

The opening statement - lines 13 and 14 of page 1 - is thoroughly, demonstrably and emphatically false. Antarctica, essentially 14×10^6 km² of snow and ice (not counting winter sea ice), exists, for six months or more of each annual cycle, in a completely frozen state. Frozen = zero terrestrial ecology/biology/biodiversity. During the 'warm' season (SH summer), the minuscule areas ($< 5 \times 10^3$ km², 0.03% or less of the total area) of Antarctica not snow covered (for hydrological rather than temperature reasons), e.g. McMurdo dry valleys, support a tiny, desperate exotic (and fascinating) mini-ecosystem which has virtually no impact on hydrology or glaciology beyond its restricted boundaries. The biodiversity of Convey, which the authors like to cite, refers to sub-Antarctic islands wherever BAS operates long-term research bases; note that Pete's very good work rarely if ever refers to British Halley Station on the continent (adjoined ice shelf) itself. Biodiversity issues for Antarctica, including invasive species, habitat (sea ice) modification or reduction, competition with human predators (e.g. for krill) occur almost exclusively in the marine realm. Likewise for proposed protection areas. No liquid-water hydrology exists at the surface of Antarctica. Extensive glacier mass balances and motions have little to no dependence on surface air temperature. Snow surface halogen chemistry, particularly within regions exposed to wind-blown sea salt aerosols, does show temperature dependence, on reaction rates and - to less extent - on products, but the authors seem blind to that entire field. Their deep ignorance of Antarctica, even if they had a valid surface air temperature product (which they do not), disqualifies their entire concept from the start. One wishes they might have read some early cross-ice explorers (e.g. Behrendt) or explored IPY blogs from Norwegian or US transects. Do they even know about the snow road? Have they ever heard of crevices, sastrugi, nunataks? Do they know the Mawson story? They demonstrate no competence whatsoever.

Do the authors not understand 24-hour polar night alternating with polar constant daylight? They provide a daytime nighttime data extraction routine (page 3 line 27) which, one can scarcely believe, apparently ignores the entire issue of seasonal light levels (complete light, complete dark). Later (on page 5, paragraph starting at line 5) they describe use of solar angles to calculate hillshading as one of their predictor variables but they give no indication that they understand Antarctica; the description sounds more relevant to mid-latitude Germany.

One appreciates mention and use of the RadarSAT DEM, but even 200 m resolution (which they interpolate to 1000 m) misses most relevant surface texture. Higher-resolution airborne radar surveys over large areas of the ice sheet show flat smooth areas of various extents (over basal lakes) amidst much rougher ridged and fractured ice, often (evidently) with substantial temporal evolution. Again, they apparently have no idea. Their predictors have no relevance.

Their primary tool, MODIS LST, has demonstrated and much-argued weaknesses over snow and ice, both for cloud masks and surface temperature extractions. One might have hoped that Meyer et al (2016, the predecessor to this work and again much cited by these authors) might have addressed if not offered new resolution to some of those known issues but that paper blithely accepts MODIS products (citing primarily mid-latitude terrestrial examples) as de facto valid despite a large, vociferous and continuing debate about applicability, suitability and errors over snow and ice. Until or unless these authors demonstrate and document new algorithms or techniques to improve performance of MODIS products over snow and ice they and we must regard this particular application as un-proven at best. A large literature, none of it cited here, debates these troublesome issues of single or multiple sensors and their individual or combined effectiveness at retrieving surface air temperatures over snow and ice. Again, the authors demonstrate no competence whatsoever in the use of MODIS LST.

After casual application of four different machine learning techniques, the authors in the end **rely on visual inspection!!!** Their complete inability, despite multiple runs of multiple software tools, extensive spatial and leave-out cross-validation, to rely on any single outcome despite extensive statistical evaluations disqualifies the entire effort. This potential user might have asked for fine-scale validation in areas of (relatively dense) met measurements or perhaps RMSE sorted by elevation, but why bother? They literally have nothing valid to show.

To report absolute and RMS errors of 5K seems absurd. Who do they think might use such imprecise unreliable data? From any of several authors (try anything from Scambos, for example) these authors should know concern about long-term climate induced trends of 0.3 to 0.4 °C per decade over higher elevations of East Antarctica. Here they can't provide better than 5K? Over 15 years (assuming their time period of 2003 to 2018 (but apparently 2003 to 2016 according to page 11 line 6)), we might expect temperature change of perhaps 0.6 °C? Even by yearly averaging (1.73 °C, page 11 line 5), they fail to come close to necessary precision. They refer (e.g. page 8 line 31) to "RF being superior in the temporal prediction" but they fail entirely to demonstrate necessary temporal skill. Again, once senses that they fundamentally do not understand the system they attempt to model. What, by the way, do bold values in Tables 1 or 2 indicate? Some kind of statistical certainty of statistical summaries? And what do the axis units in Figure 4 indicate? One gets the strong sense that we have gone substantially backward in precision, accuracy and reliability with this product.

I find a several other conceptual errors (which only reinforce a sense that these authors - despite apparent mathematical skills - have not the faintest idea of the Antarctic environment) and several of language, but why bother? I sincerely regret that I took on the task to serve as reviewer. I can well understand that many others declined to review. Perhaps these authors will receive a different more-favorable review. This reviewer emphatically recommends complete rejection: ESSD will damage its admirable reputation by publishing such nonsense