Review of: "Thermal Remote Sensing: A tool to Determine Temporal Land Surface Temperature in Hawassa City, Ethiopia"

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Potential competing interests: No potential competing interests to declare.

1. Original Submission

1.1. Recommendation

Major Revision

2. Comments to Author:

Ms. Ref.

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Thermal Remote Sensing: A tool to Determine Temporal Land Surface Temperature in Hawassa City, Ethiopia

Overall opinion: This paper attempted to estimate land surface temperature (LST) changes in Hawassa City (Ethiopia) using spaceborne remote sensing. However, according to the abstract and conclusions, the findings are extremely scanty and do not meet the requirements of a peer-review study. Only two mean estimates of LST are reported over the chosen location during seemingly two randomly chosen dates, which also critically undermines the motivation of such fragmented study. Of the reported mean LST estimates, the one from 22.03.2022 is likely statistically insignificant considering huge standard deviation. The rest findings based on Figures 4 – 6 are not reported with sufficient details and are only partially described. Critically, the research aim was not articulated; if you state that you would measure something, it does not mean such measurement or calculation automatically becomes your research aim. Only by answering why you measured LST during these two dates in this location, you can formulate your research aim/question. Furthermore, the methodology is ambiguous, some conclusions cannot be justified, many sentences have no grammar or practical meaning. Overall, the level of grammar, style and punctuation is below average – many signs of raw condition of your manuscript can be seen just by the first glance. Despite the very low quality of the current material, I see some potential for the improvement here. In this light, I strongly recommend to perform a major, extremely thorough revision of your article, where you can improve the methodology, formulate your research aim, provide more detailed results and ensuring the statistical and scientific significance of these results. Below, please see the detailed comments which can help you with revision.

2.1. Major comments:

1. **Abstract:** The motivation and research aim of the study were not articulated. Peer-review process implies that you are able to explain why it is important to study your topic. In particular, why it is interesting to examine temporal land

surface temperature in Hawassa City. You dedicated some efforts to classify land cover type in the study area (such as the description of your results below Table 2), but this information was not reflected in the abstract.

2. **Technical Terminology:** Normalized Difference Vegetation Index is referred as 'NDVI' in remote sensing-oriented research. However, you did not introduce this acronym in the abstract, nor explained what is NDVI in the same section. On the other hand, some acronyms were not introduced and used right away (such as LST – Land Surface Temperature in the abstract). The use of acronyms is also inconsistent because in the introduction you returned back to "land surface temperature" full term. Please also adhere to commonly used terms for denoting your units. You can utilize a commonly used short terms such as 'km^2' instead of 'Square km' (page 3/18). The references to degrees Celsius are incorrect as well. Instead of ^0C it should be ^oC. Note the difference between 'o' and '0' (zero). The term DN was never introduced and was used right away as acronym starting from page 6/18. I assumed DN reflects the term 'Digital Number'.
3. **Language:** The language can be considerably improved from both grammar and stylistic perspectives. There are many stylistically cumbersome formulations such as "*This investigation was conducted for the estimation of the temporal land surface temperature value*" (why not to say "we estimated the temporal land surface temperature" for instance?), "satellite datasets" (more concrete description of dataset can be used). See also unnecessarily long and puzzling formulations such as "*It has been seen that there is an increment in the built-up area from 2002 to 2019*" instead of "increase in the built-up area was discerned between 2002 and 2019". Moreover, you said that your analysis was conducted in the Hawassa City Administration. How spaceborne analysis can be applied to administrative organization? Perhaps, you meant a city itself here. All these examples were taken from the abstract, but there are much more to find in the main text. Some sentences in the abstract critically lack the verbs, which makes them non-sensical. For instance, this sentence "*Urban expansion (built-up area roads, and other impervious surfaces), decline in vegetation levels due to deforestation, an increasing population density*." has no grammar meaning, let alone, any practical significance. Punctuation of the article is poor – for instance, you do not have a dot in the end of the sentence, which finished your abstract. Moreover, there are obvious grammar errors such as in this sentence, for instance "*Calculating Land Surface Temperature is the final step in**this algorism***" or "*lower LST **esteems**"*
4. **Methodology:** I have serious concerns about whether remote sensing-based analysis of just two days can reveal any hidden patterns in land surface temperature in a certain location? Why did you choose these particular dates? How can you make robust conclusions about the studied topic if no time series, seasonality or long-term analysis are considered? The justification of studying these dates is missing. Many unclear terms such as "land use land cover maps" are used without explanation (see the abstract) or "the discrepancy vegetation index" (2.3 section). The description of data (2.2 section) is insufficient. You have not provided such essential aspects of remote sensing data as (1) the core reference, required for every single spaceborne dataset to be used in a peer-review study, (2) temporal resolution of the studied parameter, (3) explanation how NDVI was calculated, (4) description of the uncertainties of NDVI from TIR, (5) reference to QGIS package in any form (section 2.3). This gap makes your study essentially unjustified from methodological viewpoint. Also, sometimes you say Landsat, but then you say LANSDAT. Please be consistent here. The geometrical correction procedure was mentioned but never explained (page 6/18). You also mentioned that it is important to retrieve TOA spectral radiance but you did not explain why it is important to do so. I

refer to the sentence "*On the other hand, Normalized Difference Vegetation Index is very essential to identify different land cover types of the study area*". Moreover, this sentence is unjustified given the absence of academic references mentioned here. Although NDVI essentially reflects the difference between the reflected and absorbed vegetation, it cannot be solely used to distinguish different types of vegetation with similar reflectance characteristics (such as broadleaf forest and mangrove forest, for instance). Due to this fact, the land cover classification you describe above Eq. (7) is over-simplified and unlikely meets the requirement of a peer-review study, which needs to precisely determine the land cover type. Regarding the land cover classification, for me it is rather unclear why you decided to classify the land cover by your own efforts. It does not bring any new information as the land cover classification has been previously conducted for entire Africa in high-resolution. By making a new classification of land cover, you force yourself to make a new evaluation of your results, thereby complicating your methodology beyond the research objective you set (to analyze LST in a given location). Why not to use the existing land cover dataset. You may find a plenty of such datasets by following this link for instance (https://freegisdata.rtwilson.com/), search "land cover" datasets and "land use" category. Most of these datasets have valid references and had been previously evaluated. MODIS temperature products which were used for validating your main LST findings should be more explicitly described in the methodology. You mentioned them for the first time in the results (Page 14/18), while neglecting such description in the methodology.

