

Dear Joshua Ran and co-authors,

I have received a new comment to your manuscript from Dr. Robert Way which is highly relevant for the data product you are describing and needs to be addressed during the review (see the supplement)

Kind Regards

Kirsten Elger

Original comment of Dr. Robert Way (Queen's University, Canada; robert.way@queensu.ca)

To the authors of Ran et al (in review - <http://doi.org/10.5194/essd-2021-83>). This is an interesting approach where the authors are combining the Obu et al (2019) FDD/TDD products with advanced machine learning techniques and GTNP datasets to estimate permafrost characteristics and distribution across the Northern Hemisphere. The approach is interesting and has some utility so far that the authors certainly deserve praise. However, I am left with the same thoughts that I have had about many of these empirical-statistical modelling approaches that have accelerated in the permafrost community.

My concern is largely that the increasing number of statistical variables being used in these products does not make up for the lack of quality of the input products. Crucially in the case of two of the most important variables: snow thickness and surficial materials type. In the former case, the authors use snow cover duration as a supposed proxy which many on the author team know is not a suitable replacement for snow thickness. In the latter case, the authors use the Soilgrids250 product which is quite similar to the author's approach methodologically and is a derivative of many of the same input parameters. Critically, the latter product does not include any representation of the glaciomarine limits or of regional glacial depositional history making it unsuitable for specific uses such as permafrost risk mapping in coastal areas. Finally, the large gaps in the regions represented in the statistical modelling mean that the regional variations in the importance of certain other factors is likely to be missed. Alterations to the thermal impact of temperature range was brought up by Dan Riseborough's thesis where lower temperature ranges at similar MAATs can produce colder MAGTs. The geostatistical approaches used in these types of papers would undoubtedly miss this 'process-based' explanation leading to systematic biases.

I can only speak to the regions that I have worked in (Yukon / Québec / Labrador) but I do not find this distribution map (nor many predecessors using similar empirical-statistical approaches) to adequately represent permafrost distribution in Subarctic and coastal northern Canada. This is particularly the case in Québec and Labrador where the lack of inclusion permafrost input data or any metrics of snow thickness / coastal geomorphology leads to a permafrost distribution map that is at odds with our regional understanding. There have been a number of publications which suffer from similar challenges (e.g. Aalto et al 2018; Hjort et al 2018; Karjalainen et al. 2019; Hugelius et al 2020; Olefeldt et al 2016) owing to the heavy reliance

on MAAT/FDD/TDD as a proxy for permafrost distribution without incorporation of the mediating effects of snow thickness. The importance of coastal effects (mostly via wind scouring, temperature amplitude and snow density changes) and coastal geomorphology (marine limit) is also a challenge in the Labrador region as we find permafrost far more likely to occur in the coastal areas versus the interior - opposite to what is seen in this distribution map and those of a variety of other products.

Representing snow thickness and surficial materials are significant challenges that will take time for various products to be generated but using more variables with more advanced statistical techniques is not necessarily a substitute for better quality datasets needed for this type of mapping initiative. In my specific region (which the authors included no data from) these issues lead to a completely wrong representation of permafrost distribution. The newly developed Ground Ice Map of Canada shows a pathway forward for including process-based knowledge in large-scale map generation and I would strongly urge the author team to consider lessons from that product.

Normally, I would not comment on these efforts but admittedly these products are now being used by other studies (e.g. Hjort et al 2018) for infrastructure hazard assessments and it is leading to a misinterpretation of the regions at risk of permafrost thaw. Producing maps of permafrost distribution or infrastructure risk to thaw that are at odds with our detailed understanding of the drivers of permafrost distribution **does have ethical implications** and I would ask the author team to reflect on whether the products being produced should include regions for which these errors are likely to be exasperated. As a co-author of Obu et al (2019), I do also have to acknowledge that I think our paper is also being regularly misused in a similar manner.

As two side notes: (1) Using DZAA data for calibration may mean that thinner permafrost bodies are missed leading to an underestimation of the distribution of permafrost in marginal environments. We observe this at all of our sites in southeastern Labrador (Way et al. 2018). (2) There should be a rationale provided for the use of FDD/TDD from Obu et al (2019) compared to alternatives from gridded climate products from meteorological stations or downscaled reanalysis. I would challenge the authors to evaluate whether these MODIS-based datasets are an improvement in representing air temperatures compared to meteorological products (most studies in North America do not support MODIS inclusion being unbiased).

I will end by stating that the criticism of this paper is not meant to be targeted to these authors as I think this is more of an expanded set of thoughts that extends to this overall methodology. Can we produce (overfit) estimates of various permafrost parameters using random assortments of inputs of varying quality? Sure, we can. But the question is should we? And if we do produce these, what is the value of doing so and what are the ethical implications of not making clear to users that these products are not suitable for regional scale analyses?

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