Response to reviewer comments

First of, we would like to thank both reviewers for excellent and insightful comments that have greatly improved the quality of the manuscript.
A temporary version of two new figures is at the end of this document.

Anonymous Reviewer #1

General Comment:
The sensitivity experiments comprise changes in lake size and different flow law ex- ponents. Lake discharge is also simulated for different lake sizes. Since the results appear to show that the most critical area where changes occur is at the boundary between the lake and the surrounding ice sheet, i.e., a boundary between free slip and high friction, I would have liked to see a sensitivity on the contrast between both. The used friction factor of C=1e13 is a rather high factor and three orders of magnitude larger than the friction one may expect under ice streams. It seems necessary in the general discussion of the observed features, such as the hummocky surface anomaly and the viscous heating at the contact between the lake and the surrounding ice sheet, that the sensitivity is enlarged to different factors of of slipperiness. This would not only make the paper richer, it would also explain features of other types of subglacial lakes encountered in Antarctica (and Greenland). A question that arises is whether at lower C-contrasts the hummocky feature is still prevalent, whether the viscous features play a similar role or not, and if internal layer anomalies show similar characteristics.

Answer: These questions have now been addressed with a new sensitivity study with a lower C-contrast and two different values for ice thickness (1500m as before and 3000m). The new simulations use $C = 10^{10}$, more appropriate for streaming conditions, and a smaller bed inclination ($\alpha = 0.1$). In contrast to simulations previously presented, most of the velocity obtained at the surface arises from sliding at the base. Consequently the transition in velocity from sliding outside and over the lake is much less sharp. Velocities only increase slightly over the lake ($\sim 7\%$) with very little additional strain heating in the transition zone and no temperate layer forming. The deflection of isochrone layers is also very small and hardly noticeable when compared to previous simulations. Two new subfigures will be included in the revised manuscript. One showing surface profiles for the new simulations and the other showing a comparison between steady state isochrone profiles for 3 different simulations with 1500m ice thickness (the new $C = 10^{10}$, and the n=3 and $\eta = 10^{14}$ cases already presented) The dip and ridge features still form but with a much smaller amplitude.

Detailed remarks:

P3860, L8-9: rephrase "A question is what effect this would have on internal ..."

Answer: Changed to "A question that arises is ...."

P3860, L33: past discharge events?

Answer: Changed to "past drainage events"

P3862, L14: The hydrostatic equilibrium of subglacial lakes may well be a function of their *size. Since you use a full Stokes model, you can demonstrate whether this is the case for event the smallest lakes sizes in your sample.*

Answer: We do use a full-Stokes model but in our model the lakes are already assumed to be in hydrostatic equilibrium. The ice sheets base is flat and the weight of ice everywhere fully supported at the base, including over the lake. This wasn't clear enough in the paper so the text in the manuscript has been changed to include the complete description of the basal stress boundary condition.

P3868, L8: Are 15 layers really enough? Have you tested this with more? Moreover, in the discussion you base your analysis on just two layers with temperate ice (70m). I am afraid that the undersampling may have an influence on the results.

Answer: At present we have tested this with higher vertical resolution (2 times higher at the base, with 4 layers covering the first ($\sim 70m$)), but only solving for enthalpy, keeping the steady state surface and velocity field fixed. These simulations indicate that the thickness of temperate ice is somewhat overestimated in the manuscript at 70m (with just 2 temperate cells), but nevertheless that a temperate layer should exist (for the given surface and velocity field that is). We can be sure of that because even when keeping the temperature fixed everywhere at the pressure melting point it results in the 2nd cell above the base being temperate in the transition zone. This of course does not necessarily mean that there should be a temperate layer present if velocity, surface and enthalpy were all evolving simultaneously, but these are very computationally expensive simulations and I'm not sure that we will be able to do much better. The chosen sliding law has a big effect of course. We did however try to be a bit cautious when interpreting these results and only state in the manuscript that a temperate layer could potentially form (for a real lake) not that it necessarily would. This has been further addressed in section 4.1 where a discussion on the limitations of these results has been included and the wording in the abstract and the conclusions made a bit more cautious.

P3868, L25: What considerable distance? be precise. Is this so-called considerable distance a function of ice thickness/size of lake? Does slipperiness play a role?

