

The Cryosphere Discuss., referee comment RC2  
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## **Comment on tc-2021-377**

Elizabeth Bagshaw (Referee)

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Referee comment on "Topology and spatial-pressure-distribution reconstruction of an englacial channel" by Laura Piho et al., The Cryosphere Discuss.,  
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### **Review of Piho et al.**

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.

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.

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...?

### **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.

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?

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

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

Table 1: can you define 'complete data deployment' 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.

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?

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

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

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.

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

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.