Response to reviewer 1

> *We thank the reviewer for their comments and helpful suggestions for improving this manuscript. Our responses are italicized and indented below.*

The paper uses a model of subglacial hydrology to investigate processes of lake filling and drainage beneath a synthetic ice stream that resembles the Recovery Glacier System. The conclusion is that the system is characterized by (i) water accumulation at the bed that results in intermittent channel formation and meltwater drainage that leads to lake filling; (ii) subsequent steepening of downstream hydraulic gradients that drive increasing efflux from the lakes, (iii) downstream channel formation that allows lake drainage, and (iv) eventual shutdown of the channels as water supply decreases due to drainage of the stored water volume. It is also argued that the system is characterized by slow-moving pressure waves.

I found the story quite interesting, but since it is based largely on model results with limited observational constraints, I was left wondering how well it compares with reality. Certainly it provides a mechanism to explain the lake filling/drainage events that are suggested by altimetric studies (which show rapid uplift/subsidence cycles in restricted locations). However, I felt that the presentation lacked the depth of insight and understanding displayed by some other work on Antarctic subglacial meltwater drainage (such as that by Tulaczyk, Christoffersen and Bougamont). I think this is because the authors never really identify a set of well-posed research questions that they address with the model, so the whole paper has the feel of a report on the results coming out of a black box, and it isn't at all clear how the study advances knowledge and/or understanding of the system. The paper left me feeling intrigued by the problem, but unconvinced that it will influence thinking about these systems very significantly. It might provoke work that will, but I think the authors could make a lot more of their results than they do if they were really clear about what are the issues they want to resolve through conducting and publishing the work.

> *We appreciate that our previous introduction in the manuscript did not address the aims and objectives of our study as well as we had intended and we thank the reviewer for highlighting this lack of focus. We have made an effort to address this by including our aims and rephrasing the introduction so that our reasons for use of a synthetic model are clear. Below we elaborate on this in response to the specific reviewer comments above.*

In response to: "I found the story quite interesting, but since it is based largely on model results with limited observational constraints, I was left wondering how well it compares with reality."

> *The reviewer is correct in that our limited ability to directly access subglacial hydrological systems of Antarctic subglacial lakes and ice streams means there are few data to compare with model outputs. We suggest that it is for this reason that modeling is currently one of the most appropriate tools for exploring subglacial hydrological development. As we are using a synthetic model, we do not suggest that this can be directly compared with surface altimetry records, for example, and instead use the results of the model to encourage testing of hypotheses. In response to comments from the reviewer, we have emphasized this at the beginning of the discussion by saying: "by*

applying a 2D hydrology model, which produces lake filling and drainage through internal dynamics, we can make a step towards understanding and projecting the development of Antarctic subglacial drainage systems in addition to generating testable hypotheses.".

Because the process of hydrology modeling is complex and involves both distributed and channelized systems it is a valuable exercise to assess the development of the system without complicating it with realistic topography. This means that we can examine the controls on lake growth and drainage and also the sensitivity of the system to various parameterizations. The model will in future be applied to real topographic systems but prior to this it is useful to know what hydrological phenomena are common features in relation to lake dynamics and ice streams rather than unique to certain topographical situations. As a result, we maintain that the application of a synthetic model to this scenario is a useful and important step in understanding the causes of subglacial lake formation and drainage.

In response to: "I felt that the presentation lacked the depth of insight and understanding displayed by some other work on Antarctic subglacial meltwater drainage"

