

General Response to the Editor and the Referees

Dear Eric Larour,

many thanks for handling the revision process of our manuscript and for sending us the referees comments. In this response I refer in detail to the comments the reviewers gave. All page and line numbers given, refer to the originally submitted manuscript. All section, equation and table numbers in our answers refer to the revised manuscript.

Thanks again, we are looking forward to your final decision.
Sebastian Goeller

Dear referees,

thank you for your review of the manuscript. We understand that good reviewing is a time consuming process, so we appreciate the effort you put into completing the reviews. We found your comments very helpful and modified our manuscript according to them.

Best regards,
Sebastian Goeller.

1 Response to B. de Fleurian (Referee #1)

1.1 General comments

This paper present a new approach for the modeling of steady states water distribution at the base of ice sheet. This approach uses the Shreve approximation (Shreve, 1972) to compute the hydraulic potential which is used to compute the water fluxes. This new approach take advantage of the existing development of the balance flux concept which is based on the same working hypothesis. The improvement of this methods lie in the fact that it allows to compute the water fluxes and so the conservation of the mass of water. The major achievement of this approach is the ability to model subglacial lakes which are clearly identified underneath Antarctic ice sheet and have a non negligible impact on ice dynamics.

The hydrological model is coupled to the thermo-mechanical ice model RIMBAY which allow to compute the water inputs needed by the hydrological model and also gives the ice geometry which will allow the computation of the hydraulic potential. The retroaction between hydrological and ice sheet model is handled through the basal boundary condition. This retroaction on sliding is defined as a free slip condition

above subglacial lakes and a Weertman type friction law which could be defined with a water flux dependent parameter.

As stated in the paper, the water layer concept allow the diagnostic modeling of subglacial lakes and then their effect on ice dynamics. This is a great improvement with respect to the preceding balance flux concept. I am more doubtful on the relevance of the relation between glacier sliding and water fluxes. With regard to the last development in subglacial hydrological modeling (Schoof,2010; Hewitt et al.,2012) it seems that the convenient value to compute the retroaction between hydrology and sliding would be the effective pressure (i.e. ice overburden pressure minus water pressure) which is fixed to zero in this approach. Commenting on this point would be an interesting development to add to the paper.

From our point of view, all notes the reviewer makes in this 'General comment' section, are described in more detail in the following section, where we will answer to each item in detail.

The writing of the paper is clear but I will advise to shift part 2 and 3 to put the description of the hydrological model first. Moreover, current part 2 is treating more of the coupling of the hydrological model to the ice model and I would rename it to reflect that.

We totally agree with your suggestion and shifted these two parts, and we renamed the section 'The ice model' into 'Ice model and coupling to hydrology'.

1.2 Specific comments

1.2.1 Abstract and introduction

Abstract and Introduction goes to the point of the study and are well referenced and clearly written. I have noted a few points that could be modified or improved :

1. Regarding the hydraulic potential, there is a misunderstanding in the given definition. In it's original form, the hydraulic potential ϕ does not include any effect of the ice pressure, it writes :

$$\phi = \rho_w g z + P_w$$

with ρ_w the water density, g the acceleration of the gravity and P_w the water pressure at the considered point of elevation z . This expression can be simplified while stating that the effective pressure at the base of the glacier (ice overburden pressure minus water pressure at the bedrock) is null. Under this approximation the hydraulic potential could be written as:

$$\phi = \rho_w g z + P_i$$

with P_i the ice pressure.

We reworded the sentence in the introduction to: *In general, the basal water flux follows the gradient of the hydraulic potential (Shreve, 1972) which includes both the water pressure and the bedrock elevation.* Additionally,

we added a much more comprehensive introduction of the hydraulic potential and the used approximations to the section '2.1 General formulations' following your above suggestions.

2. The reference to Lythe and Vaughan (2001) could be updated to Fretwell et al. (2012) for the second version of Bedmap.

Done.

1.2.2 Ice Model

Apart from the position and title change of this part I don't think it needs much modification. The possibility to fix β^2 to 0 in case of a lake should be introduced in the presentation of the sliding law (Equation 1) rather than in Section 4.2.

We added the sentence *We treat all grid cells, where the basal water layer thickness exceeds one meter, as subglacial lakes and fix β^2 to zero there.* to the presentation of the sliding law.

I don't really see the point of neglecting the frictional basal heating term which is known to be dominating the water input at the base of ice sheets (Cuffey and Paterson, 2010). I think that this point needs more explanation or that this term should be included.

We agree with the reviewer and reworded this paragraph to clarify why we ignore the contribution of the frictional basal heating in the basal melt calculation.

The basal melt rate M is given by (e.g., Pattyn, 2003)

$$M = \frac{1}{L\rho_{\text{ice}}} \left(k \frac{\partial T_b^*}{\partial z} + G + \vec{\tau}_b \vec{v}_b \right), \quad (22)$$

where $L = 335 \text{ kJ kg}^{-1}$ is the specific latent heat of fusion and $k = 2.1 \text{ W m}^{-1} \text{ K}^{-1}$ the thermal conductivity for ice, T_b^ is the basal ice temperature corrected for pressure melting and G is the geothermal heat flux. The last term in Eq. (22) is the contribution of basal frictional heating which can dominate the melting at the ice base in areas of faster ice flow (e.g., Joughin et al., 2004; Cuffey and Paterson, 2010) and can be ignored in areas where the ice is frozen to the bedrock. We neglect this term in our study as it leads to instability due to the positive feedback between basal melting, water flux and basal sliding velocities. Future studies which might include this term for a higher realism of the modeling have to parametrize this feedback to avoid model instabilities. The parametrization should reflect the influence of an existing basal water-film on the strength of the frictional heating and thus mitigate the generation of melt water for higher sliding velocities.*

1.2.3 The balance water layer concept

The only improvement that I see in this section is the adjunction of a general figure of the model where the different altitudes of Equation 6 would be depicted.

We decided to omit a figure of the hydraulic potential as it would not provide the reader with any further insights. It would show the bedrock plus $\approx 90\%$ of the ice thickness, resulting in a parabola (similar to the ice sheet surface) with very slight fluctuations (originating from the mountainous bedrock).

1.2.4 Experiment and results

My appreciation is that some small change in the phrasing in this section could lead to a better clarity and understanding. I find that there were too much figures (volume and fluxes) that are not easily compared. In my opinion, these figures could be replaced to comparison with the quantities of the reference simulation and written in the form of percentage that will give a much clearer image of the different simulations.

We thoroughly reworded the section 'Experiments and results' and renamed the experiments for a better clarity. Additionally, we added a table, showing the schematic coupling between ice- and hydrology-model for all experiments. In the 'Experiments and results' section the reader finds a general introduction of the different experimental setups, the used couplings and the quantitative description of the outcomes and the belonging figures. In the 'Discussion' section the results of the different experiments are compared and discussed. Here, the reader finds a table with the comparison of the ice and water volumes in percentage, a figure with differential plots of the ice velocities and a figure with the temporal evolution of water and ice volumes for all experiments.

I would also drop the reference to the cardinal points which is not really relevant in the case of a synthetic geometry to replace it by the geometric terms (front, divide and sides).

We changed the notation to "ice sheet front", "ice divide" and "lateral margins".

I am also quite not sure of the boundary condition that are used for the ice model from the given terms and don't see at any point the boundary condition that are applied to the hydrological model.

