This document lists all the changes, and responses to the reviewers' comments. The changes have also been highlighted in the .pdf document. For reference, we have kept the **original comments in bold blue text**, and the *original replies in highlighted in italic.* The changes done in the final document are in normal text.

Changes based on comments by Elizabeth Bagshaw

The paper presents a new method for calculating path length of an englacial drifter deployment by using the IMU data gathered in a model. The drifter deployment is a significant technological achievement that has resulted in a previous publication in TC. The added value of this paper is the method to utilise the data to generate a more accurate assessment of position to couple with the pressure readings from the drifter. It is a valuable contribution to the emerging arsenal of in situ measurements from englacial and (one day!) subglacial systems. The application of a model to deal with the complex IMU data is welcome.

Reply: *We thank the referee for this overall positive assessment of our work.*

Changes: N/A

The paper hangs on the comparison of the model constructed from one prototype drifter vs. data from another prototype drifter. Whilst I appreciate that the GNSS is quite likely to be reliable, it would be useful to give a few more details beyond a quick citation. How can the reader be confident that the 'reference' reports the 'correct' results for comparison? You state in line 232 that the GNSS is 'not the most accurate' and give some generic errors for the method. I would like to see much more detail about the reliability of this drifter, as well as the method in general.

Reply: *This is a very valid point and we will hence add a couple of sentences to detail the used GNSS drifter, the processing of its data and its accuracy.*

Changes: Additional details from the GNSS data have now been included in the "Supraglacial reconstruction and validation" section where the GNSS results are used (Section 3.1.) We calculated and are reporting the average error between the GNSS drifter measured locations and the average path used. That shows that the GNSS recording error is within the standard expected GNSS error.

The discussion is missing comparison with other published works beyond those used in the generation of the results. For example, I think comparison with the work of Church et al. at the Rhonegletscher should be considered (https://tc.copernicus.org/articles/14/3269/2020/). This uses radar to map an englacial channel: how do the methods compare? The authors should remember that this is a Cryosphere journal, not a technical development paper, so make every effort to demonstrate how their method compares with others. I would also recommend looking to other disciplines for validation: for example, Maniatis 2021 reviews the application of IMU sensors for geomorphology (https://onlinelibrary.wiley.com/doi/abs/10.1002/esp.5197). I would also recommend closing the discussion with a sentence on the glaciological implications of the paper. Essentially: we can measure step-pool sequences, so what…?

Reply: *This has also been remarked in the review by Ugo Nanni. We will therefore add an additional paragraph in the revised version of the manuscript where we will compare our method to other current work (e.g. Church et al.). We would further like to thank for pointing us to the paper from Maniatis*

(2021), as we haven't come across it yet. We will further close the discussion with a sentence about glaciological implications.

Changes: Comparison to Rhonegletscher has been added to the discussion. In addition, advantages and limitations of using submersible drifters have been added.

In Maniatis 2021, the author starts a discussion to try and work towards collective agreement on a presentation/reporting protocol of IMU data in geomorphology. As the issues he brings out in the paper are valid also in ours, we have added a citation to Maniatis 2021 review paper at the end of the discussion.

The discussion now includes the glaciological implications of the paper.

Detailed comments

Sentence 1 and 2 of the abstract don't really follow – sentence 1 states glacier hydrology is about the whole system, whereas sentence two states it is purely subglacial pathways. Suggest rephrasing sentence 2. Sentence 3 also needs attention: two 'pathways' in same sentence. I think the abstract needs to be upfront about these being englacial measurements, and why those are important for glaciology: at the moment you undersell what is a great technological achievement by trying to frame it as a subglacial experiment when you did not go into the subglacial environment.

Reply: *We will follow the suggestion of rephrasing the abstract and putting more emphasize on englacial measurements and the technological achievement.*

Changes: The abstract has been rephrased and more emphasis has been put on the technological achievement of the englacial measurements.