Answer: What we mean here by a considerable distance is around 50km for the L_M size. And yes, it is a function of all of the above; ice thickness, lake size and slipperiness outside the lake. In general, the influence of membrane stresses, or longitudinal coupling increases with increased slipperiness, ice thickness and lake size (Cuffey and Paterson 2010, Hindmarsh 2006, Kamb and Echelmeyer 1986), but the effect of such a transition in basal friction as the one we model here also diminishes as the difference in friction outside and inside the lake decreases (the C-contrast) as can be witnessed by the new sensitivity studies that you proposed. The wording in the manuscript has been changed in order to further elucidate these facts and new citations added.

P3869, L16: I suppose that this concentrated deformation is very much a function of the basal sliding law and coefficients used outside the lake. The value of C is also very high (3 orders of magnitude higher than that of an ice stream) which implies a sharp transition between the ice flow across the lake and the surrounding ice. See my general remark.

Answer: True. It is quite high. It was meant more as representative for areas at the onset of ice streaming, although we do state in one place in the paper (end of the introduction) that the aim was to model a lake underneath an ice stream. This was an oversight. Nevertheless, it is still maybe 1-2 orders of magnitude too large. Higher values than what you propose might still be justifiable though because the bed surface is completely flat, unlike any real bed surface which would be highly irregular, with obstacles of all sizes. Therefore, no resistance is provided by "large" obstacles (compared to the grid size) over which the ice would have to deform and which would add to the available energy for internal heating. Your point is a good one though, which is why we decided to do the sensitivity experiment you suggest. The results indicate that if you lower the C-contrast such that the majority of surface velocity is derived from basal sliding then the increase in surface velocity over the lake is relatively small ($\sim 7\%$) for the case presented, and that whatever strain heating that occurs in the transition zone is not enough to create a temperate layer. Two new figures are presented in the revised MS, one showing surface profiles for the simulations with lower C-contrast and the second showing steady state isochrone layers for low and high C-contrasts and the fixed viscosity case.

P3870, L8: Could the dip and ridge feature also be a function of bridging stresses in the full Stokes, besides a strain-induced flow-law effect?

Answer: It is possible that bridging stresses play a role, especially for small lakes, but it's not something that we can shed much light on without changing the model set-up as the lakes are assumed to be in hydrostatic equilibrium and the ice column fully supported everywhere by the normal stress at the bed.

P3870, L27: The travelling wave is surely an interesting phenomenon and would have had more value if this has also been detected in radargrams. I understand that it is probably difficult to detect when topography is rough. This would certainly be an added value to the paper.

Answer: Yes, we agree. Unfortunately we are not aware of any study where this has been detected and documented. In most cases we would imagine that previous drainage events would be difficult to detect and probably not immediately obvious from radargrams alone, as explained in the manuscript. Most likely a numerical model would be needed in order to isolate the effect of any past drainage on isochrones from the effect of varying rheology and basal topography.

P3871, L18: See previous remark on undersampling (2 vertical cells seems to me very narrow to base conclusions on).

Answer: see previous remark for answer

P3872, L13-14: remove both commas

Answer: commas removed

P3872, L24: change "everywhere the same" in "constant".

Answer: changed as suggested

P3873, L6: The sharp transition in sliding velocity is not tested. It is only an experiment with a sharp transition and should be compared to a less sharp transition to make this hard.

Answer: We have now compared two cases with different C-contrasts and included a new subfigure in the manuscript. For lower C-contrasts the amplitude of the dip and ridge features is much smaller and very little additional strain heating is introduced at the lake edges. No temperate layer forms. See answer to general comment.

P3874, L24: replace "so this", by "which"

Answer: changed as suggested

G.J.-M.C. Leysinger Vieli (Reviewer #2)

Specific Comment:
For the first experiment suite, a steady state experiment, the resulting surface velocities and topography as well as the internal layers are obtained for different sized lakes. The effect of a different viscosity on the surface topography and the vertical distribution of horizontal velocity is studied. However we do not see how a different viscosity affects the internal layers. If there is no change this should be mentioned in the text.