The works of Tulaczyk, Christoffersen and Bougamont are interesting advances in our understanding of controls on ice stream stability and dynamics. These papers use a 3D ice dynamics model and couple with a simplified till-based hydrology, with water either produced and refrozen in situ (and therefore not actively flowing through the system) or with Darcian flow alone. While this is very useful for establishing controls on ice dynamics from till characteristics, our approach and research questions are very different. We are interested in catchment-scale hydrological development and also the controls of lake drainage. Our model is the first to analyze 2D development of subglacial channels and the hydrological controls on lake drainage. The works of Tulaczyk, Christoffersen and Bougamont do not address either of these issues and it is therefore difficult to compare their research to ours. In the future we hope to develop the model to couple with ice dynamics and include till properties but for the moment we believe our findings of hydrological development are an interesting and highly relevant output that would enhance rather than contradict the findings of Tulaczyk, Christoffersen and Bougamont. In terms of insight, there is so little known about the subglacial hydrological systems of the Antarctic: what the substrate consists of, catchment-scale controls, whether channels can form and/or persist, controls on lake drainage, pressure etc. any advance we can make with modeling that will encourage further data collection and future modeling is important.

In response to: "I think this is because the authors never really identify a set of well-posed research questions that they address with the model, so the whole paper has the feel of a report on the results coming out of a black box…"

We have changed our introduction so that the final paragraph is now clear about our aims and research questions. We hope that this will allow readers to understand our motivation behind this research and why outputs from a synthetic model enhance *knowledge of subglacial drainage development in Antarctic ice streams. The final paragraph of the introduction now reads:*

"Our primary aims are to examine a) the hydrological conditions that allow subglacial lake growth and drainage on a catchment scale, and b) the impact of the lake drainage on downstream water pressures and, by proxy, ice dynamics. To achieve this, we apply GlaDS, a finite-element basal hydrology model, to a synthetic system designed to represent an idealized Antarctic ice stream with one overdeepening. Using this simplified system allows us to identify hydrological controls on lake dynamics and examine the wider catchment without complications of highly variable basal topography. Our approach is novel as it does not require any external forcing to fill and drain the lakes (c.f. Carter et al, 2012); this instead occurs due to internal model dynamics. We begin, in section 2, by giving a brief summary of the model and, in section 3, our application of the model to our idealized ice stream. This is followed, in section 4, by an exploration of the model outputs for an ice stream without and with an overdeepening, and the differences between the two setups. Section 5 gives an outline of results from sensitivity tests of the model and section 6 covers the limitations of the modeling approach. We discuss the relevance and application of the model outputs in section 7 before concluding in section 8.".

Detailed comments (referenced to page and line number)
3.4-3.8: over a period….has been found…. over a period

Changed

3.9: Is this interconnection permanent or intermittent? If permanent – I assume the flux itself is time-varying. I think discussing this issue more fully here will help provide more compelling motivation for the paper.

It is not yet clear whether the interconnection between lakes in Adventure subglacial trench is permanent or intermittent and we hope that our outputs from this synthetic modeling exercise will help address this question. We have added in "The mechanisms and longevity of hydrological connection are, however, not well understood." following this sentence to illustrate that it is an area of research worth pursuing.

3.15: "impact of subglacial lakes on hydrological development" – it could very well be that looking at this connection in reverse could be useful (i.e .the issue could be how hydrological evolution drives lake behaviour – rather than opposite). I think failure to look at the system in this way may be the biggest weakness of the paper (I recognize that feedbacks may be such that it is hard to figure out what the actual drivers for change and evolution are – but the paper just seems to sidestep the issue)

Much of the discussion of this paper is dedicated to assessing the drivers of lake growth and drainage. We discuss in detail the causes of lake growth due to the changing hydraulic potential gradients as a result of pressure wave movement. We also discuss the growth of channels on the downstream side of the lake due to large-scale downstream *hydrological development as a causal factor for lake drainage. In addition, we cover the impact that lake drainage has on downstream hydrological development. As a result, the focus of this paper is primarily on the causes of lake growth and drainage rather than the impact of lake drainage on hydrological development. To clarify that hydrological models in general can be used for this, we have changed the sentence from:*

"As an alternative, numerical models can be used to infer conditions at the ice-bed interface and to estimate the impact of subglacial lakes on hydrological development."

To

"As an alternative, numerical models can be used to investigate the causes of lake growth and drainage, and to estimate the impact of subglacial lakes on hydrological development."

