The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC2
https://doi.org/10.5194/tc-2022-11-RC2, 2022

Comment on tc-2022-11

Stef Lhermitte (Referee)

Referee comment on "Megadunes in Antarctica: migration and characterization from remote and in situ observations" by Giacomo Traversa et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-11-RC2, 2022

SUMMARY
Traversa and colleagues present an analysis of two transects of megadunes in East Antartica based on optical and thermal infrared satellite remote sensing data and they relate the spatial patters in the satellite data with topographic characteristics (e.g. slope, aspect). Additionally they analyse the spatial migration of the megadunes by cross-correlation techniques on the satellite imagery.

MAJOR COMMENTS
Although the paper tackles an interesting research topic (assessing spatial variations in megadunes) with novel results (upward migration and role on SMB), it may eventually warrant publication if some very major comments are addressed. The major comments are mostly related to a complete reorganization of the paper, which would require a significant effort. The major comments are outlined below and identified in detail in the specific comments are made in the uploaded pdf.

- The paper is currently written in a very lengthy and narrative setup following the research path with parts of the data, methods and results diluted throughout the paper. This makes it difficult to quickly read the paper and/or look for specific data set processing, analyses, etc. Reorganizing this into better aligned data, methods and results sections will allow to shorten and focus the paper better highlighting the main message.
- The paper shows some direct overlap with a previous conference proceeding by the same authors (Traversa, G., Fugazza, D., and Frezzotti, M.: Analysis of Megadune Fields in Antarctica, in: 2021 IEEE International Geoscience and Remote Sensing Symposium IGARSS, 5513–5516, https://doi.org/10.1109/IGARSS47720.2021.9554827, 2021a). I would consider removing the overlap (e.g. again showing the NIR profiles) and focusing on the novelty in this paper
- The migration problem remains complex as the Traversa et al show that the windward flanks migrate, while the leeward flanks don't. Consequently, it cannot be a (moving) steady state and after a long time the windward flanks would overlap with the leeward flanks, which would seem rather problematic on the long term. Therefore, the migration

part of the study would benefit from some extension to address/document the importance of this discrepancy in more detail.

- The introduction now contains two separate parts with a general introduction with short summary, focus, aim and is then followed by a second more in-depth introduction about the processes, uncertainties related to megadunes. I would advice to go for general introduction -> in-depth introduction (including scientific problem statement) -> aim -> short summary. This will increase the readability and flow of the introduction.
- The subtle differences in use of different data sets (Landsat, Sentinel-1) and preprocessing (e.g. FCC on different bands for different data sets) makes it complex to follow the flow and setup of the paper as it therefore reads as a patchwork of different things. Consider switching to a more homogeneous or grouped approach that would allow the reader to better understand (Study Area / Data including processing (Landsat, Sentinel, Wind, GPR, Velocity)) / Methods (reflectance + albedo, thermal brightness temperature, SPWD, classification, migration (including comparison with existing velocity) / Results per method subsection / Discussion without new results / Conclusion).
- The paper mentions seven transects for the analysis, but from my understanding many of analysis seem limited to one transect (C in figures 4-6). Consider making the analysis more general and extensive so the results can also be generalised for the other transects.
- Based on the equation of SPWD (Eq.4) the SPWD is only calculated for eight potential neighboring cell (i.e. in steps of 45 degrees). This implies that wind uncertainties of 22.5 degrees (and corresponding height differences in different directions) would not affect the affect the SPWD as the wind can only flow N,NE,E,SE,S,SW,W,NE and nowhere in between. This could have large impact on the SPWD as the 8 directions do not necessarily align with the maximal slope alogn the terrain. I would therefore recommend to recalculate the SPWD along the real wind direction but for interpolated DEM data.
- Given the difference in satellite response for images with SZA < 70 and >70 I would consider only using images with SZA<70. The other seem erroneous and by limiting it to SZA<70 (in the data section) based on know artefacts (e.g. Picard 2016) it would allow the story to focus on the main points.
- The classification of Figure 7 is completely unclear as all methodological details are missing. Additionally I am missing analyses that show that albedo and brightness temperature cannot be used and/or that the method works and is reliable. Just using a method and saying that it works is not how it should be done.
- I would advice to deposit the data corresponding to the paper in a open access repository (with doi) and not rely on requests to the corresponding author.

SPECIFIC COMMENTS related to line numbers, figures etc
See attached pdf with annotations and comments.

Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-11/tc-2022-11-RC2-supplement.pdf