Author response to anonymous referee #1 (RC1)

Dear Reviewer,

We sincerely thank you for your careful and constructive review of our manuscript. We greatly appreciate the time and effort you have devoted to evaluating our work and providing valuable comments and suggestions. We have carefully considered and addressed all the issues raised in your review, both in our responses below and in the revised manuscript. In addition, we have taken this opportunity to correct further typographical errors and improve the wording and clarity of the manuscript.

Our detailed responses to each of your comments are provided below.

Best regards,
Corresponding author: Guangyin Hu
(on behalf of all co-authors)

Overall comment

The topic is interesting and the spatial patterns shown in the paper are worth investigating. My main concern is that the current analysis shows a strong spatial relationship between sediment distribution and LST, but this is sometimes presented as proof that sediments are causing the temperature differences. I think the paper will be much stronger if the authors explain the Landsat-MODIS matching clearly, provide more information about the satellite processing and regression models, address the uncertainty in the data, and make the conclusions more careful.

Major Comments

Comment (1): I am concerned about how the Landsat and MODIS images were matched. Some of the images are several days apart. For example, the Landsat image from December 26, 2017 is matched with MODIS LST from December 20, 2017. There is a six-day difference. Since the paper shows that sediments can move across the lake surface with wind, how can we be sure that the sediment distribution six days later represents the conditions when the MODIS temperature was measured? Weather conditions could also change during those days. The authors should show the time difference for every Landsat-MODIS pair and explain why these differences are acceptable. If possible, the main analysis should use same-day observations.

Reply: Thank you for raising this concern. We agree that temporal offsets may lead to uncertainty through sediment redistribution and changing meteorological conditions.

We have clarified the temporal matching procedure in Section 3.1 and added the temporal difference for each pair to Table 1. Landsat images were screened independently, and same-day MOD11A1 observations were preferentially selected. When no valid same-day observation was available, the temporally nearest observation satisfying the quality-control requirements was used. Five pairs differed by 1 day, one by 3

days, and one by 6 days.

For the 3-day and 6-day pairs, we compared MOD09GA images acquired on the MOD11A1 dates with the corresponding Landsat images (Fig. R1). The major sediment-covered areas showed broadly similar extents and locations, providing qualitative support for their spatial comparability. However, these comparisons cannot exclude finer-scale sediment redistribution or changes in meteorological conditions. We have therefore explicitly acknowledged these limitations in the manuscript and interpret the results from these pairs with caution.