We also change the final paragraph of the introduction to highlight our aims:

"Our primary aims are to examine a) the hydrological conditions that allow subglacial lake growth and drainage on a catchment scale, and b) the impact of the lake drainage on downstream water pressures and, by proxy, ice dynamics."

3.25: "assessing lake volume from altimetry is challenging" – I agree – it can suggest where lakes are, and surface height changes may suggest volume changes are underway, but quantifying this is really an inversion problem that has yet to be tackled. I agree with the final sentence of the paragraph but am not convinced that this paper really changes the situation.

The reviewer is correct that quantifying lake volumes from surface uplift and subsidence is an inversion problem. As we are presenting a synthetic model of catchment-scale hydrology and lake drainage dynamics we do not address this inversion problem in this manuscript. However, we believe that in order to quantify changes in lake volume in relation to surface records, a hydrological model, such as we have presented here, is necessary in addition to the inverse problem the reviewer suggests. We are open in the manuscript that there is still work to be done, for example coupling ice flexure and dynamics with hydrological models, in order to fully comprehend the system. We hope that the new introduction will clarify our aims for the synthetic model.

4.2: do you mean "drainage" in the generic sense here (i.e. water flow over the bed) or do you just mean "lake drainage" – important to be clear about this.

Changed to "lake drainage".

5.24: has been shown to have…(or maybe just "has up to 13…")

Changed to "has up to 13".

6.23: over a period of..

Changed.

7.18-19: I assume you mean the modeled water pressure, not “the model” as stated

Changed.

7.21-7.23: If you have low water pressures because the cavities do not fill with water, how are you sustaining the basal velocities at rates that keep the cavities open? Seems to be a feedback missing from the model somewhere.

In future versions of the model we hope to have a system where ice dynamics and hydrology are coupled so that these feedbacks are included. However, this is currently a hydrology model without a dynamic component and does not include temporal variation in basal sliding, which we discuss in the model limitations section. As a result, the basal sliding rate is fixed and not linked to water pressure. To clarify in the section referenced here, we change the phrasing from “there is not enough water to pressurize the distributed cavities for an ice speed of 100 m/a” to “there is not enough water to pressurize the distributed cavities for our fixed ice speed of 100m/a”.

8.24: in terms of either the magnitude of the water pressure or its persistence

Changed.

11.9: I doubt the over-deepening itself forms and drains on these timescales. I assume you refer to the lake within it?

Changed to: “The larger overdeepening also allows a lake to form and drain slightly more quickly compared to lake in the standard overdeepening of 150 m.”

11.11: We also vary…

Changed.

11.11: “When the rate is decreased.” – the rate of what? Basal melt I assume?

Changed to “When the basal melt rate is decreased”.

11.12 The depth of the lake is also smaller.....

Changed.

11.16: water levels fluctuate over a similar range

Changed.

11.21: and it therefore takes more time to reach near-overburden….

Changed.

11.24 conductivities within which the lake…

Changed.

11 – Section 5 – the range of parameter values used in the sensitivity analysis seems quite limited. It would be useful to explain this – is it based on physical reasoning , or just a means of limiting the number of model runs required? Either way, how did you settle on this specific range?

The parameter ranges were chosen both because of physical reasoning and to limit the time for model runs. To clarify, we have now elaborated on our choice of sensitivity parameter range in the text. In the case of the distributed conductivity, lower values caused the model to run too slowly to allow analysis of the system. For the water production rate, 2mm/year is double the value suggested for Recovery catchment by Fricker et al (2014) and we do not test greater values. We also now note that if the overdeepening depth is increased to 500 m, the model cannot run efficiently with the current mesh setup (i.e. running the model takes weeks rather than days) and so for deeper lakes a different model configuration would be necessary.

12.5: "a lake does not grow" – this implies a lake that maintains a stable volume – is this what you mean, or are you actually discussing whether or not a lake will form at all?

Changed to "lake does not form" for clarity

12.8: Little is known about the spatial and temporal evolution of the subglacial meltwater drainage systems of Antarctica and their…

Changed.

12.11: the system may be substantially…

Changed.

