The Cryosphere Discussions

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Comment on tc-2021-2

Anonymous Referee #2

Referee comment on "Ice roughness estimation via remotely piloted aircraft and photogrammetry" by James Ehrman et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-2-RC2, 2021

The authors present a case study from a river in Canada, using a drone to estimate the roughness of the ice surface in winter. The surface (ice-air) roughness is used to estimate the subsurface (ice-water) Mannings coefficient, and these estimates are found to correlate with other estimates of the same coefficient using the Nezhikhovskiy equation, which uses ice thickness as an input variable.

The paper seems novel and useful, since using a drone to estimate surface roughness is less hazardous then coring river ice to measure its thickness in winter. The outcome of the workflow—an estimate of the water-ice roughness—is important for understanding ice dynamics in rivers. I think that, overall, the paper could be a valuable contribution to The Cryosphere. However, I have three major concerns, which are:

- The authors state in the abstract that the central hypothesis of the paper is that “the surface roughness of a newly-frozen fluvial ice cover is indicative of the subsurface roughness.” However, in the body of the manuscript, they jump immediately into the details of how surface roughness is calculated, almost without discussing the hypothesis at all. Somewhere in the introduction or the background sections, the authors need to discuss their rationale for this hypothesis. What is the physical reason for suspecting that surface and subsurface roughness are connected? As an outsider from this field, it seems to me that the surface and subsurface of the ice cover are subject to very different environmental conditions. The authors have data from this case study that seems to indicate that the surface and subsurface roughness are linked, but they should explain why they expect this to be the case in general. Also, the authors should indicate from earlier on (possibly in the abstract) that “subsurface roughness” refers to the water-ice interface. At first I wasn’t sure if that was the case, or if they were referring to the roughness of the underlying riverbed.

- The estimates of surface roughness from the drone seem to correlate well with estimates of subsurface roughness from the Nezhikhovskiy equation. This seems to be the most important outcome of the paper, and it shows that the method is promising, but it is also based on just a handful of sites along a single river. In the Introduction and Discussion sections, I think the authors should state that this is a preliminary test of the Method, and additional data from other field sites is necessary in the future to determine how effective it truly is.
- In its present form, the writing in the paper is often awkward and unclear, which takes away somewhat from the message. Grammatical errors are common (for example, “its” and “it’s” are often confused), the language often seems unnecessarily wordy, and some sentences are even left incomplete. I think the authors should do a close reading of the manuscript to improve the clarity of the writing.

Some more specific comments are:

- The abstract seems to be longer than necessary. For example, the first three sentences provide background information about photogrammetry which is more fitting in the Introduction or Background sections. The authors should try to get to the point more quickly, which is that they are testing a method using drones to estimate the ice-water roughness, and the method is valuable because it is less hazardous than the more traditional route of measuring ice thickness in order to apply the Nezhikhovskiy equation.
- The Background section is difficult to follow as it lacks a clear structure, lumping together a description of the field site, details about traditional methods for estimating roughness and the Mannings coefficient, and details about photogrammetry. I think this section should be broken into at least three subsections with appropriate titles.
- Since the authors mention that several methods exist for constructing DEMs from overlapping images in lines 129-130, please briefly describe how Structure from Motion differs from the other methods.
- Please briefly define doming errors at the start of the paragraph that begins in line 132.
- What is the rationale behind the codes for the study locations (e.g., DRLL08)? For the purposes of this paper, would it be appropriate to give them simpler names, like Site 1, Site 2, etc?
- Please add more explanation to the paragraph that runs from lines 236-247. I understand that you are removing certain trends from the DEM to better quantify roughness, but I find it hard to follow the details. Please provide more details about the plots in Figs 7a and 7b, and how these plots were used to assign the low-pass and high-pass components of the filter.
- The beginning of the paragraph that starts at line 270 is critical but unclear. What is “observed ice Manning’s n”? Is this the Mannings coefficient as calculated using equation (1) or (3)? Please be very clear here. The first few sentences of this paragraph are difficult to follow and the sentence beginning on line 271 is incomplete.