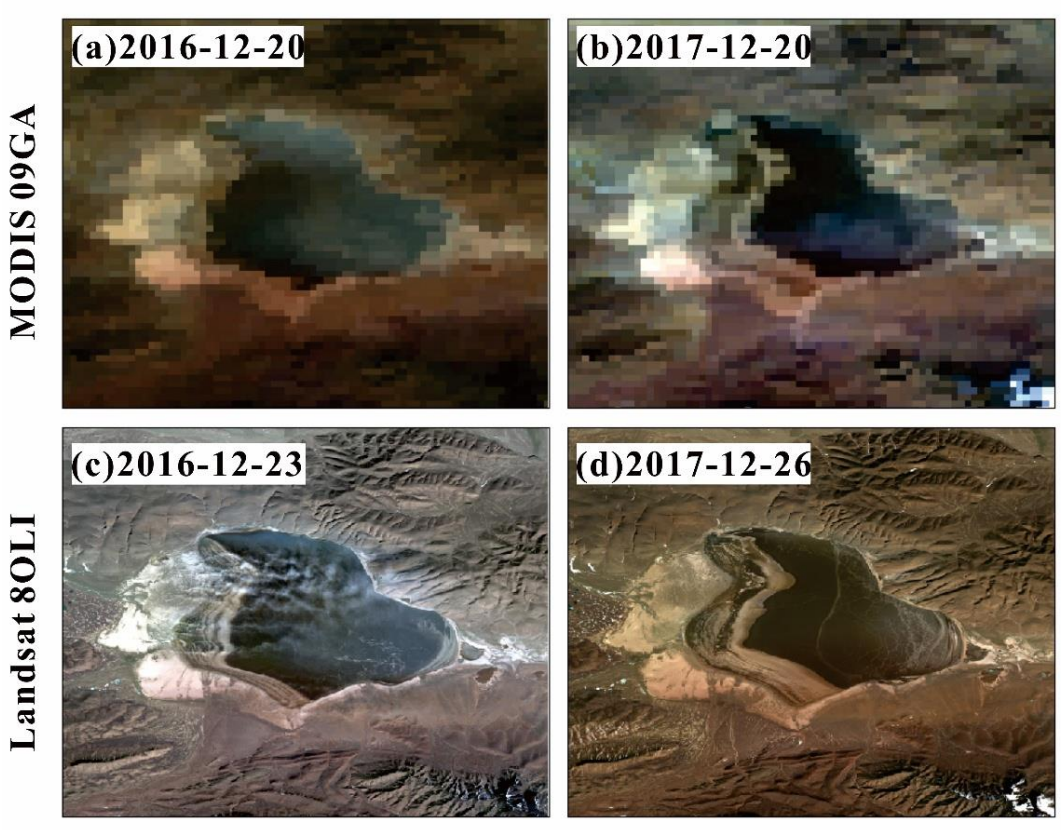

Figure R1. Comparison of sediment distribution between MOD09GA and Landsat 8 OLI imagery for image pairs with temporal offsets of 3 and 6 days.

Comment (2): This part needs more explanation. Landsat NDSLI is at 30 m while MODIS LST is at 1 km. The paper says that MODIS LST was resampled to "higher resolution (1 km × 1 km)," but MOD11A1 is already 1km. How exactly were the two datasets combined? Was the 30 m NDSLI aggregated to 1 km? If so, was the mean NDSLI used, the percentage of sediment-covered pixels, or something else? If MODIS was resampled to 30 m, this would not create new temperature and could result in many Landsat pixels sharing the same MODIS temperature. The authors need to clearly explain the complete spatial matching and resampling process.

Reply: Thank you for pointing out that the spatial matching procedure was not sufficiently clear. We have corrected the phrase "resampled to a higher resolution" and revised the Methods section to describe the process more explicitly.

The MOD11A1 data were used as the spatial reference at a nominal resolution of approximately 1 km. A

corresponding fishnet grid was generated, and the centroid of each grid cell was used to extract the NDSLI value from the 30-m Landsat raster. This centroid-based NDSLI value was then matched with the MOD11A1 temperature value for the same grid cell and used in the subsequent analyses.

We have also clarified that this centroid-sampling approach may not fully represent fine-scale sediment variability within each MODIS-scale grid cell, and this spatial-scale difference is considered when interpreting the results.

Comment (3): The paper says MOD11A1 V6 was used, but important processing information is missing. Was daytime or nighttime LST used? What MODIS quality flags were applied? How were cloudy pixels removed? How were mixed shoreline pixels handled? What was the acquisition time? This is especially important because some of the reported temperature differences are less than 1 °C. The authors should also discuss the uncertainty of MODIS LST over snow- and ice-covered surfaces.

Reply: Thank you for pointing out the need for a more complete description of the MOD11A1 processing and associated uncertainties. We agree that these details are important, particularly because some of the observed temperature differences are relatively small.

In the revised manuscript, we have clarified that the daytime band (LST_Day_1km) of MODIS/Terra MOD11A1 V6 was used. Quality control was performed using the QC_Day band, and only pixels with mandatory QA bits of 00 in bits 0–1, indicating LST produced with good quality, were retained. Cloud-affected observations were screened through the MOD11A1 product-level cloud screening and quality-control information; no additional independent cloud mask was applied. Terra's nominal descending equatorial crossing time during the study period was approximately 10:30 local solar time. Pixel-specific observation times are recorded in the MOD11A1 Day_view_time band.

To reduce potential shoreline contamination, the lake boundary was buffered inward by 1 km before extracting temperature observations. This reduces the influence of adjacent land surfaces, although mixed-pixel effects cannot be completely eliminated.

We have also added a discussion of the uncertainty of MOD11A1 temperature retrievals over snow- and ice-covered surfaces. In particular, uncertainties associated with surface emissivity, residual cloud contamination, viewing conditions, and sub-pixel surface heterogeneity may affect the retrieved temperature. Therefore, relatively small temperature differences, especially those approaching the uncertainty of the satellite retrieval, are interpreted cautiously in the revised manuscript.