L42-49: this paragraph doesn't quite convince me that this is new and exciting work. To the non-specialist (in this case, most of the readership of the Cryosphere), the follow on work from the last paper sounds incremental. Can you explain what you did and why it was important in more simple terms in this paragraph?

Reply: *Thanks for pointing this out. We will rephrase this paragraph to highlight the advancements of this paper better.*

Changes: We have rephrased this paragraph.

L65: '65 kg heavy' doesn't quite work. Remove the heavy or replace with mass.

Reply: *We will remove the word 'heavy'.*

Changes: The word "heavy" has been removed. The sentence has been restructured.

L74: suggest clarifying that these are long-lived englacial channels.

Reply: *Good point. We will clarify this to avoid any confusion of the reader who might otherwise compare our results with short-lived englacial channels on alpine glaciers.*

Changes: We specified that the englacial channels are "persistent".

Table 1: can you define 'complete data deployment' in the legend?

Reply: *Yes, we will define this in the legend.*

Changes: Clarification has been added to the table title.

Figure 4 is really helpful. Can you give a reference for iHMM in the legend – it is described in detail in the text, but the figure appears before.

Reply: *Thanks, you for making us aware of this, we will add the reference to the legend.*

Changes: We have changed the acronym in the figure to full wording and added a reference.

L177: Here you state the geometry of the channel is known (presumably because you can see it), but state the reference is the GNSS drifter. As stated above, this is also a prototype. What I'm wondering is if you just mapped the channel either doing a walkover or aerial imagery to validate your reference? You state later that you attempt to use Planet imagery for the englacial channel – did you try this for the supraglacial channel?

Reply: *As the channel is located outside the research village Ny-Alesund, it has regularly been visited and studied, both as part of research and of leisure time activities. As a result, several unpublished maps of the channel exist. An example of this is the work of Kamintzis et al. (2019) who published LiDAR scans from an englacial channel higher up on the same glacier. The authors have, however, also scanned the same englacial channel, as investigated in this manuscript. Their impressive scans can be found here:*

https://www.youtube.com/watch?v=eATAsRcINWk

and here

http://www.derij.co.uk/images/panos/Portal/Portal.html

There is, however, no publication about this dataset and we were not able to get in contact with the corresponding author. As details about the data collection and processing of this work are unknown to us and we do not have access to their 2D map of the channel and other private mappings from local village residents are of unknown quality, we have chosen to use our own GNSS drifter investigations. As the channel was full of water and with a partly collapsing roof in 2020, we were not able to do a walkover. We also did not have access to a drone (drone flying is prohibited by default in Ny-Alesund). We did try to map the supraglacial channel on Planet imagery. The resolution of the imagery is, however, too low to identify the channel.

We will add reference to the unpublished dataset from Kamintzis et al to allow the reader gain a better idea/ understanding of this particular englacial channel.

Changes:

We have added reference to the LiDAR work by Kamintzis et al. from 2017.

L215-222 is really interesting. I like the comparison with historic data.

Reply: *Thanks. This particular channel is especially useful for this, as published work about this channel is going back to the 1980s and unpublished evidence/ work all the way back to around 1920 (quite different from rapidly changing englacial channels in alpine glaciers). The discussion of this data would, however, be outside the scope of this manuscript.*

Changes: N/A

L224: the motivation for the paper here is not the same as is sold in the introduction. Hydrological models of glacier dynamics – how do englacial measurements help with that?

Reply: *You are correct. We will adjust this.*

Changes: We have reformulated this sentence.

You state there are handheld GPS measurements (L432) – it's not clear how these measurements have been used. Are they plotted?

Reply: *They are indeed not plotted, only used to measure distances within GIS. We will clarify this.*

Changes: We have specified that the coordinates were used within QGIS.

L271: over what path length? You lost one drifter in a short englacial deployment, so how can you justify that you need three for another deployment? Please be specific on the likely path length for this assessment.

Reply: *Good point. We will specify this. And following the early eTracer work (Bagshaw et al. 2012) with the reported increasing equipment losses with increasing distance from the glacier margin, the required number of deployments would be likely much higher for longer path lengths.*

Changes: We have added additional details in the discussion section.

