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# ***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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2.1 Use of model vs. observations In this paper the NDII is compared to the soil moisture storage content ( $S_u$ ) from the FLEXL model. My main point here is why the authors did not use observations of root zone soil moisture? A model is an approach of reality, but it depends on the quality of the model how close the model represents reality. Gao et al. (2014) show that the FLEXL model did not perform well in simulating low flows in two sub-catchments in China, which is important in this study since periods of low flow are the periods of interest. Using a distributed model instead of a lumped model sounds better when modelling a dynamical process like root zone soil moisture.

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However, Gao et al. (2014) showed that the distributed version of the FLEX model did not perform better, because of the inappropriate soil moisture distribution by the Thiessen polygons. How is then guaranteed that the model will give reasonable values for root zone soil moisture?

Reply: We hesitate to compare our results with observed soil moisture. Firstly, observations of soil moisture data are not widely available. But moreover, it is not straightforward to link soil moisture observations to the actual root zone soil moisture. Most observations are conducted at fixed depths and at certain locations within a highly heterogeneous environment. Without knowing the details of the root distribution both in the horizontal and vertical plain, it is hard, if not impossible, to estimate root zone soil moisture. We should realise that it is difficult to observe root zone soil moisture even at a local scale. But measuring root zone soil moisture at a catchment scale is even more challenging. The state-of-the-art remote sensing technique can observe spatial distributed soil moisture, but what they see is only the top layer if there is no interference of vegetation cover. This top layer moisture may be correlated with the root zone storage, but it is definitely not the same. Our method assumes that the NDII sees the moisture in the leaves of the vegetation, which is subject to the suction pressure of moisture in the root zone. The leave water content (and tension within the leave) then acts as a manometer of the suction pressure in the root zone moisture, which is a function of the available moisture. So the NDII has a causal connection with the moisture content in the root zone. Using FLEXL model is not a problem. Gao et al., 2014 showed that FLEXT has better transferability if we take landscape heterogeneity into account. But this does not indicate we cannot use FLEXL to simulate hydrological processes in catchment scale. The point is we have to recalibrate the FLEXL model before we use it, which is normally conducted by most hydrologists and not a problem in these gauged stations. The similar calibrated results of FLEXL and FLEXT also supports the choose of FLEXL (Gao et al., 2014). Gao et al., (2014) did show that the landscape based model (FLEXT) has more capability to simulate base flow than the lumped model. The base flow in the dry seasons is generated by the groundwater

reservoir, and is almost independent of root zone storage. The better performance of FLEXT over FLEXL in baseflow simulation is because it considers the interaction between the groundwater reservoir and the wetland dynamics, which is independent of root zone storage. And we do not think root zone soil moisture is not well presented in FLEXL. The water retention curve in the FLEXL model was developed based on the variable contribution area theory (VIC), which has been tested and widely used in many different catchments all over the world.

I think that using observations to show how the NDII correlates with root zone soil moisture would be more convincing. I therefore suggest to test the model first with observations in the study area, or even better, test the NDII with observations. At least the performance of the model should be discussed.

I see a confusing circular reasoning in the title of the paper, apparently root zone soil moisture is not well presented in hydrological modes and the paper wants to fill in this gap by using NDII. However, the usefulness of NDII is tested with a hydrological model (which does not present root zone soil moisture well)! By stating this the authors are actually not convinced by their own methods of using a model. The authors tone down their title statement a bit by mentioning once that the NDII could be used for calibrating models in ungauged basins. However, also in that case you want to make sure that the values used for calibration represent reality. An example showing that model root zone soil moisture is still a major challenge is the study of Das & Mohenty (2006) where a vadose flow model did not model the root zone soil moisture perfectly. Also this point is therefore solved by using observations. Though, if the authors assume that modelling root zone soil moisture is possible with their model, the use of NDII for calibration in ungauged basins should be made more clear in the beginning, starting from the title.

Reply: Validation by root zone soil moisture observation is restricted by the limited observations available and other reasons mentioned above. We do think the simulated Su indeed represents the root zone storage, which can be used to compare with NDII. We will change the title to avoid this misunderstanding. The beauty of our method

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is that we compare the NDII with the root zone storage that is actually active in the dynamics of the rainfall-runoff process. Our comparison is NOT circular, because the hydrological model is calibrated on the runoff dynamics, whereby the Su is a side product. This side product is then compared to an independent observation of moisture stress in the leaves of the vegetation. So it is not circular at all. The argument about the study of Das&Mohanty is probably not valid, because they probably used soil moisture probes at fixed depths, whereas roots don't take water from fixed points, both in depth and in the horizontal plain. Here we fairly compared lumped NDII with Su from a lumped model. The fact that it works for a lumped model is already a strong indication that there is real information. In follow-up research we would like to use a landscape based FLEX, with distributed accounting of soil moisture in response to distributed drivers (precipitation and potential evaporation). We expect that this will lead to even better comparisons between NDII and distributed root zone storage

Another issue of only using a model is that the water stress periods, where NDII performs best, can be virtual water stress periods. It is not sure that with low values for logarithmic Kling-Gupta efficiencies (but still reasonable), the moisture stress periods are modelled well. Also here a validation with observations would help.

