

Response to the review by Elizabeth Bagshaw

First of all, we would like to thank Elizabeth Bagshaw for taking the time to read through our current manuscript and provide very helpful feedback, which will certainly help to improve the quality of the manuscript. In the following we present our responses to the referee comments and how we will address these in the revision of the manuscript.

The referee comments are presented in ***bold and italic***, our replies follow immediately thereafter.

Overall comments

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.

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

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.

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.

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...?

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.

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.

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

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?

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

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

We will remove the word 'heavy'.

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

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.

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

Yes, we will define this in the legend.

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.

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

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?

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.

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

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.

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?

You are correct. We will adjust this.

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

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

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.

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.

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

We will rephrase this.

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.

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.