L280: 'decent' is not a scientific assessment. Rephrase.

Reply: *We will rephrase this.*

Changes: Rephrased. "Decent" has been removed.

Conclusions reads like the final paragraph of the discussion. It brings in new information (there is little detail about the gravity vector problem in the main body of the results and discussion) and highlights a number of method weakness. In my opinion this should be in the discussion, with the concluding paragraph summarising the paper as a whole.

Reply: *Thanks for pointing this out. We will move the paragraph in question to the discussion section of the manuscript and rephrase the conclusion to summarize the paper.*

Changes: The Conclusion has been rewritten and as suggested, the paragraphs in question have been moved to the discussion.

Changes based on comments by Ugo Nanni

The study is a methodological contribution that presents a new method to map the topology of supra and en glacial flow and retrieve the water pressure along flow. Such method will benefit to the investigation of glacial hydrology and its influence on ice dynamic. The paper well presents the method. However, the purpose of the work is not clearly articulated and it lacks a wider comparison with other methods (dye tracing, GPR, seismic, satellite observations) that would help to highlight the benefits of this new methods. It also lacks information on what kind of observation are currently needed by the glacial hydrology community and how this new method may contribute to provide such observation.

Reply: *We thank the referee for pointing this out and we will, following his suggestions and the ones from the second referee, adjust the manuscript to include a wider comparison with other methods as well as more information on currently needed observations.*

Changes: Additional comparisons have been made for GPR. Details about the proposed method's advantages as well as limitations have been added to the discussion.

I think that the proposed method is valid and suitable as well as very original and will bring new and useful information about glacial hydrology, at relatively low cost and easy deployment. It is therefore important for such method to be shared with the glaciological community.

Reply: We thank the referee for his positive overall assessment of the method.

I support the publication of this manuscript on the condition that the authors highlight (1) the advantages and limitations of their method (e.g., water pressure measurements, applicability to other setups, to subglacial environments …) (2) how their method finds its place in the current methods used to observe and model glacial hydrology and the associated challenges. Such changes might necessitate to revisit the structure of the discussion and conclusion sections as well as to provide some changes on the introduction.

Reply: *We will adjust the manuscript to better describe 1) advantages/ limitations of our method and 2) place it better into context of other current methods. For this we will add additional details to introduction, discussion and conclusion section of the manuscript.*

Changes: Additional details have been added to the introduction, discussion and conclusion. The Discussion and Conclusion have been reworded, some of the text from the conclusion has been moved to discussion, as suggested by Elizabeth Bagshaw. Advantages and limitations have been added to the discussion.

Detailed comments:

TITLE: Topology and pressure distribution reconstruction of an englacial channel

"In this paper, you show a few pressure changes because the water is free flowing, so you do not really show your capability to investigate pressure distribution, I would not stress that much on this aspect in the abstract or the title."

Reply: *We disagree. Indeed, the channel is free flowing and the pressure changes within the channel are rather small. The goal of this manuscript is, however, not to go into scientific details of pressure changes*

within a channel (pressure variation), but to provide a method with which in situ measurements can be spatially referenced. For this it doesn't matter if pressure changes are small or large. Our method could also provide spatial reference to other sensor modalities (e.g., the magnetometer readings from the IMU). Those are, however, not of relevance to the glaciological community (e.g., magnetometer, gyroscope) or are currently not implemented in our drifter (e.g., temperature, conductivity). To avoid the current confusion that we are not talking about the variation of the pressure itself, but it's spatial distribution, we will add the word "spatial" in front of "pressure distribution" in the title and the abstract.

Changes: We added the word "spatial" in front of the "pressure distribution". The title is now reading "Topology and spatial pressure distribution reconstruction of an englacial channel"

ABSTRACT:

FP: 'Water reaching the ice bed influences ice motion and ice dynamical models, therefore requiring a good understanding of glacier hydrology, particularly water pressures and pathways.'

