

Interactive comment on “AntAir: satellite-derived 1 km daily Antarctic air temperatures since 2003” by Hanna Meyer et al.

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Despite a title that implies a product of Antarctic-wide impact, coupled with an expansive “climatology, hydrology or ecology” first sentence, these authors - by their own words - motivate and justify their work solely by its relevance to ice-free areas: “we’re mainly talking of ice-free areas when it comes to terrestrial biodiversity”. Although they claim to respond to reviewer 2 (R2) “point-by-point”, I found nothing responding to the “0.03%” affected land area claim by R2. Do authors have information to refute that number? Can they show quantitatively that the relevance of their work for “biodiversity” extends to larger areas of the Antarctic continent? Can they cite Antarctic or polar or even temperate biological or biogeochemical pathways or processes for which a temperature resolution of 5K would prove sufficient? The single reference they

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cite (Wauchope et al. 2019) deals exclusively with protected areas, shows an areal extent image (Figure 1 in Wauchope) that appears to prove R2’s point about vast areas of frozen Antarctica, and provides emphasis of narrow applicability of this work: “Terrestrial biodiversity in the Antarctic is predominantly restricted to areas that are permanently ice-free—currently estimated at somewhere between 0.2 and 0.5% of the Antarctic continent.” Hogg & Wall (a citation that appears later in their response to R2) represents an overview document for a special issue - Hogg & Wall introduce work of others but do not present their own evidence. These authors appear not to contend R2’s point about most of Convey’s work and sub-Antarctic islands?

Hogg & Wall repeat the point about temperature trends: “roughly 1°C” over some unspecified (50 years?) time period. These authors dodge or avoid the question of relevance of RMSE of 5K. Instead they admit: “Indeed, we have higher errors than in other studies related to air temperature” and “the validation statistics are less impressive than in a lot of studies”. They follow with a list of machine learning citations none of which mention Antarctica and none of which an Antarctic researcher will ever read. Their RMSE vs elevation graphic, included in response to R2, paints an even worse picture. Compared with Figure 1 of Wauchope, these authors show worse RMSE (around 8K) in coastal and perimeter regions where improved air temperatures might prove useful to biodiversity while their lowest RMSE (4 to 5K) occurs in ‘bio-deserts’ high on the ice sheet? (Also, according to their own Figure 1, worse RMSE where they have higher abundance of situ validation data? Does that not give them pause?)

About the use of LandSat LST over snow- or ice-covered regions - a concern raised by both reviewers - they conclude only “This is an ongoing challenge and further research effort on this will certainly improve the presented AntAir dataset in the future.”

This product involves application of four machine learning algorithms - Random Forests, Generalized Boosted Regression Models, Neural Networks, and Partial Least Squares Regression - chosen “because they are either frequently applied algorithms or have shown good performance in the pilot study of Meyer et al. (2016)”. Meyer et

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al looked only at GBM (with a preliminary RMSE of 11K!), so how can they cite that as qualifying distinction? If this does not represent a “casual” (in words of R2) assembly of algorithms, these authors certainly have not given readers or data users sufficient information to understand why these four? Do none of the four algorithms have prior effective published use for air temperatures in polar regions? I found the comment by R2 about ‘visual inspection’ curious but also relevant. Would a different visual inspector have arrived at a different conclusion? How would one certify or replicate ‘visual inspection’ in a validation sense? These authors reference a satellite product that requires “further research” with outcomes that (again, in their words), refer, *sensu stricto*, to “ice-free areas” with “higher errors than in other studies” and “validation statistics are less impressive than in a lot of studies”.

ESSD exists to reward data providers for sharing useful high-quality data with a large community of users. I fail to see how these authors have provided sufficient justification or certification of quality.

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