12.12: What do you actually mean by "and to some extent. Greenlandic outlet glaciers" – that they are only to some extent more closely studied, or that the difference between Antarctic subglacial systems and Greenlandic systems may be less than the difference between Antarctic systems and mountain glacier systems? It seems pretty obvious that systems fed by supraglacial inputs that vary seasonally and diurnally and also have extreme input events will be significantly different from systems fed primarily by basal melt and subglacial storage release events.

We have removed "to some extent" in order to fully differentiate the mountain and Greenlandic outlet glaciers from the Antarctic systems we are discussing. Indeed, the systems that are fed only by basal melt are likely different from those fed from seasonally-varying ice surface melting but, given that relatively little has been discussed about spatially and temporally developing hydrological systems in the Antarctic compared to other regions of the world, we believe it is a valid point to make.

12.13: features is that there is no water input… surface, so variability in water fluxes (and pressures?) does not occur on diurnal, weather-related, or seasonal timescales, but over years or even decades (BUT I think you need to discuss the sources of variability over these timescales – is it just drainage system instabilities?)

We have changed the phrasing as suggested. We agree that over the scale of years and decades that changes can happen in Antarctic systems other than internal hydrological development, including changes in ice dynamics due to ocean processes and mass change etc. The model is currently not configured to include ice flow and so we cannot address dynamic changes or mass changes that might impact the hydrological system. This, however, would be an interesting area for future research.

12.16: the phrase "basal hydrology develops" is tricky because you seem to be confounding the issues of time varying water fluxes with those of drainage network structure and channel morphology. I really think you need to be much more careful about this and think about each of these separate but connected issues clearly and distinctly – even if you do this solely in terms of what your model is simulating (which may or may not bear some resemblance to reality. I think the paper is struggling here simply because your thinking about the issues is not yet clear.

With this sentence we aimed to point out that the lack of knowledge about subglacial water production and the lack of available data about subglacial hydrology means that it is difficult to determine characteristics of the subglacial hydrological system in the Antarctic, which varies over years and decades and has no water input from the surface. We therefore clarify by changing the sentence from: "This characteristic causes two major difficulties when attempting to establish how Antarctic basal hydrology develops: (1) the subglacial production of water is based on modeled geothermal heat fluxes and modeled ice fluxes rather than measured water inputs rates from the surface and (2) available data records, particularly from satellite sources, are limited to the last couple of decades."

To: "These features cause two major difficulties when attempting to establish the characteristics of Antarctic subglacial hydrology: 1) estimates of subglacial water volumes are extrapolated from modeled geothermal heat fluxes and modeled basal friction from ice flux, rather than measured water inputs rates from the surface and 2) available data records, particularly from satellite sources, are limited to the last couple of decades."

In terms of our later analysis of basal hydrology development, we suggest that this 'development' encompasses changes in water flux, water pressure and network *development for both the sheet and channel. It precisely because of this complexity that the model outputs we present are new and exciting results in relation to lake dynamics and ice stream hydrology. Because these features of the hydrological system are all interconnected it puts us in a unique position to discuss channel growth in relation to water pressure, or increased flux out of the overdeepening causing channel growth that eventually allows the lake to drain. It is not possible to separate the examination of water fluxes from network development and we believe attempting to do so would hinder interpretation and discussion of the hydrological system development.*

12.17: surprising there is no mention of basal friction here (including friction between entrained debris and bedrock), or of the heat generated by the water flow itself – these terms are minor in temperate systems so are typically ignored – I'm not sure we can make the same assumption here.

We have clarified by changing the phrasing from "modeled ice fluxes" to "modeled basal friction from ice flux". Our water input rate to the system is based on estimates made in other studies about Recovery Ice Stream (Fricker et al., 2014) and attempting to quantify this estimate further is beyond the scope of this study. In addition, the additional flux from melting due the dissipation in water flow is always small and will likely not impact out outputs.

12.21: predicting the development of Antarctic subglacial drainage systems

Changed.