Comment: "this is not very clear. I see what you mean, but I suggest rephrasing. Water does not directly influence ice models."

Reply: *Thanks for pointing this out. We will rephrase this.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract.

FP: 'However, as in situ observations are sparse and methods for direct observations of water pathways and internal pressures are lacking, our understanding of the aforementioned pathways and pressure remains limited.'

Comment: "what do you mean by internal? Specify; "lacking is too strong, for instance borehole measurements can be used to measure pressures"; "What do you mean by aformentioned ? I would suggest rephrasing to me more precise"

Reply: *We will rephrase this sentence to clarify it.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract.

FP:' Here, we present a method that allows the reconstruction of planar subsurface water flow paths and spatially reference water pressures.

Comment: "this is not clear what you mean here. I understand that you map pressure, but I would suggest rephrasing."

Reply: *We will rephrase this sentence to clarify it better.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract.

FP: ' We showcase this method by reconstructing the 2D topology and the water pressure distribution of an englacial channel in Austre Brøggerbreen (Svalbard).'

Comment: "the data you show are also obtained on a surface channel and the englacial part you studied is mainly free flowing, I suggest adding this particularity here to do not oversale the study"

Reply: *We will add detail about the channel being at atmospheric pressure.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract.

FP: 'Validation on a supraglacial channel shows an average length error of 3.9 m (5.3%).'

Comment: "is it the same channel? You need to precise it, or say that you did the validation on the open part of the channel."

Reply: *This sentence relates, as stated, to the "supraglacial channel", not the "englacial channel" and is therefore not the same channel. We will clarify this better.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract and highlighted that it is a different channel.

FP: 'Our method allows mapping sub- and englacial flow paths and the pressure distribution within, thereby facilitating hydrological model validation. Further, our method also allows the reconstruction of other, previously unexplored, subsurface fluid flow paths.'

Comment: "I do no think you prooved that the method can be used to map subglacial flow. I suggest to only say "mapping glacial water flow paths""; "Here you just show a few pressure changes because the water is free flowing, so you do not really show your capability to investigate pressure distribution, I would not stress that much on this aspect in the abstract or the title."; "This (hydrological model validation) is not supported in your study, this is speculative on the outcomes of your study"

Reply: *We refer to our previous comment in regards to "pressure distribution". We will rephrase "subglacial flow" to "subsurface flow". We will further detail more what we mean in terms of model validation as we indeed had hydrological potential modelling in mind, where the most likely flow path for water is modelled. This could indeed be validated by our method and these models are currently already questioned by maps of englacial (Hansen et al. 2020) and subglacial channels (e.g. Gulley et al. 2012). We will provide additional reference to these papers and specify this sentence.*

Hansen et al. 2020: doi:10.1017/jog.2020.1

Gulley et al. 2012: doi: 10.3189/2012JoG11J189

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract.

FP: 'Further, our method also allows the reconstruction of other, previously unexplored, subsurface fluid flow paths.'

Comment: "This last sentence is not clear, please detail or remove. What do you mean by previously unexplored ? This is not the first time people map englacial / supra glacial channels."

Reply: *As stated, we refer to "other" "subsurface fluid flow paths". So, this is not about englacial or supraglacial channels but we were thinking about e.g., pipelines, karst caves, sewage systems and the like where in situ data currently doesn't exist. We will specify this better.*

Changes: Based on this comment and the comment by Elizabeth Bagshaw, we have reworded the abstract. We provide examples of other areas where the method might be useful.

INTRODUCTION:

FP: ' Water movement through and under glaciers and ice sheets in en- and subglacial drainage systems is an essential factor in the control of ice dynamics (Hubbard and Nienow, 1997; Fountain and Walder, 1998; Irvine-Fynn et al., 2011).'