Answer: There is a considerable difference, especially between the case with a fixed viscosity versus the other two. The n=1 and n=3 cases result in very similar ratios of vertical to horizontal velocity (w/u) in the transition zone between little to full sliding. The fixed viscosity case results in much higher ratios, especially at depth, and consequently a much larger downward displacement of internal layers. This can also be witnessed in Fig. 4a where the surface of the fixed viscosity case has been displaced considerably more vertically than the other two, although no dip/ridge feature can be discerned. The temperature and pressure dependent viscosity concentrates velocity changes in the vertical to the lower layers of the ice sheet so a large part of the ice column has a horizontal velocity close to the surface one. A typical vertical velocity profile with fixed viscosity results in a much more gradual change in horizontal velocity with depth so that within the ice sheet, although the surface velocity for the 2 cases is similar, the velocity at depth for the fixed viscosity case will be lower along with the total mass flux. Regardless of viscosity, the vertical distribution of horizontal velocity over the lake is pretty much everywhere the same, uniform. So at depth for the fixed viscosity case, the ratio of vertical to horizontal velocity will be much greater for a particle of ice traversing the transition zone than in the n=3 case and isochrones therefore displaced further downward. Referring to your own work (Leysinger Vieli et al. 2007) and that of Parrenin and Hindmarsh (2007) we can consider the difference between flux shape functions for n=3 and a fixed viscosity and how the maximum step in isochrone elevation is proportional to the change in flux shape function in the transition from internal deformation to sliding. For the fixed viscosity case, the flux increases much more gradually with height and the maximum step in isochrone elevation will therefore be larger. The revised manuscript has been complemented with a new subfigure, showing a comparison between steady-state isochrone profiles for two cases previously presented (n=3 and $\eta = 10^{14}$) in addition to profiles from a new simulation with the same ice thickness but a smaller friction factor ($C = 10^{10}$), more appropriate for streaming conditions.

In the second experiment, a transient experiment, the authors look at how the internal layers evolve under a lake drainage happening over 10 years (compared to the ice velocity this is seen as an instantaneous drainage). However the lake surface is not a free surface in the vertical direction as it is fixed to the bed plane and only changes in the horizontal extend. What effect does this have on the internal layering?

Answer: If you were to isolate changes in the vertical, keeping the horizontal extent of the lake fixed we believe that the effect on isochrones should be much smaller than with changes in the horizontal. Such an experiment has been performed previously by Sergienko et al. (2007), although not looking specifically at isochrones. Their experiment resulted in little changes in horizontal velocity ($< 1\%$) and the ice surface reached its initial stage in about 20 years (there drainage occurred during 5 years). Its not unreasonable to believe that the vertical velocity (they used a 2D vertically integrated model) would also experience little changes as the horizontal one and therefore result in only minor disturbances to internal layers. As the lake boundary stays fixed in the horizontal, the points of strong vertical velocity (at the upstream and downstream ends of the lake) are also fixed and for there to be a strong change in isochrone pattern, there would have to be a strong temporally persistent change in the ratio between vertical and horizontal velocities (w/u) as a particle of ice traverses the transition zone, which is unlikely. This has now been addressed in more detail in section 3 (model experiments)

Furthermore it is not clear to me if the water that drains from the lake has been accounted for the ice downstream of the lake, which would affect the internal layering, e.g. by melting ice.

Answer: The water "draining" from the lake has not been accounted for no, and this would certainly affect the internal layering downstream of the lake by for instance, as you say, melting ice or potentially influencing ice velocities downstream. Channelized flow would induce some melting over a limited area but this would most likely be in the order of a few meters at most and therefore only have a small influence on the travelling wave, which in our case has an amplitude of around 100m. A more distributed drainage (Stearns et al. 2008) might potentially have a stronger effect with increased ice velocities downstream which would affect how fast layer disturbances would flatten and become undetectable. This has now been further clarified in section 3 (model experiments).

Nevertheless the drainage experiment is interesting, but to me it is not entirely clear if the comparison with the work by Wolovick et al. (2014), where a slip boundary is moving WITH the ice can be compared to the downstream boundary of the draining lake that is moving UPSTREAM. I feel that there the interpretation of the observed result is going too far and is too speculative.

Answer: Yes, that could have been worded a bit more carefully. The intention was not to directly compare our work to the work of Wolovick et al. (2014), merely to note that slip boundaries or slippery patches moving with the ice (such as our upstream lake boundary) are capable of distorting isochrone layers to a much greater extent that stationary ones and that for maximum effect the boundary should be moving at a velocity comparable to the averaged ice column velocity. In our case the upstream lake boundary is moving too fast to cause any significant upward deflection of isochrones during the drainage. The paragraph has been reworded to avoid confusion.

However, the discussion under which conditions the modelled signature in the internal layers - a travelling wave - might be observable with radar is good. The question however is how long is it visible? I find Figure 5 not yet that clear in showing the travelling wave. Maybe you could add a later time frame or show the last graph with the initial internal layers to better visualise the contrast.

