stress in the methods that our aims are to look at the "relative comparison of potential subglacial lake locations and their evolution during deglaciation".

P 1187 Line 1: You may want to look to Catania et al., 2012 and Conway et al., 1999 for a brief history of the Siple Coast as these works seem to compliment what you're doing and I don't see them referenced.

We have added in an extra sentence at the end of this paragraph relating our work to that of Catania et al (2012) and Conway et al. (1999).

Page 1191 Line 1-5: You mention that the 3rd option is least preferable but I don't know that that would be such a bad thing. It would indicate that their technique of standard GIS watershed delineation is not appropriate for what they are doing. Not a problem in my opinion.

We have deleted this sentence.