The boundary conditions are briefly explained in the introduction of the model domain in 'Experiments and results'. We added the applied boundary conditions for the hydrology model: *Closed free-slip boundaries are defined at the lateral ice sheet margins and the ice divide. At the ice sheet front an open boundary allows mass loss, which could be interpreted as calving into an adjacent ocean. In experiments, where a hydrological model is applied (Tab. 1), the ice sheet front is treated as an open and the lateral margins and the ice divide as closed hydrological boundaries.*

1.2.5 Discussion and conclusion

In this section, the introduction of a number of comparisons between the experiments using percentage values make it much clearer than in the preceding one.

Table 2 shows absolute and percentage values for ice and water volume. *Table 2. Ice volume, change of ice volume compared to the control run (CR) and stored subglacial water volume for all experiments.*

1.3 Technical comments

page 5227 line 4 : replace modeling by model

Wording changed to ... *incorporate basal hydrology into ice sheet models*

page 5227 line 8 : replace into the base... by at the base...

We mean ... *the geothermal heat flux into the base of the ice sheet* ... here.

page 5228 line 1 : I don't get the meaning of "data basis" perhaps the correct term is data base

Changed to *data base*.

page 5230 Equation 2 : Meaning of ρ_i is not given in the text

We added the equations of the shelfy-stream-approximation, where we explain ρ_i , before this equation.

page 5234 : I find the first sentence hard to understand it could be rephrased as "... the difference of the potential (Eq. 11). These transferred amounts are bounded by the maximum available volume of water so that $\epsilon\Delta_x P_{i,j}$ or $\epsilon\Delta_y P_{i,j}$ will never exceeds $W_{i,j}$."

Wording changed like suggested.

page 5236 Equation 19 : Typo error in the equation

We moved the corrected balance equation for the water layer W to the section '2.1 General formulations'.

page 5236 line 8 : I would replace model by grid

Changed like suggested.

Figure 7 : As a colorblind person I could not make anything out of this figure. I would suggest to change the color scale.

We changed the color scale from green/red to blue/red.

2 Response to Anonymous Referee #2

2.1 General Comments

The paper describes a new update / improvement for the thermomechanical ice sheet model RIMBAY and then several experiments highlighting the insights gained from this new development: 1. Sliding laws are coupled in part to the properties of the subglacial water system 2. Water is routed by a mass conserving algorithm that appears to be similar to the upwinding practiced by time-dependent version of the LeBrocq et al., (2009) water model for the Siple Coast. 3. When water is routed into an enclosed basin within the hydropotential, it is allowed to pool until they overflow at which point the lake basin overflows at the lowest point along the hydropotential rim. This is an important development as water budgeting work by Carter et al., (2011) showed that many active subglacial lakes are found at collection points in the hydropotential and that when a subglacial lake is filling it prevents the meltwater generated upstream from travelling any further. Additionally it is significant that the authors are considering the co-evolution the ice sheet geometry and subglacial

hydrology as did Johnson and Fastook (2002) did for the Pleistocene glaciations for North America. Furthermore, uncertainties about the basal environment continue to pose challenges for predicting future sea level rise in particular from Antarctica (Lemke et al., 2007). The largest concern is that the work in its current form does not place itself particularly well in the context of existing literature and consequently makes several claims that are not very reinforced. And while the model in its current form is a new way of illustrating the point the authors are trying to make, there is no consideration of how their approximations may affect reality. A prime example of this comes from the description of the topography in which the authors place the Gamburtsev Subglacial mountains in West Antarctica and then use a terrain generation algorithm that results in an inordinately high number of completely enclosed bedrock basins, completely inconsistent with published descriptions of the Gamburtsev Mountains topography (Bo et al., 2009; Bell et al., 2011).

From our point of view, all notes the reviewer makes in this 'General comment' section, are described in more detail in the following section, where we will answer to each item in detail.

2.2 Specific comments

Page 5230: Line 17: any thoughts as to why one might be able to calibrate the reference flux to various regions but not the whole ice sheet?

We changed the wording into a more general form: *A reasonable reference flux ϕ_0 can be obtained by adapting it to observed ice surface velocities.*

Line 20: at a minimum this should be changed to (e.g. Wingham et al., 2006), as work by Stearns et al., 2008, and Carter and Fricker, (2012) have also inferred flux rates by the same technique). Secondly the Wingham et al., (2006) number represented the water flux during a lake drainage event occurring over a time frame of 2 years, and then went on to estimate a refilling time of 30 years. Other efforts to quantify flux have stemmed from integrating estimates for basal melt over a whole catchment (e.g. Joughin et al., 2004; Carter et al., 2009a). There have also been efforts to infer water flux from seismic (Winberry et al., 2009). In general, there are quite a number of ways people have inferred this value in the past and range reasonable values for this parameter

We reworded that section and added appropriate citations. *In general, basal water fluxes for Antarctica elude direct observation. They can be indirectly estimated by the observation of ice surface elevation changes resulting from filling and discharge of subglacial lakes. Deduced volume fluxes vary from about 1 to $20\text{m}^3\text{s}^{-1}$ (Gray et al., 2005; Fricker and Scambos, 2009). In some cases up to $40\text{m}^3\text{s}^{-1}$ (Wingham et al., 2006; Fricker et al., 2007) and even peak values of about $300\text{m}^3\text{s}^{-1}$ (e.g., Carter and Fricker, 2012) are estimated.*

Page 5231: Line 12: I am a bit confused by the wording here. Again a quick read though of Joughin et al., (2004) would show that this term can be quite significant, especially in ice stream tributaries. Also you should reference the term for frictional basal heating. Again I recognize this model a simplification that works for the point you're trying to illustrate, but in order to get a more realistic representation of ice

sheet flow in the future, this is HUGE.

We agree with the reviewer and rewrote this paragraph to clarify why we ignore the contribution of the frictional basal heating in the basal melt calculation.

The basal melt rate M is given by (e.g., Pattyn, 2003)

$$M = \frac{1}{L\rho_{\text{ice}}} \left(k \frac{\partial T_b^*}{\partial z} + G + \vec{\tau}_b \vec{v}_b \right), \quad (22)$$

where $L = 335 \text{ kJ kg}^{-1}$ is the specific latent heat of fusion and $k = 2.1 \text{ W m}^{-1} \text{ K}^{-1}$ the thermal conductivity for ice, T_b^ is the basal ice temperature corrected for pressure melting and G is the geothermal heat flux. The last term in Eq. (22) is the contribution of basal frictional heating which can dominate the melting at the ice base in areas of faster ice flow (e.g., Joughin et al., 2004; Cuffey and Paterson, 2010) and can be ignored in areas where the ice is frozen to the bedrock. We neglect this term in our study as it leads to instability due to the positive feedback between basal melting, water flux and basal sliding velocities. Future studies which might include this term for a higher realism of the modeling have to parametrize this feedback to avoid model instabilities. The parametrization should reflect the influence of an existing basal water-film on the strength of the frictional heating and thus mitigate the generation of melt water for higher sliding velocities.*

Line 20: You probably want to cite Shreve (1972) here.

Done.

Page 5232: Line 4: This is an important issue you highlight. It would be useful to describe how you illustrate the water layer thickness term more specifically.

Reworded.

Line 7: the wording here is a bit awkward

Reworded.

Line 9: You're mentioning that the model is based on finite differences only NOW?

We added the statement *RIMBAY is a finite differences ice model* to the first paragraph of the (renamed) section '3. Ice model and coupling' in order to make that section self consistent. However, it is repeated here as motivation to formulate the balanced water layer concept in finite differences, too.

Line 20: Tautology. You may be able to eliminate this sentence.