5. **Introduction:** The introduction is over-focused on the LST and the methods for retrieving LST from various spaceborne platforms. However, the justification why to study LST in this particular location (Hawassa City) was not articulated. You also did not explain why you selected only two days for your analysis. The justification you provided in the results is insufficient because it just informs a reader about your choice without providing any arguments about why these days and not any other random days from other years were used for this purpose. Moreover, I would suggest you to shorten the description of LST importance itself and create a paragraph explaining the importance of studying LST in the Hawassa in particular and in Ethiopia in general for instance.
6. **Results:** Please note that the only results you reported in the abstract are scanty and rather trivial. In particular, you reported the change of temperature during two days where a mean daily temperature of 22 March is substantially lower than a standard deviation during the same day, thereby sparking the question whether this estimate is statistically significant or not. Moreover, this kind of analysis is simplistic and can be done by operational means (downloading the data, calculating mean and standard deviation) without necessity to make a peer-review study. In other words, what is the significance of these two reported numbers for this particular location during two randomly chosen days during this year? Spatio-temporal variability of LST, reported in the paragraph below Figure 5 was not reflected in the abstract despite it was stated that such analysis if one of your key objectives. The same can be applied for the 2002 – 2019 LST dynamics, which were never mentioned in the abstract. Figure 6 reflects the minimum and maximum temperature trends in 2012 – 2017 for every month, but these results were never mentioned in the abstract, thereby limiting the useful result you obtained to unacceptably limited results (namely, to 2 mean estimates of LST in the abstract). Although you stated from time to time in your manuscript that one pattern was determined or driven by some other phenomenon, you do not provide any causal or statistical arguments to support any of these statements.
7. **Ambiguity in presentation**: Many formulations are undesirably ambiguous in the manuscript. For instance, such

formulations as “different algorithms” (in the abstract) can be elaborated or specified. Such references as “Source: compiled by author” (page 5/18) are unacceptable for peer-review. It was not clarified what are “the thermal constants in the metadata file” (page 7/18). There was no direct reference to metadata as well, which makes your study unreproducible. It is unclear what do you mean by “hypothesis regarding atmospheric parameters” in section 2.5. Please explain this hypothesis timely. Although you stated in the paragraph below Figure 3, “*This clearly shows the healthy vegetative condition in the year 2002 as compared to 2019*”, a reader cannot understand this clear pattern without your proper explanation in the text.

8. **Validation of the results**: You have mentioned the validation procedures in 2.5, but no details were provided. The qualitative description of the validation procedures lacks the following aspects. First, you mentioned that it is important to determine “its precision”, but the evaluation of the precision was not presented. Moreover, the exact precision of the applied methods was not mentioned in the previous sections. Second, you state that it is important to evaluate the current results with some co-located measurements, but in the next sentence you admit that you do not have the solution for this problem. Thus, what is the point of such description? I see that it only underlines the methodological gap in your validation aspect. Third, you mentioned the cross-validation technique, but it is unclear what do you mean by this term. In particular, the approach of cross-validation should be qualitatively and quantitatively described. The only sentence that has some practical sense for me in this section is “*we used regular data sets of Landsat 8- OLI earth surface temperature products with precision within 1oK range (Weng et al. 2004) to confirm the earth surface temperature of Landsat 8 satellite image.*” I would cut this sentence from this section and paste it into the “methodology”. The rest descriptions provided in this section are redundant and can be deleted because they do not reveal any important information for a reader.
9. **Figures**: A tip for future studies. The description of any figures should be explicit. Second, if few panels are plotted on a single figure, an explicit description of each panel should be given. For instance, in Figure 1 you have not explained what is the orange-green gradient we see on the left panel? Is it a real color image? Moreover, you have not described what is the red line, outlining the study area. Please explain every panel by denoting them a, b and c, for instance.
10. **Conclusions**: The conclusions are inconsistent with the abstract because you state here that you analyzed 17 years of data, while in the abstract you mentioned only two days of the analysis. Moreover, the conclusions do not reflect the patterns you found based on Figure 6, you only highlighted the results regarding these two days. On top of that, you mentioned some correlation between the distribution of the land cover with the abundance of vegetation, but two aspects are obscure. First, how this analysis is related to your objective associated with LST analysis. Second, where did you present this correlation analysis? I have not noticed any quantitative statistical analysis in your study (at minimum, the correlation strength and significance should be provided). The implication about “the role of geospatial technologies” is not tailored to your findings. You just reported two estimates of mean LST in conclusions and the demonstration of geospatial technologies cannot be attributed to such scanty results in any way. If you support the findings, demonstrated in figures 3 – 6 by convincing arguments and consistent narration during the revision, you may provide an implication about LST changes in this particular city. Perhaps, some hidden LST patterns might be useful for local policy-makers for making decision on urban planning or agricultural activities. Please note that despite such tailored implication might be potentially formulated based on your improved results in the next version, I am convinced

that it is better not to mention the geospatial technologies here at all. The scales of your study do not correspond to such broadly profound recommendations.