03 Review

General

This article examines the spatial variations in snow surface temperature at the plot scale (hundreds of meters) on sea ice, aiming to identify the primary drivers. Snow temperature is highly sensitive to even minor changes in incoming fluxes (such as shortwave and longwave radiation) and serves as a means to explore the surface energy budget (SEB) and its unique characteristics on snow. To address this question effectively, the authors collected a unique and exceptional dataset in the Antarctic. The paper holds significant interest for the TC community but has some shortcomings, one of which is notable yet addressable.

The main issue lies in how the paper's question and objective are approached. The hypothesis proposed by the authors to justify and structure the study is unconvincing from my perspective, and they ultimately demonstrate that this hypothesis is indeed not validated. Instead, they identify one/two more adequate hypotheses, which, to my knowledge, are more obvious. While it is appreciable to build a paper around a clearly stated hypothesis, the fact that this hypothesis is not commonly supported in the literature and can be dismissed with magnitude order calculations undermines the paper's construction. What if the hypothesis had been, "Does local topography explain most of the surface temperature variations at the plot scale?" Research on the link between topography and SEB is prevalent in the literature, and the current title aligns more closely with this hypothesis. With the same dataset and results, the introduction and discussion would flow better, aligning more effectively with the results and conclusion.

Apart from this shortcoming, the paper is excellent and has many strengths, including significant work on TIR camera correction and an exceptional dataset. The paper is clear and well-written, with an easy-to-follow logic (except the order of the figures) and a generally sufficient level of detail (see the exception on the model below). However, the abstract is difficult to understand without having read the paper, as the logical progression is unclear (unlike in the paper itself). To attract more readers, I suggest rewriting the abstract from scratch, incorporating more results and fewer hypotheses.

The paper is quite long and my recommendation for the review is to shorten where possible, but avoid lengthening (except for the model description!).
AC: We appreciate the reviewer's thoughtful comments and fully agree with the concerns raised regarding the framing of our hypothesis. We will revise the manuscript to shift away from the narrow focus on snow depth as the primary driver of snow surface temperature

variability. Instead, we will adopt a more comprehensive and better-supported hypothesis, aligned with our findings: *"At the plot scale on Antarctic sea ice, spatial variability in snow surface temperature is primarily driven by local surface conditions, including microtopography, sediment presence, and variations in irradiance, rather than snow depth alone."*
This re-framing will be reflected in a revised introduction, clearer discussion, and an updated abstract that emphasizes our key findings rather than the initial hypothesis. We agree that this restructuring will improve the logical flow and impact of the paper.
We also acknowledge the comment regarding manuscript length and will make targeted reductions to enhance clarity without compromising content. Additionally, we will expand the model description section to ensure sufficient detail is provided for reproducibility and transparency.

Detailed comments:

L 6 "Our airborne maps reveal a mean snow depth of 0.16 ± 0.06 m". The mention of snow depth measurements is new in this sentence. The previous sentence is about surface temperature and the end of this sentence is about surface temperature. A reorganisation is necessary.
AC: We agree and will revise the sentence to improve clarity and logical flow by separating the introduction of snow depth measurements from the discussion of surface temperature. This reorganization will help ensure that each concept is introduced in a more coherent and contextual manner.

L11 "seemingly flat snow field" can you give a number, this statement is relative to reader's expectation of what flat is.
AC: We agree and will quantify the flatness of the snow field by including the observed range in snow depth, as well as provide a description of the overall roughness features (absence of pressure ridges and rafted ice) to clarify what we mean by "seemingly flat" and make this statement more objective.

L13-14. It is not clear what the variability accounted in the uniform irradiance model.

