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Discussions

Interactive comment on "Evaporation suppression and energy balance of water reservoirs covered with self-assembling floating elements" *by* Milad Aminzadeh et al.

Anonymous Referee #1

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This manuscript describes the formulation of a numerical model to describe the water quality behaviour of a reservoir covered in floating, evaporation-suppressing elements.

The work undertaken by the authors appears to be novel as I am not aware of previous attempts to quantify numerically the change in fluxes from a reservoir covered in evaporation-suppressing elements.

However, I do have some serious concerns about the manuscript that would need to be addressed before I could support publication.

First, at best, this investigation is speculative as there is no verification data presented to show whether or not is predictions are robust.

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Discussion paper

At the simplest level, it would be anticipated that if a reservoir was covered in such a way as to prevent the penetration of electromagnetic radiation into its surface by elements of low thermal conductivity, the surface thermal forcing would be reduced. Therefore the outcomes shown in Figure 6 are not surprising.

The authors have elected to use meteorological data that does not appear to have been gathered in the vicinity of a reservoir and apply it to a "hypothetical" reservoir.

The key question is the degree to which the predictions are reliable and the authors have not addressed this question.

As stated on page 11, the authors do have access to a model reservoir that is described in Appendix A. It is difficult to comprehend why they have not verified their model for a system where they were able to make measurements.

At present, I find it difficult to support publication of this manuscript given the degree of speculation in the modelling and that the authors already possess data that could be used to verify it, at least on a small scale.

Secondly, there are considerable inconsistencies in the formulation of the numerical model.

Equation (4) is the conventional expression for stress transfer at an uncovered surface in the absence of wind wave growth. On page 11, line 13 we are told that u*a has been determined from bluff body theory. There is no discussion of the merits of combining these characterisations when their underlying assumptions are clearly at odds.

Also, in Figure 2, the authors invoke a conventional approach to the numerical modelling surface mixing of reservoirs which encapsulates unstable convection due to surface cooling. However, such an approach is unreliable in terms of heat fluxes and the authors' own observations with their infrared camera should show. Certainly the longstanding work by Andy Jessup and his collaborators have revealed very different behaviour of the surface skin (responsible for radiant heat from the surface) from that of the bulk.

Given these significant inconsistencies, I cannot support publication of the manuscript in its present form.

To progress the speculation to estimating evaporation suppression or oxygen transfer in the absence of any measurements is unjustified in my view. Given the degree of speculation, I cannot support publication of these results until independent verification is available.

In summary, the manuscript has considerable inconsistencies as well as a high degree of speculation. Given these significant weaknesses, I cannot support its publication until these concerns are addresses.

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