Comment (4): The authors use NDSLI < 0 to identify sediments and NDSLI < -0.15 for stronger sediment accumulation. However, frozen lake surfaces can have different spectral conditions because of snow, bare ice, cracks, wet ice, snow depth, and surface roughness. How can we be sure that negative NDSLI values are caused mainly by sediments? The visual comparison in Figure 2 is useful, but I do not think it is enough to say that the sediment boundaries were "accurately extracted." The authors should provide additional validation or test how the results change when different NDSLI thresholds are used or soften the language.

Reply: We sincerely thank the reviewer for this important comment. We agree that the spectral characteristics of frozen lake surfaces can be influenced by various factors, including snow cover, bare ice, cracks, wet ice, snow depth, and surface roughness. Therefore, negative NDSLI values alone may not unequivocally indicate the presence of aeolian sediments under all surface conditions, and the visual

comparison in Figure 2 alone is insufficient to fully establish the accuracy of the extracted sediment boundaries.

The results show that NDSLI values along the B–D section were predominantly negative, corresponding to areas identified as sediment-covered through visual interpretation of the original satellite imagery (Fig. R2). Within this section, the C–D segment exhibited more negative NDSLI values, generally below −0.15, consistent with more pronounced sediment coverage. In our classification scheme, NDSLI < 0 was used to identify areas with pronounced sediment accumulation, while values below −0.15 indicated greater relative accumulation within this range. Positive NDSLI values were further grouped into intervals of 0.2, with increasing values interpreted as progressively lower relative sediment accumulation. These empirical divisions describe relative accumulation patterns rather than an absolute distinction between sediment-covered and sediment-free surfaces.

We acknowledge that this transect-based analysis provides supporting evidence for threshold selection rather than independent validation of sediment boundaries, and that uncertainties associated with different ice and snow surface conditions remain. Accordingly, we will revise the relevant statements in the main text, replacing overly definitive expressions such as “accurately extracted” with more cautious wording and explicitly acknowledging the limitations of NDSLI-based sediment identification. These revisions will clarify the basis of the threshold selection and provide a more balanced interpretation of the results.

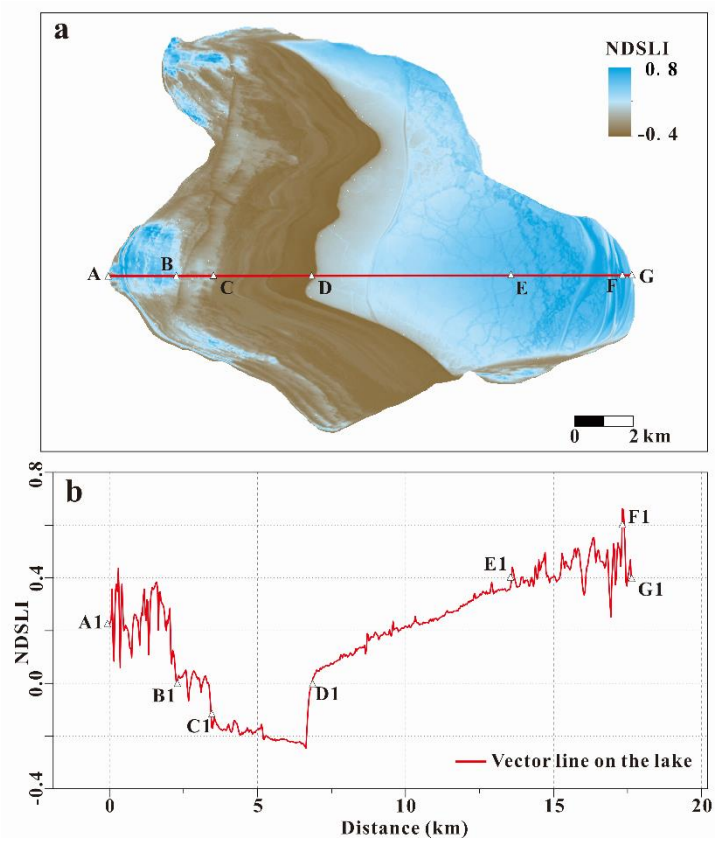

Figure R2. Spatial distribution and transect-based analysis of NDSLI over Zonag Lake on January 11, 2018. (a) Spatial distribution of NDSLI, showing the west-to-east transect A–G across the lake surface. (b) Variation in NDSLI values along the transect, with A1–G1 corresponding to the locations A–G in panel (a). Negative NDSLI values along the B1–D1 section corresponds to visually identified sediment-covered areas,

while the more negative values along C1–D1 indicate relatively pronounced sediment coverage. NDSLI values generally increase eastward into areas of relatively clean lake ice. The transect illustrates the empirical grading scheme, in which NDSLI < 0 denotes pronounced sediment accumulation and NDSLI < −0.15 denotes greater relative accumulation within this range.