What is meant exactly by "the incoming solar radiation (irradiance) at the point scale". In principle incoming solar radiation is measured w/r to a horizontal surface. Do you mean the solar radiation received / perceived by the surface ?
AC: We thank the reviewer for pointing this out. To clarify, our irradiance model computes the solar radiation actually received by each inclined sea-ice surface element, accounting for local slope, aspect, solar geometry, and terrain shading—rather than using irradiance

values referenced to a horizontal surface. It combines measured global and diffuse radiation during the flight with the DEM-derived surface geometry using established radiative transfer approaches. We will revise the manuscript to better explain this and eliminate ambiguous phrasing such as "incoming solar radiation at the point scale."

L17 "While we initially hypothesized that snow depth was a key driver of snow surface temperature," this hypothesis should be stated in the first part of the abstract (L5) to position the problem addressed in the paper, and it should be justified, backed by literature because this hypothesis is not intuitive, at least to me. From the SEB equation, I'd expect surface temperature to depend on irradiance first, the snow depth does not appear in this equation unless the conductive term is written with the Fourier law. However even in this case, the snow is far too insulating to allow this term to become significant with respect to the others in summer, especially the incoming irradiance.
AC: Our original hypothesis was motivated by the fact that, because of the strong insulative and scattering properties of snow, in case of shallow snow covers small changes in snow depth have a strong impact on the heat conduction and surface albedo. How much this affects the surface temperature will, however, very much depend on ice thickness, air temperature, and range of albedo variability: surface temperature is more sensitive to snow depth for thinner ice, colder air temperature, and large albedo contrast between bare ice and snow. In this case study, the very thick sea-ice (2.4 m) strongly insulated the snow layer from the ocean heat flux and also caused a relatively large bare ice albedo. Both these effects minimized the sensitivity of surface temperature to snow depth. We will revise the abstract and introduction to frame our initial hypothesis more broadly - acknowledging that while we considered snow depth as one possible contributor to spatial variability in surface temperature, we also evaluated other controls, particularly irradiance and topography. We will support this revised framing with references to relevant literature on energy balance partitioning over snow-covered sea ice. However, we would like to point out than when solving the equations for the SEB, snow depth a) plays a role in the heat conduction, and b) it influences the lower boundary condition, which is the snow-sea ice interface, so snow depth does certainly play a role for the surface energy balance. This may not be relevant for thicker snowpacks, but for the relatively thin snowpacks that are observed in regions of Antarctica and the Arctic, the snow depth (and the snowpack's physical properties) has a dominant role in the heat exchange in the air-ice-ocean system.

L34-35: "through the satellite period" "since record-keeping began". Should indicate the starting year to avoid ambiguity.
AC: Agreed. We will specify the starting year for both the satellite observational period and historical record-keeping to ensure clarity and remove ambiguity for the reader.

L47. While the authors are free to make the hypothesis they want as long as it is clearly stated – and I acknowledge it is very well done here w/r to the literature in general -- still I found this hypothesis strange. The physical reasoning behind this hypothesis should be developed a bit and examples from the literature could help.
AC: We appreciate the reviewer's feedback. While we initially presented the hypothesis focused on snow depth, we agree that incorporating surface topography and irradiance provides a more physically grounded framework. We will revise the hypothesis accordingly and include relevant literature examples to better support the physical reasoning.

L90. The objective would benefit to be rewritten without this simple hypothesis. A more neutral approach would be to list all the potential factors influencing the small scale variability of snow surface temperature with a short literature review for each, and reframe the goal into investigating/quantifying which term is the key driver in the specific context of this study (summer, sea-ice with small snow depth).
AC: We appreciate the reviewer's insightful suggestion. We will revise the objective to present a more neutral approach by outlining the potential factors influencing small-scale variability in snow surface temperature, supported by a brief literature review. The objective will then focus on investigating and quantifying the dominant drivers within the specific context of summer sea ice with shallow snow depths.

L116: "The other four sites" I'd remove this sentence, it diverts from the objective of the paper.
AC: We agree that this sentence somewhat diverts from the main objective of the paper. However, we will retain a brief mention to provide context on how this study fits into our broader ongoing research, which will extend these findings to additional sites.