12.19-12.22: Need to recognise that the best you can do is generate testable hypotheses – until it is possible to do in situ measurements in these systems, the models will remain untested and their results no more than hypotheses

We acknowledge that a primary usage of models such as the one we present here is to generate testable hypotheses that will drive further research. To clarify this at the beginning of the discussion we have changed our last sentence to read: "…by applying a 2D hydrology model, which produces lake filling and drainage through internal dynamics, we can make a step towards understanding and projecting the development of Antarctic subglacial drainage systems in addition to generating testable hypotheses.". In addition, we have moved the model limitations section prior to the discussion so the readers will be aware of limitations of numerical modeling approaches while reading our analysis of the model outputs.

12.24: are forced by seasonally varying and weather-related water inputs

Changed to "forced to seasonally varying water inputs".

12.25: Do you know for sure there are no seasonal forcings on these systems - from snow loading variations for instance (self-organised criticality…) , or non melt-related sources (e.g. Kulessa, B., Hubbard, B. & Brown, G. (2003). Earth tide forcing of glacier drainage. *Geophysical Research Letters* 30(1)). These may be minor in alpine/Greenland systems but could become very significant where the surface melt signals are missing.

It is not possible to know absolutely that seasonal forcings do not impact the Antarctic hydrological systems and so we have rephrased to say "variability in subglacial water pressure and fluxes likely does not occur on diurnal, weather-related, or seasonal timescales". We think that snow loading likely does not impact the Antarctic hydrological systems that we are examining under very thick (2km) ice and our model is not currently configured to test this. In terms of tide forcing, we note that the boundary conditions include a static outlet pressure and that tidal influences are not included. While it would be interesting to see the influence of tides on the hydrology of the system it is beyond the scope of this study.

13.2: water volume per unit area

Changed.

13.4: the development of these hydrological systems will also be different

Changed.

13.9: similar phenomenon

Changed.

13.11: suggest that funneling

Changed.

13.15: channels beneath the ice stream….therefore do not induce temporal…

Changed.

13.18: channels is a key enabler of spatially propagating pressure waves

Changed.

13.19: phenomenon that has ..

Changed.

13.21: allow the waves to develop

Changed.

13.22: realistic to assume a unidirectional relationship between hydraulic gradients and water fluxes. This whole sentence is pretty arm wavy and doesn't give the impression that the issues have been thought through clearly. I have a similar problem with the next sentence as well.

> *We think this is a misunderstanding due to our phrasing. We are not suggesting that the low hydraulic gradients are as a result of shallow surface slopes and low water fluxes etc. Instead we are saying that the reasons for the pressure waves are a result of a) hydraulic potential gradients, b) low water fluxes and c) funneling of water from a large catchment.*
>
> *To clarify we have changed the sentence from: "These are low hydraulic potential gradients due to shallow surface slopes, relatively low water fluxes, and funneling of water from a large catchment"*
>
> *To:*
>
> *"These factors are: relatively low water fluxes; low hydraulic potential gradients due to shallow surface slopes; and funneling of water from a large catchment so that the water input rate is higher than the capacity of the ice stream."*
>
> *We have also altered the following sentence for clarity (see below response).*

13.24: it is water pressure gradients that drive water flow, not the pressure per se

> *We have clarified by changing the sentence from: "Our hydrological explanation for the waves is that water pressure builds up in the constricted system, followed by faster water flow resulting in temporary channel growth, which moves the excess water downstream."*
>
> *To:*
>
> *"Our hydrological explanation for the waves is that water pressure builds up in the upper region of the ice stream, increasing the hydraulic gradient. This leads to faster water flow resulting in temporary channel growth, moving the excess water downstream."*

13.27: distributed system water thickness –but there seems to be an unstated assumption that the change in film thickness is directly/linearly related to the change in water pressure. Is this true?

> *The film thickness is directly related to the change in the water pressure in the cavity system. However, the pressure change does not allow more than 8cm increase in the distributed system thickness. We clarify by changing the phrasing to:*
>
> *"Despite the pressure change, the water layer thickness of the cavity system only increases by a maximum of 8 cm".*

14.2: might be identifiable at the surface.

