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

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Potential competing interests: No potential competing interests to declare.

Eritrea and colleagues compare several satellite related methods to help describe bloom dynamics of a large Ethiopian lake of high local importance. The paper is very relevant, remote sensing is an increasing concept in environmental science and this paper proves that it can be widely exploited, improved and eventually applied.

The paper is well written, however I have several methodological concerns that in my opinion needs clarification/improvement. (small suggestion: line numbering would have been easier for the review process)

- P1: Abstract, last line: something's lacking. I assume ' FAI algorithm is sensor-insensitive suggesting…'
- P4: Data collection and Approach: 'The resulting regression equation between FAISen2A and NDCISen2A was further evaluated to compare FAIOLI with NDCIMSI at four different times (i.e., April 8, June 27, September 15, December 4, 2020), when both satellite overpasses were coincident' It reads like this analysis was done for both lake parts separately but I assume this was done for the whole lake?
- P4: next sentence: 'Meanwhile, the relationship between FAIOLI and in-situ chl-a concentrations was examined using field sampling data obtained at three offshore locations (i.e., sites No. 1-3) from 2013 to 2016 (the total number of data points was 27) and three other different locations (i.e., sites No. 4-6) on May to August 2017 (the total number of data points was 12).' Was it not possible to execute a similar analysis between in-situ chl a concentrations and NDCI? Explain why (not) in the paper. About the 2013-2016 in situ chl a measurements, where these used to compare with the satellite imaginary data of 2019-2020? If so, this is not scientifically sound to do in my opinion... Bloom dynamics normally show significant inter- and intra-annual heterogeneity depending on a multitude of factors. If such a comparison is made, one should prove first that the bloom dynamics in this lake are very stable and predictable.
- P6: Figure 2 caption: **Forty** random points…?
- P7: 'To derive a relationship between FAIMSI and NDCIMSI , we decided to separate data from the south part from the January to April data, while data from the north part were combined with data from May to November.' What was the justification for adding data from other time points and from the whole lake to the northern dataset to compare with data from jan-apr from the southern part? Why not just comparing south and north datasets for jan-apr only? Not sure if the comparison as it is now is statistically sound…
- P7: 'Again, we observed in Figure 4c that in April and December…' There is no Fig. 4C
- P8: 'As shown in Figure 5, (a) and (b), data from site No.2 were well fitted to the previously proposed equations (Mishra and Mishra, 2012 and Makwinja et al. (2022); however, data from site No.1 and No.3 were consistently higher or lower than the solid or dashed lines.' Site NO.2 is only represented in fig. 5b. Explain what these equations mean and why

you make use of them...

- P8: ‘We acknowledged that sampling dates were unknown exactly, except for months, and there might be a possibility that sampling water contained higher chl-a. However, based on the stability of FAI values in about 20 days, the Lake Koka waters appeared to be not very variable within this range, so we suspect that chl-a concentrations in site No.1 were too high.’ Unclear what is meant here, the not exact sampling dates are a serious flaw in my opinion, also the rationale behind the suspected too high chl a values is not clear, these are based on measurements, how can these be too high? Also to compare chl a in-situ measurements from 2013-2017 with satellite imagery data from 2020 is not scientifically sound...
- P9, Figure 5 caption: ‘the relationship between OLI-derived NDCI vs in[1]situ chl-a obtained at three offshore sampling locations (site No. 1 to 3) in 2013 to 2016 (N=27) and obtained at sites No. 4 to 6 in May to August 2017 (N=8).’ Uncertain what is compared here, where for each time point, satellite derived data available? Please indicate with which data you compare the in-situ chla a data, but see earlier comments…
- P9: ‘OLI-derived NDCI and the higher and lower NDCI lines in Figure 5(a) were obtained from equations presented in Figures 2 and 3. FAIOLI was first converted to FAIMSI , and then NDCI was calculated based on the two different regression equations. The error analysis was conducted for April and June datasets in Figure 5(a). The mean absolute percentage errors for those datasets were 20.0 and 36.2%, respectively.’ Uncertain what is meant here and the rationale behind these comparisons. Please clarify…
- P13: ‘The plausible range of the alert level for toxic cyanobacteria species is between 12 and 25 μg/L set by WHO (Matthews et al., 2012).’ Does the range represent C-biomass, chl a or microcystine (LR or total) concentration?
- P13: ‘Dilution of the reservoir water due to precipitation contributes to mitigating cyanobacteria bloom(Coffer et al., 2020). In fact, as shown in Figure 6 (c), phytoplankton activity was apparently suppressed across the entire part during a heavy rainy season.’ However, nutrient influx due to erosion in the catchment can also induce bloom formation after rainy periods…
- P13: ‘Also, in areas very close to the shore, water is optically shallow and could induce false signals from bottom reflectance, which could be misinterpreted as phytoplankton (Coffer et al., 2020).’ Please also extend this with macrophyte coverage issues, as was already mentioned at the end of P. 8.