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Comment on hess-2021-598

Anonymous Referee #2

Referee comment on "Using NDII patterns to constrain semi-distributed rainfall-runoff models in tropical nested catchments" by Nutchanart Sriwongsitanon et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-598-RC2, 2022

The present manuscript describes an effort to incorporate the Normalized Difference Infrared Index (NDII) into a semi-distributed hydrological model based on the FLEXL model structure (including distributed time lags and channel routing routines), to drive partitioning of the water balance in favor of more realistic soil moisture storage capacities in the Upper Ping River basin in Thailand.

The effort to test remote sensing products readily available to the hydrological modelling community is scientifically relevant, and the proposals to obtain maximum moisture storage capacities from annual NDII values seem interesting and relatively novel to me. However, a broader bibliographic context on the work being done by other research groups worldwide seems to be missing from this article.

It is my opinion that some of the conclusions presented in the paper are not supported by the modelling exercise, and some others are not substantial. After carefully reviewing the methodological strategy and the results, it seems to me that a part of the validation process is not rigorous enough from the point of view of causality. For example, when trying to validate with average NDII values the soil moisture contents that were simulated/constrained with the average and range of the very same NDII, as we would want to use independent (and ideally direct) observations to achieve rigorous validation. Following this precept, the use of the Soil Wetness Index (SWI) for validation purposes is more appropriate, although it is partly model-based and does not constitute either a direct observation or an indirect one that was empirically validated on direct measurements in the study area. Furthermore, the good correlation between the SWI and all models (whether NDII-based or not), both during the dry and wet seasons, leaves the impression that the contribution of the NDII is not fundamentally relevant to simulate the soil moisture component. It would be worth asking if the modelling could be made more efficient by constraining the soil moisture holding capacity routine rather with the SWI, which seems not to be particularly affected by the wet season effect, which was known in advance to be a limiting factor for the NDII.

Although in general the article seems well written to me, the wording of section 4.2 could be improved in order to avoid ambiguities (lines from 10 to 15 are not very straightforward). Tables 4 and 5 could also be improved, as they present duplicate data in some cases, and tabulations that do not seem appropriate. For example, by presenting results of the lumped FLEXL model calibrated for each of the various sub-catchments studied but tabulating these data under the table section: "for calibration at station P.1". Evidently a lumped model that was calibrated at P.1 could not provide results for individual upstream sub-catchments.

I have trouble endorsing some interpretations and statements expressed in the results section. But I think the article is especially lacking in its conclusions, which I believe are not being formally supported and proven. For example, on p. 14, L. 16-17, it reads: "The results indicate that the relationship with the average root zone soil moisture storage is affected by the ecology of the river basin.". The term "ecology" is extremely broad and complex, and there is no definition and characterization of it in the article. Even less, a scientific demonstration of such soil moisture-ecology relationship. The article goes on to state that: "The results confirm the power of NDII to capture the spatial variation of root zone soil moisture within the sub-catchment scale", which, again, is neither accurate nor was it strictly proven through the scientific method.

On p. 15, L. 9-11, it reads: "As a result, the NDII appears to be useful to constrain hydrological models during dry conditions and both SWI and NDII appear to be useful to test model performance and to assess moisture states of river basins.". As stated earlier in this review, if NDII were used as a soil moisture model forcing, would not the simulation be expected to show this correlation? Furthermore, how sensitive is the model to forcing if even having constrained the soil moisture routine with the NDII, the result reveals a poor correlation between them during the wet season?

On p. 15, L. 18-19, it reads: "… it has been shown that it is required to account for the spatial variation of the moisture holding capacity of the root zone.". In fact, this was not shown, considering that the best independent validation tool presented in the article is the SWI, which was shown to be highly correlated with the simulated soil moisture storage regardless of the inclusion of the NDII constrains in the model. In addition, including or not the spatial variation of the moisture holding capacity of the root zone is not necessarily required in all cases, but rather depends on the objectives of the modelling exercise. For example, if the objective is the best possible calibration of a rainfall-runoff model in the outlet of a basin for seasonal hydrological prediction purposes, as we have seen, in some cases a lumped model could generate greater efficiencies.

P. 15, L. 20-21: "We concluded that the maximum of a series of annual ranges ($NDII_{MaxMin}$) and annual average ($NDII_{Avg}$) of NDII values offers an effective proxy for estimating the appropriate Sumax values in the different sub-catchments. ". Again, it seems to me that the data presented cannot support this conclusion.

P. 16, L. 1-2: "… However, during the wet season when soil moisture is replenished as a result of rainfall, NDII values are no longer well correlated with soil moisture.". In multiple parts of this article reference is made to "soil moisture" in order to later infer and conclude facts (such as its possible relationship with the NDII during the wet season), without the precaution that this soil moisture is not actually observed, but only a modeled value that was partially validated based on an indirect index (SWI). Due to the above, it does not seem appropriate to refer to soil moisture without specifying each time that it is a simulated value, nor drawing conclusions based on said simulated values, considering that they have not been sufficiently validated in the field and, therefore, cannot offer acceptable levels of accuracy and precision.

In conclusion, I believe that this article could be substantially improved by better contextualizing it within the current global research environment on the specific topic of using remote sensing to improve soil moisture simulation in distributed hydrological models (how many other research initiatives around the word are trying to use the NDII to better simulate soil moisture storage capacity?). I would also suggest better organization, simplification and clarification of the description of the methodology (particularly section 4.2), and a thorough review of the modelling strategy, to avoid later interpretations that are irrelevant or based on spurious relationships (such as correlating an explicitly introduced forcing of the model outputs with those model outputs) or reaching conclusions that were not subject to hypothesis and testing (such as concluding about the ecology of a basin without having first systematized and analyzed that concept, or establishing the need to adopt a specific approach such as distributed modelling without sufficient empirical evidence to support it).