Comment: "do englacial flow really influence ice dynamics ?"; "these refererences mainly concern mountain glaciers and not ice sheets. I suggest adding appropriated references"

Reply: *We think these references are appropriate for our case, as we study a small Arctic valley glacier. We will therefore remove the part about "ice sheets" from this sentence instead of adding references for ice sheets, as our method is most likely rather limited for the use at ice sheets due to the current necessity of instrument retrieval.*

Changes: We have removed the word ice sheets.

FP: 'Such systems vary in space and time, and their configuration is traditionally inferred using the physical principles (Röthlisberger, 1972) and concepts of hydraulic potential (Shreve, 1972) developed 50 years ago (see review by Flowers (2015)).'

Comment: "what do you mean here ('traditionally inferred'). I suggest adding more precision on how these systems vary in time and space, how they are expected to vary and also how observations confirm or challenge these expectations."; "I think you need to precise what you mean here by "physical principles"."; "this is a physical principles. I suggest to add some details to this sentence so the reader can better understand the physic that is required to understand these systems"; "this is not because it was developped 50 years ago that it is necessarily outdated. I do not see the benefit of stressing this, as it already visbile in the dates of references."

Reply: *We will rephrase this part and add additional details.*

Changes: We have rephrased this part and added additional details.

See comments for more details (annoted pdf and comments pdf). My comments are aimed at highlighting where changes could be made to improve the clarity of the manuscript.

Please also note the supplement to this comment: https://tc.copernicus.org/preprints/tc-2021-377/tc-2021-377-RC1-supplement.zip

Reply: *We thank the referee for the detailed comments in the annotated pdf. We have answered these comments directly in the pdf, which is attached.*

Changes: The line-by-line changes below will address the comments from the pdf where applicable.

Line by line changes based on the annotated pdf

The line numbers are based on the Ugo Nanni's edited .pdf file.

The following section highlights all changes made in the manuscript; based on both reviewers' comments and the annotated pdf.

TITLE

- In the title, word "spatial" has been added in front of the "pressure distribution". The full title now reads "Topology and spatial pressure distribution reconstruction of an englacial channel"

ABSTRACT

- LINE 1-12 – Based on the comments by both reviewers, large parts of the abstract have been rewritten.

INTRODUCTION

- Line 14 – We removed "ice-sheets"
- Line 15 - Combined with the previous note, as the concentration on the work is on small arctic valley glaciers, and the "ice-sheet" was removed, the citations have been decided to be left the same
- Line 14-22 – We have reformulated this paragraph, taking the reviewer comments into account
- Line 23 – A discussion about time-consumption and comparison of methods has been added to the discussion section
- Line 24 – Changed citation to Nanni et al 2021 where seismic arrays have been used.
- Line 25 – Added citation to Church et al 2021 where 3D GPR survey of englacial channels have been done
- Line 27 – Changed "seismic array" to seismic observations and added citation to Gimbert et al 2016
- Line 29 – Added citation as requested
- Line 30 – Clarification that the first is indirect measurements and second is point scale.
- Line 30 – Added details
- Line 35 – rephrasing to remove "We have"
- Line 40 – Additional details added to make the aim of the paper clear, added details about the concentration of the paper.
- Line 41 – Citation format has changed so it doesn't involve double brackets.
- Line 43 – Replaced "here" with "In this paper"
- Line 44 – Additional citation added, the citation previously existed in the methods section.
- Line 45 – Replaced "As such" with "Hence"
- Line 42-48 Reformulated paragraph

MATERIALS AND METHODS

- Line 50 – Added a reminder to the reader about what our method is meant to do
- Line 59 – Line break has been removed
- Line 65 – Replaced "drifter" with "drifters"

