02 Review

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.
AC: We thank the reviewer for these thoughtful comments. We will attempt to include a multiple linear regression analysis using snow depth, irradiance, and red band values as

predictors of snow surface temperature. This will be carried out for both the targeted clean and sediment datasets, as well as the full test field. While we agree on exploring this path to provide an additional quantification of our results, we are going to explore the feasibility of this approach as the parameters are mostly dependent on each other and cross-correlations might be the limiting factor.

In the targeted dataset, we expect red band values to reflect sediment presence, which would overwhelm the illumination effects if sediment concentration were visible, such as is the case here. In the full field, however, the red band values are likely to capture broader effects such as illumination and shadowing rather than sediment alone. While we could remove the totally shaded regions through the irradiance modelling, the irradiance modelling will not account for the differences in illuminations in our images. Given the limitations of the available imagery, we do not consider the development of a more objective sediment index or the application of image normalization techniques feasible within the scope of this study.

We acknowledge the subjectivity involved in manually delineating sediment patches. However, we are not intending to quantify sediment concentrations, but merely to try and exclude areas with significant and visible sediments, so we can understand what effects temperatures of relatively clean snow. This study is intended as a proof of concept at a single site. Broader evaluation across different sites is planned for future work but lies outside the scope of the current 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 2 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.

AC: We respectfully disagree with the characterization that our analysis lacks a quantitative framework. While some relationships are indeed weak, this reflects the nature of the data rather than a lack of quantitative effort. We disagree that a weak correlation is not a result, because it shows that, for example snow depth, is clearly not the dominant driver of surface snow temperatures. As with our response to the previous reviewer, we will address this point by including a multiple linear regression analysis incorporating snow depth, irradiance, and red band values as predictors of snow surface temperature. This approach will allow us to quantify the relative contributions of these variables beyond the current descriptive treatment. As previously noted, the development of a sediment contamination index lies outside the scope of this study. The revised manuscript will include additional analysis to support and clarify our conclusions.

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.
AC: We acknowledge the limited spatial and temporal scope of this study and will clarify this more explicitly in the manuscript. While this work is a technical proof of concept for the retrieval of high-resolution temperatures, applied to a single test site under clear-sky and, low-wind conditions, we have also clearly and quantitatively shown (using a correlation of over 6000000 data points) in what is considered a relatively large coverage and high-resolution, of local in-situ surface temperatures over snow on sea ice, that microtopography plays a dominant role for local surface temperatures under clear sky conditions. It is also clear that this would not be the case under diffuse conditions, because of the lack of topography dependent irradiance. We will emphasize the last point in the manuscript.
The use of the TIR camera is restricted to clear-sky periods, as clouds would force the surface temperature to reach thermal equilibrium with the cloud base temperature, overruling the impact of snow/ice spatial heterogeneity on the surface temperature map. While we recognize the value of expanding the analysis to additional sites or varying environmental conditions, such extensions are planned for future work and lie beyond the scope of this study.

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.
AC: We agree that the snow depth proxy represents a potential source of uncertainty in our analysis. The approach relies on the assumption of a flat ice surface and a single offset calibration based on 813 ground-based magnaprobe measurement points, which do not

have high-precision GNSS positioning. We will add a discussion on the estimated ±0.05 m uncertainty in the snow depth proxy and its potential impact on the snow temperature correlations. While this level of uncertainty is consistent with previous UAV-based snow depth studies, we recognize that error propagation has not been explicitly addressed and will incorporate this into the revised manuscript to better qualify the reliability of the snow depth-temperature relationships.

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?
AC: We did not clarify this sufficiently in the original manuscript, but we did not use the entire image for our calculations, instead, as explained in the workflow figure 3, "Oval image mask applied to account for the vignette effect (corners of the image are cooler than the centre) in thermal images due to the lens". So, the image edges were actually removed and not included in the dataset. We will rephrase "account for" to "remove the areas affected by" to be precise. We will attempt at a clearer and more comprehensive discussion of uncertainties.

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?
AC: Considering that we have calculated the slope-corrected irradiance and we discuss it in quite a bit of detail (e.g. section 2.3.9, figure 10, section 3.2.3., figure 12, figure 13, figure 14, section 4.4), we find this comment a bit puzzling. Our discussion about the slope corrected irradiance and its correlation with the measured surface temperature seems to be exactly what the reviewer is missing, so we are not quite sure how to address this comment. We do not have albedo proxies, as our RGB measurements are not set up to retrieve albedo. Conducting a comprehensive energy balance breakdown is beyond the scope of this study, which is focused primarily on mapping spatial variability in snow surface temperature and exploring its potential drivers. Additionally, EB modelling would require a lot of sensitivity analysis to input parameters and boundary conditions, to account for the large variation and uncertainties in local EB input parameters and boundary local conditions. We will revise the manuscript to better frame the discussion around temperature variability and avoid overstating conclusions related to the full energy budget.

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.

AC: We appreciate the reviewer's insightful comment and agree that a clearer synthesis is needed. We will revise the discussion to better highlight the key physical insights and explicitly connect our findings to the limited existing literature and data on Antarctic snow on sea ice, which remains scarce. This scarcity is a primary motivation for our study. We will emphasize what is new about the observed relationships between microtopography and surface temperature and discuss their implications for satellite retrievals and sea ice models. The relationship between surface temperature and sediment is relatively well established and gives our results significant confidence because we can clearly observe it. Our aim is, as we have done here, to move beyond descriptive mapping and provide quantitative statistics and process-level understanding relevant to this underexplored region.

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 3 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.
AC: We will clarify the procedure in Section 2.3.3 by clearly describing how the in situ snow depth measurements are aligned with the DEM using GPS coordinates. Additionally, we will explain how the reference elevation is selected, and detail how the associated uncertainties are estimated to improve the section's clarity and structure.

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.
AC: We will revise Figure 8 by correcting the latitude and longitude labels to a more conventional format for easier interpretation. Additionally, we will adjust the color bar to better reflect intuitive. The figure caption will also be updated to clarify that the 0 m value corresponds to bare ice areas.

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.

AC: We will add more explanation for Figure 10 to clarify its relevance, including details on aspect and its role in the analysis. Regarding Figure 13, we are considering removing it as it does not provide substantial new insight and is not well integrated into the discussion.

4. Line 300:"…to/by 0.53 °C?". Please clarify whether this value is a change "by" or an absolute value "of".
AC: We acknowledge this comment, which is similar to one raised by Reviewer 1. We are going to clarify that the RMSE for surface temperature decreased by 0.53 °C, down to 0.58 °C.

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".
AC: We agree and will correct the phrase to "area of 200 m × 200 m" in the revised manuscript.

6. Line 332 and Line 568: remove one "the"
AC: We will remove the redundant "the" in both instances.

7. Figure A2 and A3 should be swapped.
AC: We will swap Figures A2 and A3 as suggested.

8. Figure A4 (b) and (c) add little value, it's redundant with (a). Consider removing or merging.
AC: We are going to improve this Figure.