Answer: We decided against showing the wave at a later stage because it becomes distorted and smoothed as it moves downstream. In order to properly simulate its advection downstream, a grid resolution comparable to the one used at the lake edges would have been needed downstream of the lake, which would have required much longer computation times, too long to be feasible. As it is now, the grid resolution is kept reasonably small for roughly two lake lengths in the downstream direction, but still far from the grid size used at the lake edges. The travelling wave is basically visible in the model until it reaches the downstream boundary but the distortion that arises is mostly due to the coarse resolution (compared to what would be needed). We can however display the internal layers at t=2000a as you suggest with the initial layer configuration and with an arrow pointing to it and this will be done in the revised version of the MS.

Technical corrections:

P3860, L2: odd wording: subsequent draw-down of isochrones and cold ice from the surface. An increase in velocity leads to a thinner ice body because its faster. The cold ice is still at the surface, but the temperature gradient does change. The maximum change in elevation of the isochrones is found at about a third of the ice thickness (see Leysinger Vieli et al., 2007) - this is where you would find the largest effect of temperature change - but its not surface ice. What you mean here is the Weertman effect. But this becomes only clear later in the text.

Answer: Somewhat confusing, yes. The ice originally at the surface stays at the surface of course as you rightfully point out but colder ice does get drawn down from above, but not directly from the surface, although the surface is also deflected downward at the upstream lake edge. "...from the surface" replaced with "within internal layers."

P3860, L31: what is rapid?

Answer: This has been reworded to "We also conclude that rapid changes in the horizontal extent of subglacial lakes and slippery patches, compared to the average ice column velocity, create a travelling wave...."

P3862, L14: Comma after hydrostatic equilibrium?

Answer: comma added

P3862, L19: Leysinger Vieli without hyphen, studied the effect of areas with basal sliding or melting on internal layer architecture, but not explicitly a subglacial lake.

Answer: Changed as suggested.

P3864, L3-5: could be written a bit clearer as you explain terms in an equation of an equation. Maybe its easier to explain it after each other?

Answer: reordered such that one is explained after the other

P3864, Equation 8: I believe here is something missing e.g. $H >$ ....

Answer: The conditions on which the conduction term depend on are explained in the paragraph following the equation. For clarity they have been added to the equation in the revised MS.

P3867, L21: Is the ice thickness realistic for Greenland or Antarctica? What effect does the ice thickness have on the result?

Answer: The ice thickness used in the model is relatively low for Antarctic lakes, that tend to be underneath around $\sim$ 3km thick ice (Siegert 2000), and high considering the only two identified subglacial lakes in Greenland (to date) that are both underneath around 700-800m of ice (Palmer et al. 2013). So it is somewhere in between. The study has now been complemented with a new parameter study where we look at how the deflection of internal layers and surface profiles are affected by a lower friction coefficient ($C = 10^{10}$), more appropriate for streaming conditions, and ice thickness. Increasing ice thickness leads to more strain heating at the base so the inclination of the bed plane was lowered to 0.1 deg for the new simulations. The dip and ridge features show a higher amplitude with thicker ice but this cannot be contributed solely to ice thickness as basal shear stresses increase as well. A new figure has been added to the MS showing surface profiles for the new parameter study and new paragraphs in the discussion have been added addressing the new results and related issues.

P3869, L8-12: Refer to Figure 2b?
Answer: reference to figure 2b added

P3870, L10: In order to know what viscosity is used in the other experiments one needs to look it up in Table 1, but it is never referred to in the text.
Answer: references added

P3870, L26-28: Can you visualise the traveling wave better, e.g. at a later stage when the isochrones around the lake fully recovered?
Answer: see previous answer

P3872, L9-11: Is this so? Why?
Answer: Well, maybe this was a somewhat stronger statement than the results actually allow for. The lake size experiment does result in a slight difference in downward displacement of internal layers, but it's only around 3% at most between the largest (L_S) size and the smallest (L_S) lake size. It could be an effect of the periodic boundary conditions. To avoid confusion and misconception, that sentence has been removed!

P3872, L15: Here you describe the Weertman effect.
Answer: True

P3873, L20: Both boundaries move but not in the same direction. It is rather a narrow- ing of the area.
Answer: True. The paragraph was reworded for clarity.

P3873, L23: affects instead of effects.
Answer: changed

P3873, L24-23: Im not sure you can compare this to Wolovicks et al. (2014) work, as your downstream boundary is moving in opposite direction to the ice.
Answer: The paragraph has been reworded to highlight the differences between our moving boundaries and the ones in the work of Wolovick et al (2004). Such boundaries moving in opposite directions to ice flow have a smaller integrated effect on isochrones as the relative velocity increases and the points of vertical flow have less time in "action" compared to stationary boundaries.

