

## ***Interactive comment on “Validation of lake surface state in the HIRLAM NWP model against in-situ measurements in Finland” by Laura Rontu et al.***

**Jason Williams (Editor)**

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Received and published: 22 March 2019

Dear Authors,

Thank you for your response to my concerns regarding your manuscript which has been published. I should re-iterate that my main conclusion regarding the data used was based on the strong statements from the reports submitted from two out of the three independent referees (anonymous referees 1 and 2) who questioned why you were submitting a paper using results containing a significant error. The nature of the process is that the referees reports must be used by the editor to guide the review process. I am from the modelling community and a co-efficient which is not correct is commonly referred to as a bug.

Regarding my proposed solutions: (i) It is very unfortunate that the data is no longer available for you to perform an alternative analysis. However, in GMD the version of the model has to be very specific to avoid confusion regarding versioning of the code so possibly you would have been validating an old version no longer used by the rest of the community. (ii) You state that you are not responsible for the FMI code, but of course you could have offered co-authorship to someone who is responsible for such data in order to overcome this obstacle. (iii) In that the single correction possibly would not solve all the biases in the model is understandable, but you would be able to differentiate between the biases which exist due to other issues concerning the parameterization and that introduced by a co-efficient 100 times too high. (iv) The progress of each manuscript through the review process is not time limited. There are instances where the GMDD article does not proceed to GMD because requests from the referees and editors are not addressed.

I would also like to comment on some of the statements that the authors have made in their last paragraph of their reply (bottom of page C3). The fact that HIRLAM/Flake data does not capture observations well in the spring is not the reason that the major revision was requested, as I wrote in my initial comment posted in the discussion forum. It is that the data used for the validation contains an artifact that could present a misrepresentation of the biases in future forecasts for the performance around lakes. It is a completely valid reason to ask for further attention to the manuscript, especially considering the comments made by the referees and the admissions made by the authors that the impact of this error is not easily quantifiable. A validation paper serves as proof that when used in the future, there is confidence that HIRLAM can capture events to the first-order at least.

Finally, in light of the significant changes to the manuscript by the authors I am requesting additional input from the referees to ensure their major points have been adequately addressed.

Best regards, Jason Williams.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-270>, 2018.

**GMDD**

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