

Interactive comment on “Deriving Arctic 2 m air temperatures over snow and ice from satellite surface temperature measurements” by Pia Nielsen-Englyst et al.

Anonymous Referee #2

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General: This study aims to produce a 2-m air temperature product for the Arctic region by establishing multiple linear regression models with satellite-derived surface temperature measurements and other covariates. The goal of this study is well stated, and the product is potentially very useful. However, the authors tend to provide over-whelming details on some data processing techniques that already exist in two previous papers while the most important issue on cloud influences are much less discussed. Some parts of methodology and validation may have some mis-interpretation and still needs careful discussions. I am suggesting the following major/minor comments for authors to revise their manuscript.

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Major: It is a bit concerning to me that many methodology materials have already been presented in Nielsen-Englyst et al. (2019) and Hoyer et al. (2018), but the authors spent many pages describing the same amount of details in this paper. I would suggest the authors to only retain the most important information, and then delete all other repeating materials by citing those previous studies (authors should start from already pre-processed data, direct readers to read those two papers, and then leave more room for remaining story). I think it's not suitable to publish the same amount of details in this paper again, especially considering Nielsen-Englyst's other paper was also published in The Cryosphere.

P9 Methods: Why choose 0.5 degree as the matching threshold? I think this may be too large. If the authors reduce this threshold and reduce number of matching pairs, will the results be better? Please show some analyses, or at the very least, this needs to be carefully discussed.

P12L12: I think the highest correlation in ISTskinSeason may be wrong interpretation, because what the authors computed are raw data correlation. Without removing the seasonal cycle, this calculation of correlation will be dominated by seasonal cycle, and of course ISTskinSeason will have better correlation. Since the authors' goal is to produce a T2m product, this product should aim at achieving high accuracy in anomalous T2m days, and also being able to capture general characteristics such as seasonal cycle (for general analysis) and trend (for global warming analysis). In my opinion, I think the authors should aim at achieving the highest anomaly correlation when training their models, and revise relevant parts when interpreting model performance.

There seems to be overwhelming technical details provided, however, the most important issue is on how the cloud days were considered, but this seems to receive the least attention? Can the authors reduce some details as I mentioned in Comment #1, and leave more room for how the cloud days were treated? Those are the most useful and challenging issues for this product.

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P1L16: again, if using this raw correlation, my guesses are that if you simply add a systematic bias (e.g., +0.3 degree C) to the satellite surface temperature to derive T2m, you will probably also get similar high correlation. Can authors perform this simple calculation and demonstrate more clearly on the gains of their regression model?

Minor: P2L26: change to “significantly different”

P7L4-5: I am not familiar with this (18-6, 6-18) way of presenting time. Is this following any convention? If not, I suggest the authors to make revisions.

P9L5: delete “the” before “different regions”

P9: Unclear presentation as to what is the percentage split for training/testing. Please revise presentation in the format of e.g., 80%/20% split.

Table 3: It would help if the authors can use stars to mark significance level of the correlation (e.g., *: $p < 0.05$). . . Plainly presenting the correlation is less informative

P11L7: Warming effects are mainly resulted from high clouds, but low clouds can cool the surface. Was this differentiated? Can authors provide some discussions on this?

P23L21: again, this highest correlation may not be a good indicator for this product to be reliable. Please see my earlier comments and provide some revisions or discussions on this matter.

Abstract: authors should mention their data product's spatial resolution in the abstract.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-126>, 2019.

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