P3877, L24: Leysinger Vieli remove hyphen.
Answer: Fixed

P3880: Refer to Table 1 in the text.
Answer: fixed

P3883: Fig. 2b - not sure what colour-bar is for what. Are both colour-bars / colour- scales true everywhere or is one for the inlet figure only? Caption: Maybe along flow profile is a better expression than cross-sectional? I was always thinking at an across profile. Mention what the black lines are (its mentioned in the text but would be useful information in the caption too.) Mention in the caption the lake size you are showing.
Answer: Both color bars are true for Figure 2b including the inlet figure. The inlet figure is simply a zoom-in of the larger figure with the same colorscale. "Cross-sectional" replaced

with "along flow profile" and a reference to what the black lines mean and what lake size (L_M) we're showing was added to the caption.

P3884: Caption: again I find along flow profile over the lake easier to understand. Replace vertical bar with horizontal bar Not clear with what viscosity case this has been calculated (n=3?).

Answer: Replaced with "along flow profile" as suggested previously. "vertical bar" replaced with "horizontal bar". These were calculated with (n=3) as you correctly guess. That information was added to the caption.

P3885: Caption: You are showing the vertical profile of the scaled horizontal velocity but in a way you are saying it but as it reads I understood it for c) only.

Answer: changed to "Vertical profiles of (b) the scaled horizontal velocity and (c) the logarithm of ice viscosity, over the center of the lake."

P3886: Figure: The grey colour-bar is a bit odd - not clearly visible in the figure. Caption: what do you mean by yellow lines? They are not visible. With black lines you mean the grey scale colors? Mark the travelling wave - e.g. by an arrow, or show the initial isochrones together with the isochrones of the t=2000 case. Or could you show an even later stage?

Answer: The gray color-bar is not really essential. We kept it in for completeness but it could be removed. We are not really concerned with the actual age of ice in the model, just the geometry of the internal layers. This information has been added to the results section. The sentence mentioning the yellow lines was a reference to an older version of the figure and has been removed. Initial layers and an arrow has been added to the final time frame (t=2000a). With black lines we were referring to the gray-black color that denotes isochrones at different ages.

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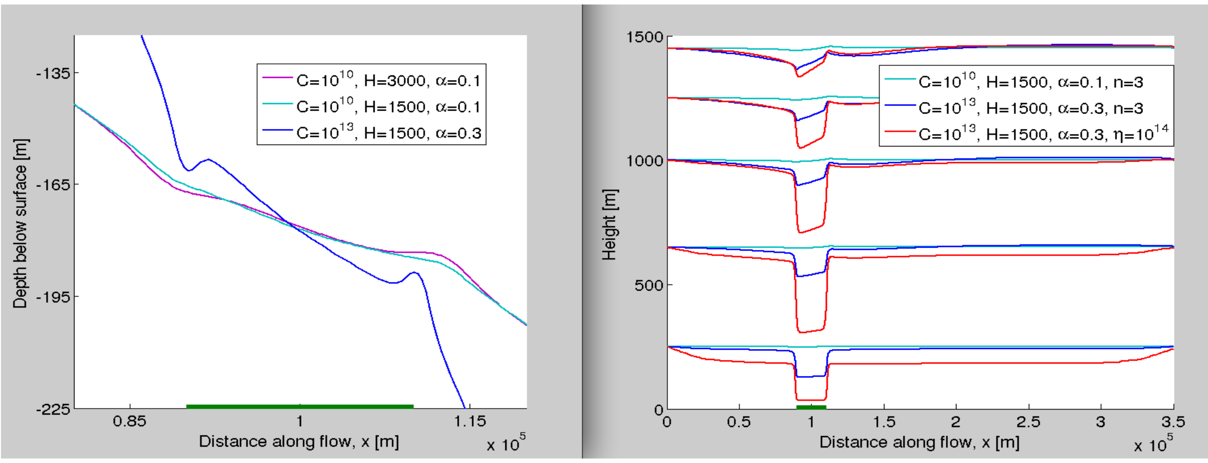

FIG. 1: A temporary version of the figure for the new parameter study ($C = 10^{10}$) showing surface profiles (on the left) and isochrone layers (to the right). The dark blue line in the left figure has been vertically displaced (because of the different angle) in order to plot it with the other two. In the figure on the right the isochrones are plotted with height, measured from the base. The downward deflection of the 2 lowermost red lines is an effect of the periodic boundary condition.