We changed and shortened the wording to *The balanced water layer concept operates on an Arakawa C-grid (Arakawa, 1977). Hence the gradients of the hydraulic potential $P_{i,j}$ are defined at the margins of the grid cells as*

Page 5233: It may be useful to explain briefly in 2-3 sentences how this is different from up-winding which is commonly practiced in other models (e.g. Rutt et al., 2009)

We completely reformulated the section '2.1 General formulations', added the balance equation for the evolution of the water layer W and explain in detail the approximations and assumptions we made. These extensive descriptions clarify the classification of the method with respect to other hydraulic schemes.

Page 5236: Line 10: I am pretty sure the Gamburtsev's are in EAST Antarctica not WEST Antarctica (Bell et al., 2011).

Corrected, sorry for this embarrassing error.

Also it seems wise to enquire about the nature of terrain generation algorithm used? The presence of small-scale deep enclosed basins does not seem particularly realistic (Anderson and Anderson, 2010).

We reworded that section, to show that we are aware of the fact, that the used terrain generating algorithm overestimates the number of totally enclosed bedrock basin. *The bedrock consists of randomly distributed peaks with a linear increasing random amplitude up to 1 km. This artificial topography with mountains and troughs roughly mimics typical characteristics of observations, e.g. in the Gamburtsev Mountains region in East Antarctica (Bell et al., 2011) or the Ellsworth Mountains (Woodward et al., 2010) in West Antarctica. Although the used terrain generation algorithm overestimates the number of enclosed bedrock basins compared to observations (e.g., Anderson and Anderson, 2010), it is well suitable to demonstrate the balanced water layer concept.*

Line 20 - 25: This is an example of a perfectly acceptable and well-justified fudge factor

We hope our reformulation of this section clarifies this. *The ice surface temperature T_s is set to -10°C , the accumulation rate A_S is 0.5ma^{-1} , and the geothermal heat flux G is 0.15W m^{-2} all over the model domain. Compared to measurements in Antarctica, we chosed a relatively high surface temperature (Comiso, 2000) and accumulation rate (Arthern et al., 2006). Also the chosen geothermal heat flux is in the upper range of the estimated spectrum for Antarctica (Shapiro and Ritzwoller, 2004; Maule et al., 2005), which simply leads to higher melting rates and thus to a faster convergence of the basal hydraulic system in our model runs.*

Page 5237: Line 11: This is a really high melt rate for East Antarctic conditions (Carter et al., 2009; Pattyn et al., 2010; Bell et al., 2011).

Reformulated and citations added. *The melt rate is about 10mm a^{-1} for the majority of the model domain and decreases towards the ice sheet front (Fig. 2c). It has maximum values up to 14mm a^{-1} in deep bedrock troughs in the vicinity of the ice divide, where the ice thickness reaches its maximum and thus insulates the ice sheets base best from the surface temperature. The melt rates are higher than estimates for the Antarctic Ice Sheet (e.g., Carter et al., 2009; Pattyn, 2010), due to the chosen thermal boundary condition for a faster convergence of the hydraulic system in the next experiments.*

Line 25: If you compare this against the lake density reported in some of the various lake inventories (Wright and Siegert (2011) being the most recent) you will find your number somewhat high. For the purposes of the model this is fine, but if the intention is to bring the model into a more realistic doman some mention of this discrpenacy is in order.

We reworded this section to clarify this. *In total we find 266 subglacial lakes covering 2253km^2 with a water volume of 372km^3 . The percentage of the bed covered with subglacial lakes is 18.8 % for the model domain. Com-*

pared to estimates of the lake coverage for the whole Antarctic continent with $\approx 0.4\%$ ($\approx 50\,000\text{ km}^2$ of known lakes, Wright and Siegert, 2011) this number is high. The discrepancy can be explained by the topography we use. It is meant to loosely resemble particular Antarctic areas with a mountainous bedrock (and even for these it overestimates the number of enclosed basins) and is thus not representative for the whole Antarctic continent.

Page 5237: Line 1: You might want to call this the uncoupled run

*We renamed all experiment for a better clarity and called this run **CR – control run without hydrology**.*

Page 5238: Line 1: Tabacco et al. (2006) has some commentary on the terrains in which subglacial lakes are located.

We added a citation of Tabacco et al. (2006) in this paragraph.

Line 7: It's a little unclear whether you mean spatial variation or temporal variation.

*We inserted the word **spatial** to clarify this.*

Line 10: This is a rather high number for basal melt rate outside of those locations where shear heating is active (Joughin et al., 2004; Carter et al., 2009). For the purposes of what you are trying to illustrate it is perfectly acceptable, but one should be aware that your number are probably and order of magnitude too high.

Please see our above comment to Page 5237, Line 11.

Line 22: I believe there have been previous estimates for the percentage of the bed covered with subglacial lakes (Wright and Siegert, 2012). It is substantially lower than then number you present. I suspect this has to do with the topography used. Again, for what you're attempting to illustrate this is fine, but awareness of this discrepancy is necessary. In fact one of those

Please see our above comment to Page 5237, Line 25, where we added a comparison to the estimated subglacial lake coverage for Antarctica by Wright and Siegert (2011).

Page 5239: Line 6: Here is a time when you may want to mention that other authors do this as well, sine you are showing your work as an improvement upon the straw people. In fact BwB looks remarkably similar to the time varying water model LeBrocq et al., 2009, except it's coupled to an ice sheet.

In order to show that we are aware of the fact, that other users of the Budd and Warner balance flux scheme also use a hollow-filling algorithm for the hydraulic potential, we added a citation of Le Brocq et al. (2009).

Page 5241: Line 10: In fact Anandakrishan and Alley, (1997) report this as having happened, and it appears to have be validated by Carter and Fricker 2012)

Please see our comment below.

Line 13: You may want to look through Folwer 1999, before making this statement, or Evatt et al., 2006, applies more directly to ice sheets, before making too many claims about subglacial lake drainage. Scambos et al., (2011) actually describes lake drainage in response to changing ice geometry.

*We added the suggested citations and reworted the sentence to: **So climate***

changes, e.g. variations of the accumulation rate and surface temperature, affect the ice thickness and thus the hydraulic potential at the ice base, which can redirect subglacial water streams at large scales (e.g., Anandakrishnan and Alley, 1997; Le Brocq et al., 2006; Wright et al., 2008) or cause subglacial lake drainage (e.g., Evatt et al., 2006; Wingham et al., 2006; Scambos et al., 2011; Carter and Fricker, 2012).

Figure 6: This is great. It would be also nice to see how ice volume evolves over time with each of the different model runs.

We are not sure about this comment, from our point of view Figure 6a shows the ice volume evolving over time at the right hand axis and the water volume on the left hand axis for all model runs . To clarify the affiliation of the axes and the curves we colorized the left axis.

Figure 7: This needs a different color scale.

We changed the color scale from green/red to blue/red.

3 Response to Anonymous Referee #3

3.1 General comments

The submitted paper A balanced water layer concept for subglacial hydrology in large scale ice sheet models by Goeller et al. is an interesting application of a coupled model of ice dynamics and subglacial hydrology. The study uses a balance flux description of subglacial water flow modified to allow the formation of subglacial lakes and a shelfy-stream approximation to ice sheet flow. The model is applied to a synthetic domain meant to loosely resemble a mountainous region of Antarctica and is able to qualitatively produce dynamic features associated with ice streams and lakes. This paper helps fill a gap in existing literature regarding coupled subglacial hydrology and ice dynamic models. While neither the hydrology model nor the ice sheet model are particularly novel, the coupled application is promising. However I find a few major flaws: the description of hydrology and ice dynamics is somewhat weak with regards to current literature, and the implementation of the sliding law makes a number of questionable assumptions that are poorly described. I describe these issues in detail below, followed by a longer list of minor comments.