- Line 73 – fixed reference
- Line74 – rephrased sentence
- Line 75 – Changed sentence structure for better readability.
- Line 76 – Added figure cross-reference
- Line 80 – Added figure cross-reference
- Table 1, title – Added details on what is meant by “complete data”
- Table 1, last column – specification on how many GNSS measurements were done.
- Figure 3 – Added scalebars in panel b) and c)
- Figure 4 – iHMM has been changed to infinite hidden Markov model
- Figure 4 – Added citation to the caption after (iHMM)
- Line 88 – We have gone through the document and replaced “flow path” with “water flow path”
- Line 89 – Added space
- Line 90 – Added citation to quaternions
- Line 91 – specified water flow path
- Line 93 – specified water flow path
- Section 2.4 - Additional details about noise removal have been added
- Line 99 – Clarification on the datasets
- Line 101 – Clarification on the datasets
- Line 103 - replaced “without” with “that helps to not”
- Line 107 – clarification of thresholds used
- Line 109 – replaced “acceleration” with ”accelerometer”
- Line 114 – Clarification added about computational complexity
- Line 116 – Removed “only”
- Line 117 – Removed “some”
- Line 119 – Clarification on HMM explanation
- Line 131 – Typo fixed
- Line 132 – Clarification on wording. The limitations of HMM are addressed by using iHMM
- Line 161 – Replaced “As a result” with “The model proposed gives”
- Line 165 – Clarification on path estimation
- Line 169 – Clarification on how magnetic declination is corrected
- Line 172 – Removed “thereby”
- Line 173 – Wording

RESULTS

- Line 177 – Replaced “We tested our approach” with brief recap.
- Line 178 – Clarification on path
- Line 182 – Clarification on wording
- Line 183 – As we start by presenting the reconstructed water flow paths, we moved the citation to the Figure 5 in the beginning of the section where we mention reconstruction.
- Line 183 – removed “therby”
- Line 186 – Clarification on wording
- Line 188 – Cross-reference to the corresponding figure added
- Line 191– Clarification on “Water flow path”

- Line 195 – Added reference to Figure panel (a) in the beginning of the section.
- Line 196 – Clarification on wording
- Line 198 – removed “overall”
- Line 205 – Removed “thereby”
- Figure 5 – Removed GNSS reference path from figure (a), the reference path is only on figure (b)
- Figure 5 – Added scale. Added deployment and recovery.
- Figure 5 – Correction on the Figure caption
- Figure 6 – Clarification on Figure caption
- Line 208 – specified with figure reference
- Line 209 – removed “Thereby”
- Line 209-210 clarified
- Line 216 – Added cross-reference to corresponding figure
- Line 216 – Added cross-reference to the corresponding figure
- Line 217 – Removed “thereby”
- Line 220 – Removed “thereby”

DISCUSSION

- Line 234 – Removed “Thereby”
- Figure 7 – Added scale, more defined deployment and recovery.
- Figure 7 – Correction on the caption
- Line 251 – Reworded and added reference
- Line 251 – changed wording
- Line 252 – Changes made in previous sentence
- Changes in discussion based on comments.
-

CONCLUSION

- Line 261 – Addition to the Conclusion to read like a conclusion. Some text moved to discussion for clarity

Changes based on comments by the editor Daniel Farinotti

See comments for L. 207 and 256: I'm not entirely convinced that "pressure error" is the correct wording here. I rather understood it to be the standard deviation of the measured time series, which looks more like an indication of temporal and spatial variability.

Changes: Reworded to standard deviation.

This reference should be given as "\citep", rather than "\citet", I guess?

Changes: Fixed citation

Does this mean that the drifter needs to be retrieved for data collection? If so, that would be important information to be passed, as it looks like a rather strong limitation when targeting en- or subglacial channels.

Changes: Added limitation in regards to recovery to discussion

I might have missed it but how was this "satellite derived path" actually derived? Was it "just" by digitizing the path as seen on a satellite image? If so: What image? From what satellite? And for which date? If differently: How?

Changes: Specification added in section 2.2.

This is the standard deviation of the recorded time series, I guess? That should be stated more clearly.

Changes: Specified.

This sentence calls for a reference to a figure (and indeed it looks like Fig. 7b has this info)

Changes: Figure reference added.

Not entirely sure about the wording here: from L. 207 I understood this to be the standard deviation of the recorded time series, which to me is an indication of pressure variability, and not of "measurement error". Possibly clarify.

Changes: Reworded.