L136. Please check the correspondence between he height of installation vs the footprint areas (1.6m2 versus 0.35m2). Is the angle of installation at the sediment site different ?
AC: We confirm that the angle of installation at the sediment site differs from the other sites, which results in a different footprint area. We will clarify this point in the manuscript for better understanding.

L214: Check that the figures are referred in order (it seems Fig 8 is referred before others).
AC: We agree and will ensure that all figures are referred to in the correct order throughout the manuscript.

L215 L223: Check figure A1 reference

AC: We agree and will verify and correct the references to Figure A1 to ensure they are accurate and properly placed.

Figure 2. Can you show a scatterplot (+ r2 and RMSE) between the inferred snow depth proxy and the magnaprobe measurements as a validation of the approach ?
AC: AC: Thank you for the suggestion. We initially considered a direct point-by-point comparison between the inferred snow depth proxy and the magnaprobe measurements, but due to the known horizontal uncertainty in the magnaprobe's GPS (up to several meters), we found that such a comparison would be misleading, especially given the high spatial variability of snow depth at our site. A small spatial offset can result in large apparent discrepancies. For this reason, we chose a histogram-based comparison, which provides a more robust and meaningful assessment of the proxy's ability to capture the distribution of snow depth.
To further support this, we will include an illustrative example showing the potential variation in measured snow depth within a radius that corresponds to the GPS uncertainty (e.g., 3–5 m), highlighting how much depth variability can exist within a single GPS error footprint. We believe this better reflects the true agreement between the datasets than a scatterplot would.

L272: It is not obvious how 0.5 °C was found based on Fig 4a. I guess it is empirical but how sensitive to conditions is it?
AC: We agree that this was not sufficiently explained. The 0.5 °C threshold was determined empirically based on the stability of the NUC correction curve observed in Fig. 4a. We will clarify this in the text and add a brief comment on the sensitivity of this threshold to varying environmental conditions.

L318: by curiosity, how large is this correction ?
AC: We interpret the reviewer's question as referring to the magnitude of the correction between the Apogee brightness temperature and the derived surface temperature in Eq. 4. For the Apogee pointed at the sedimented snow surface, the mean correction is 0.55 °C.

L321: I don't understand what this RMSE is, between what and what it is calculated (+ typo: with an RMSE => with a RMSE)
AC: Thank you for the comment. This RMSE refers to the temperature anomaly RMSE calculated after applying the NUC (non-uniformity correction). It quantifies the residual error in the temperature dataset and is used as the uncertainty estimate for the surface temperature maps. We will clarify this in the manuscript and correct the typo.

L329: " The RMSE of the residuals of this linear fit". RMSE → RMS or remove residuals
AC: Going to change the phrasing.

L330. "the square of the thermal" and "the square of the RMSE associated " check if square is correct in both cases. I'd recommend to completely rephrase or write as an equation.

AC: We thank the reviewer for pointing this out. We agree that the phrase “RMSE of the residuals” is redundant, as RMSE is inherently calculated from the residuals. We will revise the sentence to read “The RMSE of this linear fit” for clarity.

Section 2.3.8. It is not clear how the impurities are detected. Using a threshold or just visually ? If just visually, this section could be removed, and a line or two in the results section is sufficient.
AC: We agree with the reviewer. Since impurity detection was done visually, we will remove Section 2.3.8 and instead include a concise description of this step in the results section where the findings are discussed.

Section 2.3.9. Given the critical role of the model, the level of detail should match that given for the drone. It is necessary to provide the main aspect of the model (e.g. workflow) and present the equations or refer to the equations in the cited papers for each main calculation step.
Main questions are: the diffuse component, the resolution of the calculation (in relation with the positioning uncertainties), cast shadows, multiple scattering (esp for the shadows).
AC: We agree that additional detail on the irradiance model is warranted. We will revise Section 2.3.9 to include a clearer description of the model workflow and specify the treatment of the diffuse component, calculation resolution, shadows, and the handling (or limitations) regarding multiple scattering (there is no multiple scattering, and the diffuse component is just the measurement recorded by the SPN1 sensor), particularly in shadowed areas (which are a very small percentage). We will also add relevant equations or explicitly refer to the corresponding formulations in the cited literature.