From our point of view, all notes the reviewer makes in this 'General comment' section, are described in more detail in the following section, where we will answer to each item in detail.

3.2 Major Concerns

3.2.1 Background on subglacial hydrologic models

line 11: The way this sentence is written it reads like the authors find four possible water flow regimes. It should be reworded to make it clear this is a review.

Reworded, please see the comment below.

lines 11-15: This paragraph should acknowledge the fundamental difference between 'fast' and 'slow' categories of flow regimes (e.g. Fountain and Walder, 1998; Hewitt, 2011). This study only addresses the 'slow' (or distributed or inefficient) category and that limitation should be stated. Furthermore, if this list is meant to be an exhaustive list of possible modes of subglacial water flow, additional mechanisms could be added, e.g. canals eroded into sediment (Walder and Fowler, 1994), flow within groundwater/till (Alley et al., 1986).

We reformulated and expanded this section following your suggestions. *For the transport of melt water there are two fundamental water flow regimes: channelized and distributed. Channelized systems are spatially concentrated and transport large volumes of water at high effective pressure (ice overburden pressure minus water pressure), whereby the effective pressure increases with increasing water flux. Examples of channelized systems include Roethlisberger channels incised into the ice base (Roethlisberger, 1972) and Nye channels cut into bedrock (Nye, 1973). Channelized systems act to reduce slip by drawing water from off-axis flow and increasing coupling there. Their net effect is to reduce ice slip and thus ice discharge. Distributed systems are laterally extensive and transport a small volume of water at low effective pressure. Examples include systems of linked cavities, which emerge by the ice flowing over bedrock bumps (Lliboutry, 1968), flow through a water film between ice and bedrock (Weertman, 1972), flow in canals eroded into sediment (Walder and Fowler, 1994) and flow within groundwater and till (Alley et al., 1986). Because effective pressure decreases with increasing water flux, these systems tend to enhance slip along the ice-bed interface.*

line 16: Similar to the last comment, it should be acknowledged that the idea that the hydropotential is a direct function of the ice thickness is only a good approximation for distributed flow it is not a good assumption for efficient, channelized flow.

We reworded the sentence in the introduction to: *In general, the basal water flux follows the gradient of the hydraulic potential (Shreve, 1972) which includes both the water pressure and the bedrock elevation.* Additionally, we added a much more comprehensive introduction of the hydraulic potential and the used approximations to the section '2.1 General formulations' following your above suggestions.

lines 24-26: These recent hydrologic studies should be acknowledged here, but describing them as providing 'water-pressure-dependent transitions from one water flow regime to another' is somewhat inaccurate. The major advance of these models is to close the mathematical description of the system and actually diagnose water pressure, which allows the use of water pressure-dependent sliding laws. Only some of the papers cited describe the transition between modes of drainage ((Schoof et al., 2012) does not), and that transition is dependent on water flux not water pressure. Finally they do not describe transitions between the specific flow regimes the authors have listed in the previous paragraph, but instead describe general distributed and channelized drainage without a specific physical mode of drainage implied.

We corrected that point by reformulating the sentence to *Nevertheless, promising efforts have been made recently to gain a collocated mathematical description*

for distributed and channelized water flow systems (Schoof, 2010; Schoof et al., 2012; Hewitt, 2011; Hewitt et al., 2012).

page 5228 lines 1-10: I don't disagree with this paragraph in general, but I think it should be toned down. High-resolution modeling of hydrology will of course be computationally expensive relative to coarse-resolution hydrology, but it may still be cheap relative to the cost of higher-order ice sheet models. Additionally, one could model the hydrology at higher-resolution than the bedrock topography data available.

We reworded this section following your points.

lines 11-25: The distinction to this approach relative to the previous paragraph is that these models only include distributed flow (no channelized flow) and it is unable to describe water pressures on its own (but an assumption is made that it is equal to ice overburden pressure). The fact that it assumes steady-state is important, but the more sophisticated models described in the previous paragraph can also assume steady-state if desired. The distinction that this method cannot provide water pressure is important to be clear about.

We added this very important points to make the distinction to the more sophisticated models and the restrictions of the balance flux concept clear. The approach makes the assumption that the water pressure is equal to the overburden ice pressure and thus only includes distributed flow. It presumes a basal hydraulic system in steady state and delivers the associated water flux for every grid cell, but it is unable to describe water pressures.

line 15: the word 'latter' is confusing here.

The sentence was reworded.

lines 17-19: I think it would be useful to readers to explain specifically why these methods lack mass conservation. (Mass is lost because internal sinks exist at local minima of the hydropotential surface.)

Reformulated. Another disadvantage of this attempt to describe basal hydrology is the lacking mass conservation on realistic topographies: only a fraction of the melt water produced inside the model domain reaches its margins, because up-stream flux contributions are lost at local minima of the hydropotential surface. Additional computational effort is necessary to conserve the flux over these hollows.

3.2.2 Sliding law implementation

a) I am concerned about the implementation and description of the sliding law used in the model. The description is confusing. First of all, an equation describing the SSA momentum balance should be included so it is clear where τ_{a} fits in. Secondly, and more importantly, why is τ_{a} defined as the driving stress in equation 2 if a SSA model is being used? I suspect what is meant is that Eq. 2 is used as an approximation for τ_{a} in Eq. 1 to reduce the nonlinearity of the momentum balance with this sliding law, but there is nothing in the text to clarify that τ_{a} in Eq. 1 is an approximation but it is solved for in the SSA momentum balance. If my interpretation is correct, then some notation should be used to distinguish the approximate τ_{a} (in Eq. 1 and 2 and

add a statement in the text stating this assumption is being made) with the calculated $\tau_{\text{a}}^{\text{b}}$ (in line 25 on page 5229 and an equation for the SSA momentum balance to be added). Finally, and perhaps most importantly, how good is the assumption of using the driving stress for the basal shear stress within the sliding law? It certainly seems possible to use the calculated $\tau_{\text{a}}^{\text{b}}$ in Eq. 1, using a fixed-point iteration in solving the momentum balance. Alternatively, there should be an assessment of how much the final calculated value of $\tau_{\text{a}}^{\text{b}}$ differs from Eq. 2 for the types of problems used in this study. Since sliding is proportional to the cube of $\tau_{\text{a}}^{\text{b}}$, this assumption could lead to locally large errors in sliding speed on the 2km grid used.

We thoroughly revised this section and added among other things the SSA momentum balance equation. Additionally, we clarified that the calculation of τ_{b} is a common approximation used in shallow ice sheet/shelf models.

b) I also have concerns about the implementation and description of the sliding rate C (Eq. 3). Is there a source for the form used (eq. 3)? I recognize that modeling sliding dependent on hydrology is not a mature area of ice dynamics, but this description is presented as if this form is standard or self-evident, which it is not. The use of the term 'first-order approximation' implies the magnitude of the formal error, but the form of this relationship, or even what the independent variables are, is far from settled. I suspect what is meant is 'physically plausible'. A few additional sentences are needed to explain where this equation comes from. I don't necessarily think it is a bad choice, but it needs an explanation. Why, for instance, is the sliding rate chosen to be dependent on water flux, and not water layer thickness as is done in some other balance-flux applications (e.g. Le Brocq et al., 2009)? Again it would be worth pointing out that water pressure/effective pressure is likely to be the more appropriate coupling variable (e.g. Clarke, 2005; Paterson, 1994, many others), but the hydrology model used requires using something else..