Changed to "might be identifiable on the ice surface using feature tracking methods".

14.4: do arise in constricted systems

Changed.

14.7: for a period…

Changed.

14.18: pressure for between …surge-type

Changed.

14.21: do you mean transit, rather than transition?

Yes, we have changed this.

14.23: such bulls-eye patterns have also been found on outlet glaciers in northern Ellesmere Island by Laurence Gray (published too)

We apologize, we cannot identify the article that the reviewer is referring to. The Gray (2011) paper in GRL does mention regions of ice surface uplift and subsidence but does not discuss the state of the hydrological system (for example constricted vs. efficient) in reference to this, which is the primary link between our modeled pressure wave outputs and the analysis of the 'bullseye' patterns by Fatland and Lingle (2002).

14.24: period of 2 to 4.5 years…

Changed.

14.26: "due to similar criteria" is a strange way to say this.

We have changed this to "due to similar factors".

15.1-15.3: you seem to treat lake water volume and water pressure as synonymous here, but I don't think that is correct if the system is in contact with the ice and water outside the lake basin

Here we summarize the jokulhlaup theories of Nye (1976) and Fowler (1999) where leakage from the subglacial lake changes the downstream water pressure and hydraulic potential that eventually allows drainage. To clarify that we are not discussing our own outputs we now state: "The lake water accumulation in the latter type of system is driven by increased melt through volcanically-inducted heating. In these lakes, as the water builds up in the basin, some leakage seeds channel growth …"

15.15: can span two pressure waves

Changed.

15.16: pressure wave forms to conduct

We have changed the sentence to read "channels are efficient enough by the time the second pressure wave forms to conduct the extra water".

15.19: pressures from developing downstream

Changed.

15.20: wave passes…sizes of channels….are crucial….demonstrate that

Changed

15.25: how can a lack of cycles be similar to a model of cycles?

Here we intended to discuss a lack of repeating cycles rather than a lack of cycles altogether. For clarity we have changed our phrasing to "The lack of repeating cycles of lake growth and drainage is similar to the jokulhlaups modeled by…".

15.29: drainage cycles

Changed.

16.3: do you mean "linked to the passage of the wave"?

Changed.

16.8: not that 2-D hydrological networks exist in reality!

Changed from "…2D hydrological networks in addition to 1D models" to "…2D modeled hydrological networks in addition to 1D models".

16.11-16.12: rather than prevents it – but would this be geometry specific or general?

We have rephrased as suggested and to clarify we have added "the overdeepening in the current configuration" to illustrate that this is reliant on geometry.

16.16: exist at pressures slightly below…

Changed.

16.19: and at the top of the adverse slope is a key….

Changed.

16.24: data have been (data = plural of datum)

Changed.

16.26: where are these cycles seen?

We have added "…in various regions of Antarctica (e.g. Gray et al, 2005; Wingham et al, 2006; Fricker et al, 2007; Fricker et al, 2014)" to the end of this sentence in order to direct readers to some of the relevant literature.

16.28: and drainage demonstrated by the model..

Changed.

17.1: and to extend the record…. (but why no reference to CryoSat2 as a source of altimetry data?)

We now state that "Ice surface elevation data have been available from satellite-based sources such as ICESat and CryoSat2." and "It will require continued data from CryoSat2 and from systems such as ICESat2, due to launch in 2017, to extend the record and allow assessment of the system development over the next couple of decades".

17.3: forms in the overdeepening….basin to a depth of 150m

Changed.

17.4: for attempts to calculate

Changed.

17.7: water flux across a hydraulic equipotential surface

Changed.

17.15: are important for..

Changed.

17.19: rates inferred from altimetry.

Changed.

17.20: meters are also consistent with rates…..over Recovery

Changed.

17.22: The area of our lake is…

Changed.

17.25: and has an area of 60 km2

Changed.