L360. How does 2.4 ± 0.04 m translate into 1% variation and where 0.04m is coming from ? The histogram seems to indicate larger deviation. How relative variation is defined and calculated ?
AC: We thank the reviewer for the comment. The value of ±0.04 m refers to the standard deviation of all 2449 sea ice thickness measurements and was not used for calculating the relative variation. The 1% variation was derived from the mean absolute deviation (MAD) of the thickness values, expressed as a percentage of the mean thickness (2.4 m). We will revise the text to clearly distinguish between standard deviation and MAD and report the correct value used for the relative variation.

L395. Fig 12 is referred, check the order.
AC: Thank you for pointing this out. We will ensure that Figure 12 is referred to in the correct order within the manuscript.

L413. Isn’t it due to the correlation between snow depth and impurities ?
AC: We thank the reviewer for the suggestion. However, we do not think the stronger correlation is due to a direct relationship between sediment and snow depth. The “sediment” dataset includes outlines drawn around sediment patches, but these masks

also include surrounding clean ice pixels, introducing greater variability in both snow depth and temperature. In contrast, the “no sediment” dataset is based on rectangular boxes placed in uniform snow areas, resulting in more homogeneous conditions and a narrower temperature range. Therefore, we attribute the stronger correlation in the “sediment” dataset to this broader variability rather than a direct link between sediment cover and snow depth.

L469. Why is this relevant in this section ? It is well known that the irradiance depends on the local incidence angle, and not on the slope.
AC: Thank you for pointing this out. We agree that irradiance strictly depends on the local incidence angle, not slope alone. However, in this section, we refer to slope as a proxy for estimating how the local surface orientation influences the irradiance received - i.e.,” tilted irradiance”. We acknowledge that the term “irradiance” may have been used imprecisely here, and we will clarify the language to reflect that we are referring to slope-driven variations in local incidence angle, which in turn affect the effective irradiance received by the surface.

L476: The aspect distribution is uniform, not gaussian. The slope distribution is not Gaussian.
AC: We agree and will correct the statement.

Figure 12. For convenience, adding titles on the rows directly in the graph would help quickly read the figure, without having to read the caption.
The x-axis scale is very large, for just a few outliers. I suggest to reduce the range to -17°C - -10°C or so. It would make the graphs H and I more convincing for instance.
AC: We will add titles to each row directly within the figure to improve readability. We also agree with the suggestion to limit the x-axis range to reduce the influence of outliers and better highlight the distribution patterns, particularly in panels h) and i).

L491: I’m not sure but I think that the algorithm not only correct for NUC jumps but also for other trends in the camera which are usually very large.

Note that other cameras do a “better” job in NUC smoothing which makes jumps more difficult to detect… while still be necessary to applied the necessary corrections. See for instance: Arioli, S., Picard, G., Arnaud, L., Gascoin, S., Alonso-González, E., Poizat, M., and Irvine, M.: Time series of alpine snow surface radiative temperature maps from high precision thermal infrared imaging, Earth Syst. Sci. Data, 16, 3913–3934, doi: 10.5194/essd-16-3913-2024, 2024

Ideally, one would access the raw data… but it seems that camera manufacturer prefer to overprotect their (insufficient) algorithms.
AC: Thank you for pointing this out and for the great reference. We agree that our correction algorithm is relatively simple and may not capture all forms of sensor drift or be

directly transferable to all camera models, especially those with more complex or opaque NUC behavior. Still, we see it as a useful baseline that demonstrates the value of transparent correction methods and offers a starting point for adaptation in other environments and systems. Importantly, this correction was necessary to make the thermal data usable at all - without it, none of the images could have been processed reliably.