Comment (5): The authors report a 6.28 °C difference by comparing the maximum temperature in a sediment-covered area with the minimum temperature in a sediment-free area. I do not think this is the best way to estimate the sediment effect because these are two extreme values from different locations. The mean or median temperature difference between sediment and non-sediment areas is more useful. Confidence intervals or another measure of uncertainty would also help.

Reply: Thank you for this valuable comment. We agree that the previously reported 6.28 °C difference, derived from the maximum LST in a sediment-covered area and the minimum LST in a sediment-free area, represents a localized extreme spatial contrast rather than a representative temperature difference between the two surface types.

We would like to clarify that the manuscript already includes a partitioned statistical analysis comparing the mean LST between the NDSLI < 0 and NDSLI > 0 groups across the seven observation dates, together with KDE and boxplot analyses of the LST distributions under different NDSLI conditions (Fig. 7). These analyses provide a more representative characterization of the thermal contrasts between the two NDSLI groups. The boxplots additionally illustrate within-group temperature variability, rather than confidence intervals for the between-group mean differences.

In the revised manuscript, we have clarified that the 6.28 °C value is used only to illustrate a localized extreme temperature contrast and should not be interpreted as the typical thermal difference associated with aeolian sediment accumulation. The corresponding text has been revised to distinguish this extreme contrast from the mean and distributional statistics presented in Figure 7.

Comment (6): Figure 7 shows that LST decreases as NDSLI increases, meaning that the relationship between NDSLI and LST is negative. Lower NDSLI means more sediment, so the relationship between sediment amount and LST is positive. These are not the same statement. The paper sometimes describes the relationship simply as a positive relationship. The authors should make this distinction clear throughout the paper.

Reply: Thank you for pointing out this important distinction. We agree that the relationship between NDSLI and LST should be clearly distinguished from that between aeolian sediment accumulation and LST. Because lower NDSLI values indicate more pronounced aeolian sediment accumulation, a negative relationship between NDSLI and LST corresponds to a positive association between aeolian sediment accumulation and LST.

We have revised the relevant statements throughout the manuscript accordingly. “Negative relationship between NDSLI and LST” is now used when referring directly to the remote-sensing index, whereas “positive association between aeolian sediment accumulation and LST” is used when interpreting the relationship in terms of sediment accumulation. The related descriptions in the Results, Discussion, Conclusions, and figure captions have also been checked for consistency.

Comment (7): The paper mainly presents R^2, adjusted R^2, AICc, bandwidth, and sigma. However, the actual NDSLI

regression coefficients are not shown. If the purpose of the model is to understand how NDSLI relates to LST, the coefficient is very important. Please report the coefficient values and their uncertainty or significance. For GWR and MGWR, maps showing the local coefficients would also be useful.

Reply: Thank you for this important suggestion. We agree that model-fit statistics alone do not fully describe the direction and magnitude of the relationship between NDSLI and LST.

We have supplemented Table 2 with the NDSLI regression coefficients and their corresponding standard errors for all seven observation dates. We have also clarified that the reported p-values correspond to the overall F-tests.

For GWR and MGWR, we will add maps of the local NDSLI regression coefficients in the Supplement (Fig. R3, Fig. R4). These additions show the spatial variation in the direction and magnitude of the NDSLI–LST relationship and provide information on the significance of the local estimates. The corresponding explanations and figure captions have been updated. We have also clarified that Local R^2 describes local model fit, whereas the local coefficients characterize the spatially varying relationship between NDSLI and LST.