We reworded this section. *To parameterize the hydrology-dependent basal sliding a relevant coupling variable would be the basal water pressure (e.g., Clarke, 2005; Cuffey and Paterson, 2010; Schoof, 2010), which is not provided by our hydrology approach. Similar balance-flux applications (e.g., Le Brocq et al., 2009) assume a laminar water flow and then couple the sliding to the steady-state water-film depth. We want to avoid further assumptions about the type of the distributed water flow regime and introduce a simple physically plausible correlation of the sliding rate $C(\phi)$ (Eq. 18) and the subglacial water flux ϕ (Eq. 16)*

$$C(\phi) = C_0 \exp^{-m \frac{\phi}{\phi_0}} \quad (20)$$

with $C_0 = 10^7 \text{ Pa m}^{-1/3} \text{ s}^{1/3}$ (Pattyn et al., 2013) and the reference flux ϕ_0 , scaling this correlation. Consequently, an increased flux ϕ implies a smaller sliding rate $C(\phi)$ and thus an enhanced slipperiness, which decreases β^2 to a possible minimum of zero.

c) Finally, the exclusion of frictional basal heating term from Eq. 5 is troubling. Without this term present the coupling between the hydrology and ice dynamics is very weak changes to the melting rate can only happen as the temperature profile changes (very slow adjustment) and changes in ice thickness adjust the hydropotential. The argument that the SSA will overestimate the sliding velocity is not convincing if

the overestimation is large (i.e. an area where sliding is small), then the SSA is not an appropriate approximation of the flow anyway and some other flow model should be used (e.g. FO, L1L2). For the types of problems shown in the results (formation of ice streams) the frictional basal heating is expected to be important and perhaps dominate over the over terms in Eq. 5, so its absence severely limits the realism of the simulations. I recognize that including that term may lead to instability (blow-up or oscillations), and perhaps that is why it is not retained. However, it would be a useful contribution to explain what sort of behavior the model exhibits. I think the paper would be strongly strengthened to include that term and ideally either 1) use a momentum balance that is appropriate for the experiments run or 2) change the experiments to be appropriate for the momentum balance approximation used. Barring that, examples of model behavior when frictional basal heating is included would help justify why this type of simplification is necessary at present and point to areas of needed further study.

We agree with the reviewer and reworded this paragraph to clarify why we ignore the contribution of the frictional basal heating in the basal melt calculation.

The basal melt rate M is given by (e.g., Pattyn, 2003)

$$M = \frac{1}{L\rho_{\text{ice}}} \left(k \frac{\partial T_b^*}{\partial z} + G + \vec{\tau}_b \vec{v}_b \right), \quad (22)$$

where $L = 335 \text{ kJ kg}^{-1}$ is the specific latent heat of fusion and $k = 2.1 \text{ W m}^{-1} \text{ K}^{-1}$ the thermal conductivity for ice, T_b^ is the basal ice temperature corrected for pressure melting and G is the geothermal heat flux. The last term in Eq. (22) is the contribution of basal frictional heating which can dominate the melting at the ice base in areas of faster ice flow (e.g., Joughin et al., 2004; Cuffey and Paterson, 2010) and can be ignored in areas where the ice is frozen to the bedrock. We neglect this term in our study as it leads to instability due to the positive feedback between basal melting, water flux and basal sliding velocities. Future studies which might include this term for a higher realism of the modeling have to parametrize this feedback to avoid model instabilities. The parametrization should reflect the influence of an existing basal water-film on the strength of the frictional heating and thus mitigate the generation of melt water for higher sliding velocities.*

3.3 Minor Comments

3.3.1 Section 1

p5226 line 15: there is an extra comma

Removed

line 25: 'acceleration of ... ice velocity' is awkward. Consider removing the word 'velocity'

Reworded. Basal water lubricates the base of the ice sheet locally and hence leads to a reduced basal drag of the overlaying ice.

line 26: The use of the word 'streams' for both flowing ice and flowing water in the same sentence is potentially confusing. I recommend removing it from describing subglacial water features. Also, the presence of water is only one factor in the formation of ice streams thick ice and the presence of soft sediments are also important factors (e.g. Alley et al., 2004).

We changed the wording to *As a result, fast flowing ice streams can evolve above areas of enhanced subglacial water flow and ...*.

p5227

line 3: 'imperative necessity' is redundant.

We removed the word *imperative*.

line 9: unnecessary comma

Comma removed.

page 5228

line 24: 'what crucially affects the ice sheet dynamics' is awkward. Consider rephrasing.

We replaced *crucially* by *clearly*.

line 28: the word 'preceding' here is ambiguous. Perhaps 'additional' would be more appropriate?

We replaced *preceding* by *additional* like suggested.

page 5229

line 9: 'simplify' might be a better word choice here than 'clarify'

We replaced *clarify* by *simplify* like suggested.

3.3.2 Section 2

* It would make more sense to fully describe the model equations before the implementation of the boundary conditions.

We revised the whole section and added the balance momentum equation for SSA.

page 5229

line 16: the 'conventional' SIA is not that conventional any longer. There are very many recent examples of ice sheet modeling studies using SSA, higher-order, and full Stokes methods (e.g. Larour et al., 2012).

We removed the word *conventional* and slightly changed the sentence to *We choose the shelfy-stream-approach instead of the shallow-ice approximation (SIA) for grounded ice to incorporate shear stress coupling between adjacent grid cells (e.g., Greve and Blatter, 2009).*

lines 21-23: the description of the coupling between hydrology and ice dynamics is confusing it sounds here like the adjustment to the basal ice elevation is the only coupling, but that is not the case (as is described in the following pages).

We reworded this section to clarify the coupling between hydrology and ice model. *With the surface elevation S (Eq. 17) and $S = B + W + H$ the geometry of the ice model is directly coupled to the hydrology model by the basal water layer W (Eq. 4 and 5). The basal water layer, which is situated between*

bedrock and ice base, can gain a certain thickness and thus lift the overlaying ice by this amount. Additionally, the balance water flux ϕ (Eq. 16), provided by the hydrology model affects the basal sliding, which is elaborated in the following section. Additionally, we added a table (Tab.1), showing schematically the coupling for all experiments.

3.3.3 Section 3

p. 5231

line 21: 'were' should be 'are'

The whole section was thoroughly reworded.

Eq. 7: This is a time discretization, but it would be helpful to see the differential equation first (given this is a 'General formulation' section. Should there be a flux divergence term (e.g. eq. 19) included to allow water to move around as is described in the text (line 6, p 5232)?

We thoroughly revised and expanded the section '2.1 General formulations' and added among other things the time dependent balance equation for the water layer W .

p. 5232

line 9-10: the method used for RIMBAY should be in the previous section

We reordered the sections of the hydrology model and the ice model for a more consistent structure of the whole article. Therefore, this statement makes sense here, because the ice model has not been introduced so far.

p. 5233

Can you define the units for model variables? (either in the text or in a table)

We added ... *water transports $T_{i,j}^x$ and $T_{i,j}^y$ with $[T_{i,j}^{x/y}] = m \dots$* The units for the hydraulic potential P and the water layer W were given in the paragraph before.

p. 5234

line 21: no comma needed

Comma removed.

p. 5235

line 11: no comma needed

Comma removed.

line 19: 'According' has an extra 'c'

Corrected.

3.3.4 Section 4

p 5236

line 8. Why are 21 vertical layers used in an SSA model? Is this just for the vertical temperature diffusion calculation? Are they evenly spaced?