L500. I'd advocate for m ore accurate sensors than Apogee sensors when absolute value is important (e.g. close to 0°C, see Arioli et al. 2024)
AC: We agree with this statement. A colleague of ours has developed a really nice method of large metal plates (painted with a black paint of known emissivity) on top of an insulative foam and a thermocouple that logs the temperature of the plate. We plan to use that in the future.

L539. "While the red band values do not directly affect the surface energy balance, we use them as a proxy for impurities." I don't understand this statement. Maybe the verb "affect" is incorrect.
AC: We agree the original phrasing was unclear. We will revise the sentence to clarify that while the red band values alone cannot be used to infer surface impurities for surface energy balance calculations (and albedo), in our case, they serve as a proxy for surface impurities (and potential energy absorption by the impurities). But we acknowledge that the red band values are influenced by illumination as well by impurities, as discussed throughout the manuscript.

L550. I would suggest to coarsen the resolution a bit to account for the positioning uncertainty and to see how this correlation increases. Mathematically, the correlation always increases with smoothing, but here the idea is to see how quick it increases.
AC: We acknowledge this is an interesting suggestion. However, performing the smoothing analysis would require substantial computational time and resources, which we cannot accommodate within the current revision timeline. We will consider this for future work.

L567: the the => the
AC: We will correct this typo.

L570: while this result is sound, the statistical demonstration would require first to demonstrate that topography and impurities are independent in your case. For instance if the impurities areas had more north looking slopes, the relationship is biased. It is frequent (in the mountains) that the sun facing slopes are more likely to have dust emerging at the surface than the colder faces.
AC: We appreciate the comment. However, in our sea ice setting, sediment is likely wind-deposited rather than emerging from the snowpack due to melt, as is common in mountainous terrain. That said, we do observe some correlation between sediment presence and topography, with sediment-covered areas tending to be higher and in areas

of thicker snowcover. We will clarify this relationship in the manuscript.

L580 I suggest to also mention multiple scattering which is likely important in the cast shadows areas and cavity effects in the LW which is probably negligible with slopes <10°. Ref: A. Robledano, G. Picard, L. Arnaud, F. Larue, I. Ollivier, Modelling surface temperature and radiation budget of snow-covered complex terrains, The Cryosphere, 16, 559–579, doi:10.5194/tc-16-559-2022, 2022
AC: We will mention the potential role of multiple scattering in cast shadow areas but note that such shadowed regions constitute a very small fraction of the study area (less than 1%), limiting the overall impact. We will also acknowledge that cavity effects in longwave radiation are likely negligible given the generally low slopes (<10°) observed in our site. Additionally, the small-scale multiple scattering also affects the albedo, which we have already discussed in the manuscript.

L608: This is not necessarily a drawback. If only the irradiance is changing (not Tair, not wind), observing two different Ts give a lot of information on the balance between SW and the other terms of the SEB.
AC: We thank the reviewer for this insightful comment. The two flights were necessary to acquire the RGB data for constructing the DEM, and we combined the thermal infrared data from both flights to generate a single, averaged orthomosaic for this study. For future work, we are considering analyzing the flights separately or using just one flight to build the orthomosaic, which could allow investigation of surface temperature changes related to varying irradiance.

L630: “offering valuable tools for many users”. Same comment as before. To my experience, using more expensive cameras with better NUC correction make the proposed solution not applicable and the problem more severe… A recommendation could be to buy or develop open-source cameras or at least cameras that have been evaluated by others and for which correction algorithms exist.
This is a very good point, and we appreciate the reviewer pointing out that better cameras may make this problem even worse. So, we are pleased to provide a solution to this relatively “inexpensive” off-the-shelf DJI setup, which has proven to reliably work in polar regions. The heated DJI batteries used with our camera enable sufficient flight durations to cover our test fields (200 × 200 m), which remains challenging with non-heated alternatives. Many custom or alternative cameras face limitations such as insufficient battery life under the low temperatures typical of Antarctic conditions, but it’s a good idea for the community to keep looking for open-source alternatives. We will add a note discussing these practical considerations in the revised manuscript.