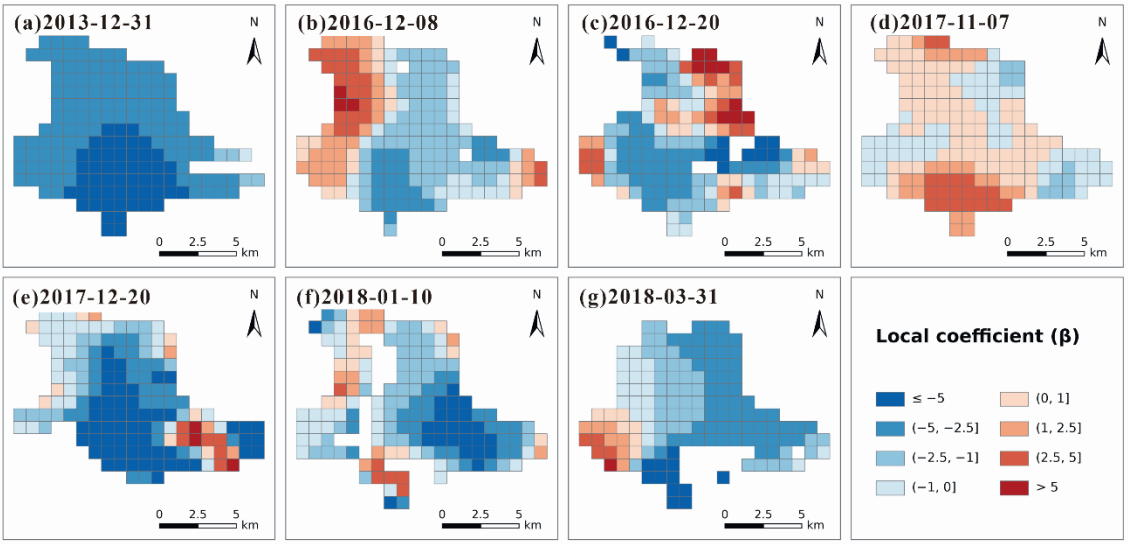

Figure R3. Spatial distribution of local GWR coefficients across seven observation dates.

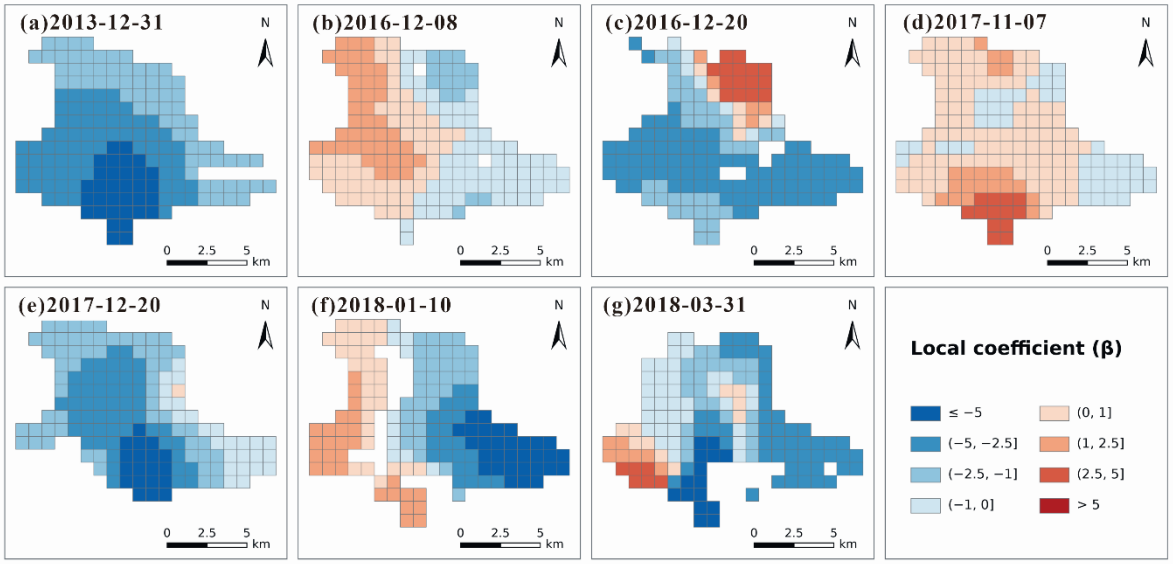

Figure R4. Spatial distribution of local MGWR coefficients across seven observation dates.

