Review of " How flat is flat? Investigating the spatial variability of snow surface temperature and roughness on landfast sea ice using UAVs in McMurdo Sound, Antarctica"

General Comments

This study presents an innovative UAV-based approach to mapping snow surface temperature and topography on Antarctic landfast sea ice. The authors provide detailed documentation of their UAV flights, ground-based measurements, and image processing steps, including a novel algorithm to correct thermal drift due to NUC (Non-Uniformity Correction) events in the thermal camera.

The study is technically sound and methodologically robust, offering high-resolution spatial insights into processes that are often oversimplified in energy balance models. The authors convincingly demonstrate that sediment and irradiance variability, rather than snow depth, dominate surface temperature patterns in their study area.

However, the manuscript in its current form is excessively long and includes some minor technical and formatting inconsistencies. Furthermore, the broader applicability of the results are not enough. With revision, this manuscript has strong potential for publication and contributes valuable observational data and methodology to polar remote sensing and surface energy balance research.

Major comments

1. My first major comments is the primary hypothesis, that snow depth drives spatial variability in snow surface temperature, is tested but ultimately unsupported. For example, the correlation between snow depth and surface temperature is weak (e.g., Spearman's rs = 0.16 for the entire field), which the authors acknowledge. While the discussion then shifts toward the influence of sediment and irradiance, the analysis remains largely qualitative. A more rigorous statistical treatment, such as a multivariate regression analysis that accounts for snow depth, sediment presence (e.g., red-band reflectance), irradiance, and roughness, would be necessary to better disentangle the relative contributions of each factor. Moreover, the identification of sediment patches is based solely on manual inspection of RGB images and red band intensity. This approach is subjective and sensitive to camera auto-settings and changing illumination conditions. As a result, I have serious concerns about the accuracy and reproducibility of this classification. Could the authors develop an objective spectral threshold or a sediment index to quantify contamination levels? Could radiative transfer modeling or image normalization techniques help constrain uncertainty? How transferable is this method to other optical datasets or regions? As it stands, the interpretation of albedo-related heating effects remains qualitative and lacks the quantitative rigor needed to support strong conclusions. This weakens the scientific impact of the paper.
2. While it is perfectly valid to report a null result (i.e., snow depth is not the main driver of surface temperature variability), the manuscript does not provide a compelling quantitative alternative. Sediment and irradiance effects are proposed as alternative drivers, but again, these are explored only descriptively, without any systematic modeling framework. There is no attempt to define a sediment contamination index or

perform a statistical regression that incorporates the full range of possible explanatory variables. As a result, the conclusions remain speculative and unsupported by robust evidence. This lack of quantitative follow-up significantly limits the paper's strength and broader relevance.

3. My second major concern is the very limited spatial and temporal scope of the study, which raises questions about the broader significance and generalizability of the results. The entire analysis is based on a single 200 × 200 m region of landfast sea ice, under relatively flat and low-wind conditions. This kind of super-local case study might be sufficient for a technical demonstration, but how representative is it of more heterogeneous, dynamic and any other Antarctic sea ice environments? Can any of the findings, especially the role of sediment or microtopography in surface temperature variability, be extended to larger spatial scales or different surface types? What about temporal representativeness? The study captures only one moment in time under clear-sky conditions. Without any diurnal or seasonal coverage, I found the conclusions are difficult to generalize. Have the authors considered applying the same methods to a second site, or repeating flights under different light/wind conditions? If not, this should be clearly acknowledged as a limitation.
4. Additionally, the snow depth proxy derived from the UAV DEM rests on the assumption of a flat ice surface and uses a single calibration offset from ground-based measurements. But where is the validation? There is no high-precision GNSS positioning of the MagnaProbe data, and the ±0.05 m uncertainty in snow depth is not propagated into any of the correlation analyses or surface temperature interpretations. How does this uncertainty affect the reliability of the snow temperature correlation results? Are there areas where DEM uncertainty dominates the signal? This aspect of the study is underdeveloped and undermines confidence in one of its core data layers.
5. A further issue is the lack of an overall uncertainty framework. The authors had ±2.1°C for temperature correction (Figure 4), but there is little analysis of how this uncertainty varies spatially (e.g., near image edges, in shaded areas), or how it might interact with surface roughness, sediment detection, or irradiance calculation. What are the sources of uncertainty from lens effects, and how do they affect the final interpretation? Could they lead to systematic biases in warmer sediment zones or sloped surfaces?
6. There is also a surprising absence of any actual surface energy budget analysis. The paper frequently references "energy balance" but never attempts to break it down into its components (e.g., incoming solar, reflected solar, outgoing longwave, conduction). Without this, the proposed mechanisms, e.g., sediment warming or irradiance enhancement on slopes, remain less clearly. Why not use the DEM and albedo proxies to estimate slope-corrected irradiance per pixel, or combine reflectance and temperature into a basic radiative balance estimate?
7. Finally, the overall discussion remains descriptive and lacks synthesis. What is the key physical message here? Are the findings consistent with prior studies in the Antarctic? What is new about the relationship between microtopography, sediment, and surface temperature, and what does it imply for satellite-based retrievals or sea ice models? Without clearer integration of results into a broader scientific context, the paper risks reading more like a high-resolution mapping exercise than a process-oriented scientific study.

Specific comments:

1. Section 2.3.3: The snow depth proxy construction is hard to follow. Are you aligning in situ snow depth data with the DEM by GPS coordinates, or using a

stereophotogrammetric offset? Please clarify the procedure with a schematic or simple diagram. This section needs better structure to explain how the reference elevation is selected and how uncertainty is estimated.

2. Figure 8: (1) The latitude/longitude labels are unconventional and difficult to interpret; (2) The color bar is unintuitive (blue implies deep snow, which is opposite of expectations). Consider reversing the color scale or annotating it more clearly. (3) Does the 0 m value mean bare ice? Please explain this in the caption.
3. Figures 10 and 13. These are not well-integrated into the discussion and don't provide substantial new insight. Consider removing them or clearly stating their purpose.
4. Line 300:"...to/by 0.53 °C?". Please clarify whether this value is a change "by" or an absolute value "of".
5. Line 486: "...area of 200 m.". This is not an area. You probably mean "length of 200 m" or "area of 200 m × 200 m".
6. Line 332 and Line 568: remove one "the"
7. Figure A2 and A3 should be swapped.
8. Figure A4 (b) and (c) add little value, it's redundant with (a). Consider removing or merging.