Review of: "Algal bloom monitoring in Koka Reservoir, Ethiopia: Application of satellite remote sensing algorithms"

Mariano Bresciani[1]

1 Italian National Research Council

Potential competing interests: No potential competing interests to declare.

The authors use two different algorithm applied to two different satellite sensors in order to evaluate the possibility to use this approach to monitoring algal bloom in the Lake Koka.

The article doesn't present innovative methodology, with the exception of the study area. I think the authors should revise the paper to try to make the analysis more aimed at monitoring rather than evaluating algorithms. In fact, by not proposing new algorithms and not presenting particular validation results, the methodological part is very weak while the discussion part is more interesting.

In fact, to be able to robustly test the algorithms, a validation of the reflectance products with the comparison with in situ reflectance measurements would be needed and a larger data-set of limnological in situ measurements synchronous with the satellite products would be needed.

Figure 5b clearly shows the problems related to the accuracy of the proposed algorithm, if the authors want to focus on a methodological work related to algorithms with these results I think the work is not acceptable.

The proposed algorithms, already extensively studied in the literature, are suitable only with high concentrations of chl-a in the surface layer of the water, in figure 5b it is clear how with concentrations lower than 10 µg/l these algorithms are not suitable. The application of indices such as FAI and NDCI can only be performed in eutrophic conditions, for water conditions not dominated by phytoplankton the authors should use a semi-analytical approach.

There are some limitations in the presentation of the work, the authors don't explain how they consider the difference of the spatial resolution of the MSI sensor compared to the OLI sensor, have you done a resampling in order to have the same pixel dimension? The autohors have considered the problem of adiacency effects? In the RGB image used in the figure 3 the presence of glint is evident, did the authors correct the images for this disturbance, if so how?

The graph in figure 2a must have the same axis dimensions!

The data-set used which is shown in figure 2a is not very representative compared to the range shown in figure 2b, in fact in figure 2a the values are between -0.08 and -0.02 while in the rest of the work values are shown between -0.08 and 0.04.

I believe that some of the graphs presented are redundant, they could be removed and summarized with some statistical

analysis indices in a table. This would allow for more space for a spatial and temporal analysis of the results for monitoring purposes.

The authors mention in the abstract and in the introduction the “conventional water quality monitoring technique”, I think is necessary better explain what is this…is only related to the sampling collection or is the technique? The authors include in the conventional the in fixed in situ fluorometer?

In conclusion, I believe that the article cannot be published in this form, I advise the authors to rewrite it emphasizing the usefulness for monitoring and not for evaluating the proposed algorithms and to improve all the part relating to the processing of satellite data.