Comment (8): The paper explains that MGWR is useful because different explanatory variables can operate at different spatial scales. However, the model appears to have only one main predictor, NDSLI. The authors should explain more clearly what MGWR adds in this particular case compared with GWR. The paper should also provide the kernel type, whether the bandwidth is fixed or adaptive, how bandwidth was selected, the distance method, and other important model settings.

Reply: Thank you for this important comment. Although NDSLI is the sole predictor, the model includes both a spatially varying intercept and an NDSLI coefficient. Unlike GWR, which uses a common bandwidth for these parameters, MGWR estimates their bandwidths separately. The selected bandwidths differed in three of the seven image pairs and were equal in the remaining four. For the 2013 pair, they were 44 and 62 neighbors for the intercept and NDSLI coefficient, respectively.

MGWR achieved higher adjusted R^2 and lower AICc than GWR on six observation dates, while GWR performed slightly better on March 31, 2018. We therefore describe MGWR as a useful complement to GWR.

The comparative results also support the application of MGWR in this study. MGWR achieved higher adjusted R^2 values and lower AICc values than GWR on six of the seven observation dates, although GWR performed slightly better on March 31, 2018. Therefore, we consider MGWR a useful complement to GWR rather than assuming its superiority in all cases.

To address the reviewer's concerns, we propose to clarify the methodological rationale and report the model settings in Section 3.2, including the Gaussian model specification, adaptive bisquare kernel, AICc-based bandwidth selection, distance calculation method, and variable standardization. We will also clarify the interpretation of the MGWR bandwidths and their relevance to the spatial scales of the intercept and NDSLI coefficient.

Comment (9): The manuscript repeatedly says that MGWR consistently performs better than GWR. However, this is not true for March 31, 2018. GWR has an adjusted R^2 of 0.442 and AICc of 355.202, while MGWR has an adjusted R^2 of 0.438 and AICc of 357.377. In this case, GWR performs slightly better. The authors should change "consistently outperformed" to something like "generally outperformed."

Reply: Thank you for pointing out this inconsistency. We agree that MGWR did not consistently outperform GWR across all observation dates. Specifically, on March 31, 2018, GWR achieved a slightly higher adjusted R^2 (0.442 vs. 0.438) and a lower AICc (355.202 vs. 357.377) than MGWR. We will revise the relevant statements throughout the manuscript, replacing "consistently outperformed" with more accurate wording, such as "generally outperformed".

Comment (10): The manuscript repeatedly says that MGWR consistently performs better than GWR but that is not the case. Please change the language.

Reply: Thank you for this comment. As explained in our response to Comment (9), we have replaced "consistently outperformed" with "generally outperformed" throughout the manuscript and explicitly noted the exception on March 31, 2018, when GWR performed slightly better than MGWR.

Comment (11): The authors do not explain how the spatial weights were created. Was queen contiguity, rook contiguity, distance, or k-nearest neighbours used? Were the weights standardized? How many permutations were used to determine significance? These details are necessary to reproduce the analysis. Also, the equations for Moran's I should be checked carefully because Equation 2 does not appear to be written correctly.

Reply: Thank you for this valuable comment. We agree that the construction of the spatial weights matrix, the significance testing procedure, and the mathematical expressions of Moran's I should be described more clearly to ensure reproducibility.

In this study, a k-nearest-neighbor (KNN) spatial weights matrix was constructed with $k = 4$, meaning that each observation was connected to its four nearest spatial neighbors. The spatial weights were row-standardized so that the weights associated with each observation summed to one. The statistical significance of Global Moran's I was evaluated using the associated z-score and p-value, while Local Moran's I was assessed using GeoDa's default settings of 999 random permutations and a significance threshold of $p < 0.05$.

We have revised Equations (2) and (3) and clarified their notation and parameter definitions in accordance with the original methodological references (Moran, 1950; Anselin, 1995). The revised formulations explicitly specify the spatial weights and normalization terms in Section 3.2.3.

Comment (12): NDSLI appears to be the only main explanatory variable in the regression. However, lake ice temperature can also be influenced by air temperature, snow cover, radiation. Etc. The conclusion currently says that the results "robustly confirm" I think this should be changed to something more careful, such as saying that sediment coverage is strongly associated with the spatial variation in LST.

