

Interactive comment on “The Normalized Difference Infrared Index (NDII) as a proxy for soil moisture storage in hydrological modelling” by N. Sriwongsitanon et al.

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The new review and my editorial comments have pushed the paper into a direction that is indeed better than the first submission, but does not address the concerns in a way that is convincing enough for accepting the manuscript in its present form. To my opinion there is no doubt about the scientific value of this comparison between NDII and FLEX output, but the framing that NDII can be considered a reliable proxy of soil moisture even in ungauged basins is not adequately demonstrated by this study. The formulation of the hypothesis does not include any verifiable quantitative assessment (like “the NDII method is better than...” or “the NDII method is good enough to...”),

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and the current study set-up does not allow to test such a quantitative hypothesis.

However, I do agree with the authors that the study in itself is interesting enough to be published. So rather than pushing this study towards this prove of adequacy of NDII as a soil moisture proxy in ungauged basins, I would recommend to downplay the framing and concentrate on the basic results: comparing NDII to a model derived soil moisture index does lead to reasonable correspondence and gives scientific insights that support the value of NDII (such as the effect that it only works well under dry conditions, and does discriminate between different subcatchment typologies). One step to bring the expectations closer to the true value of this study is to change the title into something like “Comparison between NDII and results from a lumped hydrological model”.

Having said that, the technical layout of the manuscript is still not good enough. Not only is the supplementary information (showing results for the other basins than P.20 and P.21) is missing and is the format of the manuscript file(s) very inconvenient, it also contains quite some editorial issues (unclear cross-referencing to equations, an unclear description of the spatial set-up of the FLEX model in section 3.2, arguments for using the current study set-up that are distributed across various sections rather than concentrated in the introduction section).

The new reviewer has requested to evaluate a new version of the manuscript. In that review a much more thorough quantitative evaluation (with a distributed model, in ungauged basins) was required for this manuscript to be publishable. I do not insist on these new model simulations and analyses if the authors are able to improve the framing of this study and concentrate on the (valuable) findings that were found in their analyses. So I will enable the authors to submit a new version of the manuscript that addresses the points raised above without sending it out for review at this stage. However, this new manuscript will be sent out for review if that appears appropriate at that time.

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