Indeed, the 21 vertical ice layers are only used for the calculation of the vertical temperature diffusion. They are not evenly spaced but become thinner towards the ice base. We added this fact and moved the sentence to the description of the temperature and melt rate calculation in the ice model section. *The ice temperature is calculated by solving the energy conservation equation and neglecting the horizontal diffusion for 21 terrain-following vertical layers, which become thinner towards the ice base.*

line 10: Gamburtsev Mountains are in East Antarctica.

Corrected, sorry for this embarrassing error.

line 16: 'looses' has an extra o.

Corrected.

line 17: 'cover' should be 'over'.

Reformulated. *All experiments are carried out with the same bedrock topography to guarantee comparability and are run for 20 000 years until both the ice dynamics and the hydrological system are in a steady state.*

line 18: If I understand the model formulation correctly, the hydrology is brought to steady-state with the geometry on every time step. It would be helpful to say that explicitly in section 3 and clarify that here.

The reviewer is correct, we hope the reformulation (please see above comment) clarifies this.

* The organization of this section is confusing. First of all, the two Budd and Warner methods should be described prior to the results section, with a clear explanation (table of all 5 model setups?) of how they differ from the balanced water layer method. It may also make sense to present the models in order of increasing complexity, e.g. Control run (no hydro model), One-way coupling with lakes, Balance-flux method with two-way coupling (BWA), Balance-flux method with filled depressions and two-way coupling (BWA), Balanced water layer method with two-way coupling.

We revised the entire section '4 Experiments and result' and renamed the experiments (including the applied coupling in the headings) for a better clarity:

CR – control run without hydrology ,

BW – balanced water layer concept (*lake-sliding coupling*)

BWF – balanced water layer concept (*lake- and flux-sliding coupling*)

BF and BF⁺ – balance flux concept (*flux-sliding coupling*)

Additionally, we added a table (Tab.1) showing schematically the coupling between the hydrology and the ice model for all experiments. From our point of view, we prefer the above order for the presentation of the experiments. After the introduction of the control run *CR* we present first the applications of the balanced water layer concept *BW* and *BWF*, which are the focus of this article, in the order of increasing complexity. After these, we present the (reformulated) experiments *BF* and *BF⁺* which we use as a benchmark for the latter experiments.

Finally, the terminology 'initial state' for the experiment in section 4.1 is somewhat confusing. I assume what is meant is the final (t=20,000 yr) state from this experiment

is used as the initial condition ($t=0$) for the other four experiments. However, when the word 'initial' is used subsequently there is ambiguity about which $t=0$ is being referred to.

The above reformulations should clarify this.

* Are appropriate significant figures used for values reported in this section?

We double checked the consistency between the values and the figures.

p. 5237

line 9: delete 'so far'

Deleted.

line 10: delete 'instead'

Deleted.

line 11: is 'insulates' meant instead of 'isolates'?

Replaced.

section 4.2: this experiment may be better described as 'One-way coupling' or 'One-way coupling with lakes'

Please see the above comment about the experiment names. Additionally, we point out that experiment *BW* is no one-way coupling, as the 'melting' of the ice model is coupled to the hydrology model, resulting in a distributed 'water layer' (hydrology model) which is coupled to the 'sliding' and the 'geometry' of the ice model. Please see Tab.1 for the schematic coupling between hydrology and ice model for all experiments.

p 5238

line 4: the word 'a' is not needed

Deleted.

line 13: 'branching' might be a better word choice than 'branchy'

Replaced.

line 18: a single 's' is the preferred spelling of 'focused'

Corrected.

line 20: remove the word 'still'

Deleted.

line 21: comma not needed

Comma removed.

p5239

line 5: no comma

Comma removed.

line 8: the phrase 'from the beginning' is awkward here.

We removed *from the beginning*.

line 10: It sounds like 'the significantly bigger part' of $0.0857 \text{ km}^3 \text{ a}^{-1}$ is being referred to here but I think what is meant is 'significantly bigger part ($0.0857 \text{ km}^3 \text{ a}^{-1}$) of the total flux'

We reformulated the whole paragraph.

line 23-24: Is this shown in a figure?

This is shown in Fig. 6a, which is referred to in the beginning of this paragraph.

line 27: no comma needed

Comma removed.

p. 5240

line 1: first comma is not needed

Comma removed.

line 8: first comma is not needed

Comma removed.

p5241

line 19: This statement seems self-evident and could be removed.

Reformulated. *The balanced water layer scheme can be coupled to any ice model, ...*

line 24: see comments about hydrology models above. Also the models reference need not be high-resolution, but they are more physically based.

We removed this sentence.

line 27: The flux-friction coupling is only partial since basal frictional heating is neglected (see comments above)

Please see our comment to Sec. 3.2.2c, where we explain the limitations and refer to future studies.

Figure 6. It would be clearer if each subfigure had its own legend.

We decided to present this two subfigures one on top of each other (and thus sharing one legend) to demonstrate the co-evolution of the ice and stored water volume and the water fluxes. The different colors and line-styles clarify the affiliations.

Figure 5. It might be interesting to identify the location of lakes on one of these panels.

The position of the transection is given in the caption of the figure.

Figure 7 (and p5240, line 6): Are these difference from the initial condition at time 0 or the control run?

The control run already is the steady state of the ice sheet (without hydrology) and is thus not changing over time. The renaming of the experiments and our revision of the section '4.1 CR – control run without hydrology' should clarify that. We removed the notation *initial state* to avoid any confusion. *All experiments start with the same steady-state ice sheet (Fig. 2a), which we call the control run (CR).*

4 Response to Anonymous Referee #4

4.1 Summary comments

This manuscript attempts to tackle the problem of an integrated subglacial drainage and ice flow model. Many ice sheet models account for water by balancing the heat fluxes at the basal interface making a loose approximation to sliding based on water availability. Such models do not adequately account for the water balance and simply use the presence of water as an ersatz for the ability for ice to slide over its bed. Other hydraulic phenomenon.

We are not sure what the reviewer means with the last words.

The paper as it stands is not free from errors and requires attention to several issues before it can be accepted for publication. Some of these are simple clarifications. Others require more in-depth revision.

From our point of view, all notes the reviewer makes in this 'General comment' section, are described in more detail in the following section, where we will answer to each item in detail.

4.2 Sections

4.2.1 Abstract

The abstract needs to be rewritten. There are fundamental questions about the subglacial hydraulic system such as the degree of connectivity and whether it influences ice flow. As such, the leading statement needs to be reworded and integrated later into the paragraph. If the subglacial hydraulic system does influence ice flow, under what conditions does the water system change the flux? For Greenland, the additional flux of ice from lubrication of surface-fed meltwater is roughly 10%. For Antarctica, this amount is not well characterized. The abstract needs to make that case in a succinct fashion.

We slightly reworded the abstract. However, the focus of the manuscript is the introduction of the newly developed *basal water layer concept* and not an application for surface-fed meltwater in Greenland or Antarctica.

4.2.2 Introduction

The introduction convolves separate ideas and is not easy to follow. The basis for including water is that observations link water to enhanced slip. Furthermore, separate observations of water beneath Antarctica show that there are relatively large volumes that move over timescales much shorter than ice flow. However, with the exception of a requirement for till to have enough water to have near-zero effective pressure, the role of water in ice drainage is not well understood.

We thoroughly reformulated the entire 'Introduction' section to clarify this.

5227, Line 11.