Reply: Thank you for this comment. We agree that the interpretation should reflect the scope of the

regression models. NDSLI was the sole explanatory variable, while air temperature, snow cover, and radiation were not explicitly included.

We have revised the Conclusions to describe the association between aeolian sediment coverage and spatial variations in LST during the analyzed observation periods, replacing the previous expression “robustly confirm.” We acknowledge that other environmental factors may also contribute to the observed temperature patterns and that the present models cannot isolate the contribution of sediment accumulation.

Comment (13): The Methods say that images were selected when the lake was in a stable freezing period. However, the Results later say that one image represents a non-freezing condition and another represents snowfall conditions. If these images were intentionally included as comparison or control cases, please explain this clearly in the Methods.

Reply: Thank you for pointing out this ambiguity. We agree that the original description could give the impression that all selected observations represented stable freezing conditions. In fact, stable freezing conditions were the primary focus of the image selection, while observations representing non-freezing and snow-covered conditions were intentionally retained and included in the subsequent analyses as comparison cases. We have revised the Methods section to clarify this selection strategy and to explicitly state that these observations were included to provide contrasting surface conditions for comparison with the stable freezing cases.

Minor Comments

Comment (1): Grammar and Wording

L26: "Sediments accumulation" should be "sediment accumulation". There are other places in the document with the same issue.

L32: "Researches" should be "Research." Other parts of the document have this issue as well.

L42: "Beside" should be "Besides."

L47: "The" is missing. Change to: "At the Headwater Region of the Yellow River."

L50: Change "In Mars" to "On Mars."

L31: "The" is missing in two places. Change to: "...between aeolian sediments and the ice-covered period of the lake."

L77: A period is missing after "conditions."

L189: Remove "was." Change to: "NDSLI < 0 accounted for 46.1% of the total lake surface."

L249: "Exhibit" should be "exhibits," and "a" is missing before "bimodal pattern."

L347: "Researches" should be "research."

L408: "The authors declare that they have no conflict of interest.." There is an extra period at the end.

Reply: We thank the reviewer for carefully identifying these grammatical and wording issues. All corrections have been made throughout the manuscript, including "sediment accumulation," "Research," "Besides," "At the Headwater Region of the Yellow River," "On Mars," the missing articles and period, "NDSLI < 0 accounted for 46.1% of the total lake surface," "exhibits a bimodal pattern," "research," and the removal of the extra period in the conflict-of-interest statement.

Comment (2): Sentence Structure

L231: "These evidences indicate that the aeolian sediments accumulation significantly changes the thermal distribution of ice-covered lake surface..." Check this sentence.

Reply: We thank the reviewer for this observation. The sentence has been revised for grammar and clarity. It now reads: "These results of evidence indicate that aeolian sediment accumulation significantly changes the thermal distribution of the ice-covered lake surface..."

L267: "Only one date showed relatively good fitting result ($R^2 = 0.567$) is December 31, 2013, whereas other dates exhibited poor fitting results..." Check this sentence.

Reply: Thank you. This sentence has been corrected to: "Only one date, December 31, 2013, showed a relatively good fitting result ($R^2 = 0.567$), whereas other dates exhibited poor fitting results..."

Comment (3): Date Consistency

There are some date inconsistencies:

Table 1: Landsat 2017-11-08 / MODIS 2017-11-07

Figure 3(d): 2017-11-07

Figure 4(e): 2017-11-20

Section 4.4, Line 208: November 20, 2017

The dates are not consistent. Please check whether November 20 is a typo or a different observation date that was not included in Table 1. This is important because this date is linked to the reported Moran's I value of 0.8327.

Reply: We thank the reviewer for catching this important inconsistency. We have verified the original data and revised the date information to ensure consistency among Table 1, Figure 3(d), Figure 4(e), and Section 4.4. The date associated with the Moran's I value of 0.8327 has been corrected accordingly.

Comment (4): Results Section Numbering

The Results move from 4.1 to 4.3, then 4.4, 4.5, and later back to 4.4. This is confusing.

Reply: Thank you for pointing this out. We have reorganized the subsection numbering in the Results section so that the sections now appear in a clear and logical order without repetition or backward numbering.

Comment (5): Figure Caption

The caption to Figure 8 reads "OSL" where "OLS" is intended.

Reply: Thank you. We have corrected "OSL" to "OLS" in the caption of Figure 8.