17.26: directly with observations from the Recovery system which consists of many lakes within a region of complex topography (bed, surface or both?) . Should cite a source of information about this system

We have now specified that the basal topography is complex and have cited Fricker et al (2014).

17.28: increased in volume by…

Changed.

18.1: flux (not flux rate)

Changed.

18.2: yield lake growth…

Changed.

18.4: Final sentence of para needs rewriting (clearly your results are not **located** between the 2 lakes and, even if they were, this would have no bearing in their quality)!

We have clarified by writing as: "As a result, our model outputs lie within the range of the larger and smaller Recovery lake filling and drainage rates, which gives us confidence in our results."

18.9-10: This sentence needs rewriting – I don't think channels move water..

The sentence has been changed to "The channels are of sufficient size to propagate the high water pressure to ~50km downstream of the lake."

18.12: negative in the region around the lake

Changed.

18.14: I think this sentence needs some elaboration

We have re-written the sentence to say:

"As a result, periods of high water pressure (and by proxy, faster ice velocity) in the vicinity of the lake occur as the lake is growing rather than when it is draining; conversely, high water pressures due to lake drainage are found downstream."

18.17: I presume you mean measurements rather than calculations here? But you seem to be treating ice velocity and water pressure fluctuations as synonymous, which is not wise.

We have changed 'calculations' to 'measurements'. When discussing the impact of modeled hydrological development on ice dynamics, in the absence of a fully coupled model, it is common practice to use the effective pressure as a proxy for ice velocity. We now clarify this in our introductory aims by saying we examine "the impact of the lake drainage on downstream water pressures and, by proxy, ice dynamics". Also see above changes to sentence 18.14 where we reiterate this.

18.17: such high pressure…

Changed.

18.23: attain large sizes – (though the actual sizes reported are not especially large, given the size of the system being studied)

We have removed this phrase so the sentence now reads: "In our modeled Antarctic system, however, channels can persist for a number of years."

18.25: The faster rate of shrinkage, relative to the rate of channel growth, is (BUT is it not really just a consequence of the non-linear form of the flow law of ice and the fact that water pressures drop very rapidly when a channel ceases to be full of water?)

It is correct that the faster shrinkage rate is due to the non-linear form of the flow law. To clarify we now say: "The faster rate of shrinkage, relative to the rate of channel growth is a result of the non-linear creep of ice closing the channel once pressures drop below overburden."

19.1: as a pressure wave migrates through..

Changed.

19.4: that can cause channel growth over many years

Our phrasing was intended to imply that channels take many years to form due to constant water supply so we wish to have the sentence emphasis on the water supply *rather than the channel growth. We have added in punctuation to clarify so the sentence now reads: "that, over many years, can cause channel growth".*

19.5: smaller, so water flux through these systems will be lower during the winter months.

Changed.

19.11-19.15: Para is really irrelevant unless you make a case that channels incised into bedrock influence the pattern of drainage development and pressure wave propagation in subsequent years

We have removed this paragraph.

19.17: is highly simplified. For instance….rates, or variable

Changed.

19.19-19.20: but you only gain insight into the real system if the model simplifications don't significantly change the physics

With our approach of studying a synthetic system we do not change the physics of the hydrology equations that are being applied; instead we are limiting complications from aspects like highly variable topography or basal sliding rate that could cause the pressure wave and lake growth and drainage effects that we see. We now address this in the introduction by saying:

"Using this simplified system allows us to identify hydrological controls on lake dynamics and examine the wider catchment without complications of highly variable basal topography."

We plan to later apply the model to a system with variable topography and variable basal sliding included but, in order to best asses the outputs, a synthetic model is an excellent way to test the parameter space. As we discuss in the model limitations, it is possible that basal materials such as sediment could be present, which would require different equations that are not readily applicable in this type of model. We suggest that this is an area of future work.