This paragraph is incorrect. Fundamentally, there are two water flow regimes: channelized and distributed. Channelized systems are spatially concentrated and transport

large volumes of water at high effective pressure (typically defined as ice overburden pressure minus water pressure). In short, water flux goes up as effective pressure goes up. Examples of channelized systems include Rthlisberger channels cut into ice or Nye channels cut into bedrock. Channelized systems act to reduce slip by drawing water from off-axis flow and increasing coupling there. Their net effect is to reduce ice slip and thus ice discharge. Distributed systems are laterally extensive and transport a small volume of water at low effective pressure. Examples include subglacial canals (Walder and Fowler, 1994) and linked cavity systems (e.g., Lliboutry, 1968, 1979). Because effective pressure decreases with increasing water flux, these systems tend to enhance slip along the ice-bed interface. It is not clear what role the paragraph beginning on line 16 serves. It starts with how water flows, but then discusses limitations on understanding the basal system. This space should focus on building a case for the modeling effort that comes later in the paper rather than saying why it may not be applicable. In the paragraph starting on line 24, the point of the models by Schoof, Hewitt, and Werder is that they have collocated distributed and channelized system. Thus, scaling the models from a mountain or outlet glacier is not a problem. What is problematic is knowing what an appropriate channel density becomes once the model is scaled. For example, in a 5 x 5 km Antarctic grid cell, is one channel, ten channels, or 100 channels appropriate? The coarseness of the data grid does not have a direct effect on these models because they are designed to provide both distributed and channelized flow. The text needs to be parsed to reflect the scaling issue rather than the issue of an appropriate DEM. The DEM used in constructing a hydrology model will ultimately have some effect on the output, that is true, but this is also true for ice sheet models. There has been no shortage of Antarctic ice sheet models despite the coarseness of the DEM.

We highly appreciate your detailed comments on this paragraph of the introduction and rewrote it following your suggestions.

5228, Line 11

The balance flux model needs to be discussed a little earlier in the paper. While there is no particular reason it does not incorporate specific drainage types, it is commonly treated as a generic flow type. It characterizes the momentum equation for water flow rather than the mass balance. The mass balance, and in particular the closure relationship, determines the flow type. Again, the authors insistence that it cannot accumulate water is based on what appears to be a limited understanding of how water flows subglacially. There is no particular reason that accumulation in lakes or other depressions cannot be incorporated as the balance flux is formally a momentum assumption rather than a mass balance

We revised and extended the introduction of the balance flux model in this paragraph:

Another well established method to trace the paths of subglacial melt water is the balance flux concept (Quinn et al., 1991; Budd and Warner, 1996; Tarboton, 1997; Le Brocq et al., 2006, 2009). The concept is easy to implement, fast and well applicable to continental scale modeling (e.g., Pattyn, 2010). The approach makes the assumption that the water pressure is equal to the overburden ice pressure and thus only includes distributed flow. It presumes a basal hydraulic system in steady state and delivers the associated water flux for every grid cell, but it is

unable to describe water pressures. Another disadvantage of this attempt to describe basal hydrology is the lacking mass conservation on realistic topographies: only a fraction of the melt water produced inside the model domain reaches its margins, because upstream flux contributions are lost at local minima of the hydropotential surface. Additional computational effort is necessary to conserve the flux over these hollows. Furthermore, the balance flux concept provides no possibility for melt water to accumulate in hollows and to form subglacial lakes.

4.2.3 The ice model

The justification for equation (3) is not clear. Presumably the scaling flux ϕ_0 represents a critical amount of water necessary to distribute water over the bed locally. Furthermore, is there a justification for the numerical value chosen? Is this from a previous study? Alternatively, it could be related to grid size or be empirically derived via the present modeling study. The justification for this needs to be clear in the text. Furthermore, the form of the equation states that the sliding rate increases with water flux. Because that is the case, this model is an approximation to the linked-cavity systems. Typically, sliding rate is coupled to effective pressure rather than water flux. The assumption here is that moving water remains at low effective pressure and channelization does not occur. Later in the paragraph, the authors cite Wingham et al. (2006) who examined the drainage their inferred rates using the channel theory of Rothlisberger (1972). As discussed above, channelization is not incorporated. Can the authors clarify that this number would be reasonable for the model here?

Section revised:

To parameterize the hydrology-dependent basal sliding a relevant coupling variable would be the basal water pressure (e.g., Clarke, 2005; Cuffey and Paterson, 2010; Schoof, 2010), which is not provided by our hydrology approach. Similar balance-flux applications (e.g., Le Brocq et al., 2009) assume a laminar water flow and then couple the sliding to the steady-state water-film depth. We want to avoid further assumptions about the type of the distributed water flow regime and introduce a simple physically plausible correlation of the sliding rate $C(\phi)$ (Eq. 18) and the subglacial water flux ϕ (Eq. 16)

$$C(\phi) = C_0 \exp^{-m \frac{\phi}{\phi_0}} \quad (20)$$

with $C_0 = 10^7 \text{ Pa m}^{-1/3} \text{ s}^{1/3}$ (Pattyn et al., 2013) and the reference flux ϕ_0 , scaling this correlation. Consequently, an increased flux ϕ implies a smaller sliding rate $C(\phi)$ and thus an enhanced slipperiness, which decreases β^2 to a possible minimum of zero. A reasonable reference flux ϕ_0 can be obtained by adapting it to observed ice surface velocities. In general, basal water fluxes for Antarctica elude direct observation. They can be indirectly estimated by the observation of ice surface elevation changes resulting from filling and discharge of subglacial lakes. Deduced volume fluxes vary from about 1 to $20 \text{ m}^3 \text{ s}^{-1}$ (Gray et al., 2005; Fricker and Scambos, 2009). In some cases up to $40 \text{ m}^3 \text{ s}^{-1}$ (Wingham et al., 2006; Fricker et al., 2007) and even peak values of about $300 \text{ m}^3 \text{ s}^{-1}$ (e.g., Carter and Fricker, 2012) are estimated.

4.2.4 The balanced water layer concept

In general, this section needs to be treated with the same rigor with which the ice flow section is treated. Foremost among these is that the water layer thickness is missing from the section. The equivalent equation to ice flow is,

$$\frac{\partial W}{\partial t} = -\nabla \cdot (W\vec{v}) + \frac{M}{\rho_{water}}$$

where water depth is W , water velocity is \vec{v} following the authors notation. This is equivalent to equation (19) in the text. Equation (7) goes on to discretize this equation in time, but it is not clear how in sections 3.2 and 3.3 this works in a continuum sense. The authors need to build the continuum formulation before working with the discretized form. Somewhere before section 3.3, the C- and A- grids need to be described in plain language relative to their two-dimensional stencil. The stencils referred to as Arakawa C- and A-discretizations are confusing. The differences are not clear.

We agree with the reviewer and completely reformulated the section '2.1 General formulations', added the balance equation for the evolution of the water layer W and explained in detail the approximations and assumptions we made.

The A- and C-grid stencils are shortly explained in the text (e.g., in section '2.2 Implementation for finite differences' for the balanced water layer concept and in section '3 Ice model and coupling to hydrology'). Additionally, the reader finds the appropriate citation (Arakawa and Lamb, 1977) in the text and the stencil of all relevant variables in Fig. 1.