19.22-19.24: The justification presented here is really weak

We are unclear what the reviewer is referring to here. If they are discussing the approach of using linked-cavity equations for a distributed drainage system we now elaborate on this in the model configuration section by saying:

"GlaDS is primarily set up to deal with distributed linked cavity systems. However, a sediment based distributed basal drainage system may behave in a similar fashion (Creyts & Schoof, 2009). By testing a range of conductivities in the distributed system we

can emulate Darcian flow through sediment along with more conductive cavity-type systems. Sediment deformation processes, which could be important in ice stream hydrology and dynamics cannot, however, be taken into account with this model configuration."

19.24: Why can't you have linked cavity systems at the surface of a sediment substrate, especially if it contains large clasts?

Yes, it is possible that linked cavity systems can exist on the surface of sediment, particularly with large clasts. This is demonstrated in the modeling work of Creyts and Schoof (2009) and is the study that we reference in our discussion here of the ability of GlaDS to emulate drainage in a sediment-based system.

20.1: tests to assess whether the pressure waves are …

Our tests were set up to make sure numerical artifacts were not causing the pressure waves and so we retain our wording.

20.4: obtain similar results as..

Changed.

20.4: "errors" – don't you mean differences – you have no idea what is reality so how can you refer to them as errors?

Changed to 'differences'.

20.8-20.19: The question raised by this paragraph is how value are the insights derived from the study given its obvious limitations?

All numerical models have limitations as they are merely a representation of nature. We are upfront about the limitations of our approach so that future approaches can expand on ours. Given the difficulty of in situ access to the subglacial systems of ice streams we are, however, limited at the present to primarily relying on models. As long as our readers are aware of what the model can and cannot address, the outputs can be used to drive future research and initiate hypotheses about the systems we discuss.

20.22: could allow downstream flow of water to occur….model, perhaps reducing the local…

Changed.

20.23: Why is it unlikely?

It is unlikely that the pressure waves would be entirely removed by including flexure in the model because the pressure wave propagates faster that the likely viscous response *time of ice. However, it is due to the fact that we cannot fully ascertain this that it is included in the model limitations. We clarify in the text by writing:*

"However, given the propagation speed of the pressure waves and a viscous response time of ice on the order of months, it is unlikely that flexure would entirely remove the pressure waves; instead it might change the downstream speed of the wave."

21.3: through internal dynamics alone.

Changed.

21.2-21.5: I would argue that the model results generate potentially testable hypotheses. As things stand we really have no idea whether we should believe them or not – so to argue they produce new insights seems like a stretch to me.

Our use of 'insights' is to ascertain likely features of the hydrological systems using the model, which can then be tested through data collection, rather than using a model to fully determining the basal hydrological conditions. We clarify by writing:

"The outputs therefore provide new insights and suggest directions of further research related to hydrological development and subglacial lake dynamics in Antarctic ice streams."

21.7: delete development – you aren't simulating how the drainage network develops as far as I can tell

As this is a spatially and temporally evolving model of subglacial hydrology it is primarily simulating drainage network development. It is this that allows pressure waves to migrate through the system and the lakes to grow and drain. We therefore retain our original phrasing. This is now clarified in our aims in the introduction.

21.11: efficient drainage networks

Changed.

21.13: delete "instead"

Changed.

21.16: water pressure peak

Changed.

21.18: can persist at such levels

Changed.

21.20: over scales similar to those observed beneath Antarctic ice streams.

Changed.

21.22: occurs only when

Changed.

21.25: do these pressure waves result in hydraulic jacking at the bed?

We address this when we discuss the possibility of ice flexure as a result of pressure waves. However, we also point out that the related change in water thickness is minimal and so that physical jacking is likely to be minimal.

22.3-22.5: Final sentence need rewriting

We have re-written the final paragraph. It now reads:

"The results from this synthetic ice stream hydrological experiment suggest that the Antarctic basal systems can be highly transient and variable with interactions between water pressure and channel growth that occur over a scale of years. These results encourage further analysis of Antarctic ice stream velocities, which could show an imprint of such a system. Future work will involve applying this model to Recovery Ice Stream using realistic topography in addition to adding in ice flexure and ice dynamic components to the model setup."