4.2.5 Experiments and results

On page 5229, the authors state that they use RIMBAY with an SSA configuration. Then, the authors choose a topography inspired by the Gamburtsev Subglacial Mountains in East Antarctica. The Gamburtsevs are beneath the center of the ice sheet under Dome A. It seems implausible that an SSA approximation is appropriate for ice flow through them. Could the authors strengthen their argument or justification for this here? The Gamburtsevs have valley cross-sections that are ≈ 20 km wide with peaks that are up to 2400 m above sea level (Wolovick et al, 2013). Water networks seem to flow along the valley floors before being frozen-on at the peaks. The red noise topography that the authors use does not retain these characteristics, nor is it clear what the characteristics are that are that the authors are trying to simulate. Can the authors please strengthen the case for such topography? Additionally, several papers discuss large scale roughness in Antarctica and how it is organized (e.g., Bingham and Siegert, 2007, 2009). The result is that the model domain chosen here is too complex for the simple parameterization of the balance flux method. Lakes in Antarctica rarely show inclined roofs (Fig. 3), and the few that do are much larger than several ice thicknesses. Notably, Lake Vostok has an inclined roof, but the water circulation in the lake causes basal freeze-on. Furthermore, one of the many criteria used for determining subglacial lakes in radar data is that the subglacial roof be bright and flat (see the discussion and references in, Wolovick et al, 2013). The slope amplitudes in Figure

3 appear to be massive, and without at least some justification from the literature for the results (none appear on 5237, line 27 or the first few lines of page 5238) the reader is lead into a territory that is unjustified by data. Furthermore, the combination of unrealistic topography gives rise to a spurious population of lakes in Figure 4a. The authors also identify lakes as any water body greater than 1 m deep (p. 5237, line 22), but again, there is no relationship to topography at all. If the aspect ratio of the roughness along the bed is 1:1, then the basal boundary would not be stress free, for example. While the authors claim that high surface temperatures (-10C) and high geothermal heat flux (150mW m^2) aid in convergence, it is not clear that the model can perform with a polythermal base. If this is the case, the authors need to state that. The thermal conditions chosen do no mimic the interior of East Antarctica (Gamburtsevs: -50C surface temperature, *sim*50-60 mw m^2 geothermal heat flux) or at the low accumulation rates. The surface temperature the authors choose is close to the equilibrium line in Greenland (-4C (e.g., Ohmura et al., 1992)), and other processes, such as drainage and fracturing could possibly occur in an ice sheet with such parameters. Thus, while these are set up as tests for a type of hydrological model, it seems that the justification for parameters is weak. The combination of the enhancement factor in equation (3), the topography, and the thermal and accumulation parameters chosen yield a contrived scenario in Figure 5b where the velocities follow the subglacial water drainage pathways. These are reminiscent of the major outlet glaciers in Greenland (e.g., Jakobshavn) that have a very different flow regime as well as a topographic trough. The other locations that could be analogous are the tributaries to the Amery Ice Shelf. The authors should look for some similarity in the ice sheets, particularly Antarctica to make at least a rudimentary justification for the model. The authors need to adjust their significant figures to be representative of the model that they are choosing. The accuracy of the numbers reported in the results is incredibly accurate.

We added a paragraph to clarify, that we are aware of the fact, that our artificial topography not exactly mimics the Gamburtsev Mountains, what is not our primary intension. Instead, we state to create a test topography for demonstrating our hydrology approach which roughly resembles subglacial Antartic mountain ranges.

The bedrock consists of randomly distributed peaks with a linear increasing random amplitude up to 1 km. This artificial topography with mountains and troughs roughly mimics typical characteristics of observations, e.g. in the Gamburtsev Mountains region in East Antarctica (Bell et al., 2011) or the Ellsworth Mountains (Woodward et al., 2010) in West Antarctica. Although the used terrain generation algorithm overestimates the number of enclosed bedrock basins compared to observations (e.g., Anderson and Anderson, 2010), it is well suitable to demonstrate the balanced water layer concept.

Additionally, we added a paragraph, where we ascribe the high percentage of lake coverages in our experiment *BW* and *BWF* (compared to estimates for Antarctica) to our artificial topography.

In total we find 266 subglacial lakes covering 2253km^2 with a water volume of 372km^3 . The percentage of the bed covered with subglacial lakes is 18.8 % for the model domain. Compared to estimates of the lake coverage for the whole Antartic

continent with $\approx 0.4\%$ ($\approx 50\,000\text{ km}^2$ of known lakes, Wright and Siegert, 2011) this number is high. The discrepancy can be explained by the topography we use. It is meant to loosely resemble particular Antarctic areas with a mountainous bedrock (and even for these it overestimates the number of enclosed basins) and is thus not representative for the whole Antarctic continent.

We reformulated the paragraph where we introduce the thermal boundary conditions:

The ice surface temperature T_s is set to -10°C , the accumulation rate A_S is 0.5 m a^{-1} , and the geothermal heat flux G is 0.15 W m^{-2} all over the model domain. Compared to measurements in Antarctica, we chose a relatively high surface temperature (Comiso, 2000) and accumulation rate (Arthern et al., 2006). Also the chosen geothermal heat flux is in the upper range of the estimated spectrum for Antarctica (Shapiro and Ritzwoller, 2004; Maule et al., 2005), which simply leads to higher melting rates and thus to a faster convergence of the basal hydraulic system in our model runs.

We also expanded the description of the basal melting in our artificial experiments and explain why it is higher than other modelled estimates for Antarctica. *The melt rate is about 10 mm a^{-1} for the majority of the model domain and decreases towards the ice sheet front (Fig. 2c). It has maximum values up to 14 mm a^{-1} in deep bedrock troughs in the vicinity of the ice divide, where the ice thickness reaches its maximum and thus insulates the ice sheets base best from the surface temperature. The melt rates are higher than estimates for the Antarctic Ice Sheet (e.g., Carter et al., 2009; Pattyn, 2010), due to the chosen thermal boundary condition for a faster convergence of the hydraulic system in the next experiments.*

Again, the focus of our study is the demonstration of a new hydrology approach which among other things allows the dynamic generation of subglacial lakes. Therefor we create the described artificial topography, which is well suitable for that issue.

4.3 Editorial fixes

5226, Line 19:

Many subglacial lakes instead of Hundreds. The numerical value here is not critical and seems to change with every new investigation in Antarctica.

We think *many* is too unspecific while *hundreds* has the right order of magnitude.

5226, Line 20:

Change to decade. The papers cited are all published within the last decade.

Changed to *decade* like suggested.

5226, Line 23:

are not isolated but **can** belong to **distinct** hydrologic networks.

Reworded like suggested.

5226, Line 25:

It is not clear that ice streams form over areas of active subglacial drainage or whether the heat from sliding sustains the water network. The authors should reword this so that either possibility is permissible.

We agree with the reviewer about the unclear interaction between sliding and melting, but want to refer to the last paragraph in section '3 Ice model and coupling to hydrology' on this point, where we discuss that issue in more detail.

5226, Line 25:

This is not an acceleration per se. It is either increased ice velocity or an enhanced strain rate. Technically, it is velocity per change in distance, so referring to strain rate is better here.

Reworded. *Basal water lubricates the base of the ice sheet locally and hence leads to a reduced basal drag of the overlaying ice.*

5230, Equation (4):

This should be a divergence operator rather than a gradient operator. Also, indicate that A_s and M are local volumetric rates rather than mass rates. Confusion may arise with subglacial meltwater generation where the melt rates are typically mass melt rates (Clarke 2003, Spring and Hutter, 1981)

We replaced the nabla operator by the divergence operator to avoid any ambiguity. The unit of the melt rate M with $[M] = \text{m/s}$ follows directly from Eq. 5 and 21, the unit of the accumulation rate A_s with $[M] = \text{m/s}$ can be directly derived from Eq. 21 too. Additionally, the units for both variables are explicitly given in section '4 Experiments and results'. Hence no confusion should arise.

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