Reply: As we discussed before, the baseflow is almost independent of root zone soil moisture in dry seasons. The logarithmic Kling-Gupta efficiency is a good indicator to evaluate the model performance for baseflow, but that is not related to the root zone storage in dry seasons.

In the end of the paper, the authors conclude that there is almost no time lag between NDII and Su and that vegetation shows a fast response to soil moisture changes. However, this cannot be concluded because the evaporation is not modelled in a dynamical way in the FLEXL model. The changing vegetation is not taken into account for the evapotranspiration, so the vegetation is assumed to be constant. Since evapotranspiration is related to soil moisture, a static value (Su) and a dynamical (NDII) value are compared. This does not give relevant information and is crucial for this research

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where vegetation is used instead of omitted. This means that a higher NDII because of a higher density of plants will cause a lower Su than reality and the other way around for a lower NDII. If the dynamical vegetation is taken into account it will probably improve the correlation. As suggested in the paper itself, NDVI could be used to model the actual transpiration of an ecosystem. Next to that, Wang et al. (2007) concludes that there is a time lag of 10 days and a lag of 5 days for humid and semi-arid sites, respectively, of vegetation to respond to soil moisture changes. Therefore I think the conclusion about no time-lag is not grounded and the analysis should be done again, taking a dynamical vegetation into account.

Reply: The phenology (vegetation dynamics) is a good point, although in our case the vegetation is primarily evergreen. Regarding the dynamical vegetation, we did not consider the dynamic vegetation explicitly in the FLEXL model. The evaporation, including both interception and transpiration, is calculated based on the available energy and the root zone soil moisture constraint. The Su reflects the root zone soil moisture condition, which reflects the vegetation dynamics. We found that this pure energy and water derived Su is highly correlated to the vegetation water observed by NDII, without time lag. We will include more discussion, considering the reviewer's comments in the revised manuscript. Surely, we should take the phenology into consideration in a follow-up publication.

2.2 Seasonal correlation of NDII and Su My second major point is the way in which NDII and Su are correlated. In my view everyone knows that in the wet season the soil has a higher soil moisture content than in the dry season (Wang et al., 2007). A seasonal correlation does not add new information. It is general known that seasonal correlations always give high R<sup>2</sup> values, since a lot is correlated with that (Schaeefli & Gupta, 2007). The use of umbrellas would in that case also be a reasonable indicator of soil moisture. What would be of interest is to know how root zone soil moisture behaves over different years. If different august months are compared with each other, would the NDII be able to detect a wetter and drier august month? That are the dynamics

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we are interested in. Therefore I suggest to get the seasonality out of the dataset, the same as Wang et al. (2007) did. If the deseasonalized dataset shows no correlation between Su and NDII, the NDII would not be a better indicator of soil moisture than just the day of the year.

Reply: Deseasonalization is really a good point. We will do that. In fact, we have already done so, and we shall include it in the revised paper. By the way, we agree with the reviewer that the use of umbrellas would be a reasonable indicator of the moisture content of the surface (not the deeper soil moisture). Since the use of umbrellas is simply related to precipitation, and precipitation is highly related to soil moisture of the top-soil.

2.3 Calibration, validation and correlation The Kling-Gupta efficiency for flow duration curve showed the best result, the efficiency for low flow the worst but still reasonable result. Since the whole research is based on periods where soil moisture is limited it is better to achieve the best efficiency on low flows. I suggest to define a different parameter set that has the highest Kling-Gupta efficiency for low flows.

Reply: We would like to keep all the parameter sets on the Pareto-frontier. The equifinality is unavoidable in a hydrological model. It is difficult to determine the best parameter set only by the highest KGE value for low flows.

Next to that, the model is not validated, only calibrated. The sub-basins wherefore the model is calibrated with runoff data from the sub-basin are used to determine the correlation between NDII and Su. No validation is needed here probably, because the model is only used to derive values for the root zone soil moisture. However, it should be stated more clear in the paper that NDII is compared to Su only for the calibration periods. The authors should also mention that this has some consequences for the correlation in the different basins. Some dry or wet years are not taken into account for specific sub-basins because of a shorter calibration period. It would be better to have the same comparison period for all the basins.

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Reply: We will clarify the Su is only for the calibration periods, and the possibility of missing some extremely dry/wet years due to limited calibration period. And we agree with the reviewer that it is ideal to compare the results from different basins with the same time periods, but due to the limitation of observation, it is hard to find long overlapped datasets. But we do not think this is a big problem in this study. Because this study is mainly conducted to compare the NDII and Su of the same catchment. And comparing among different catchments is not the main objective of this paper.

Furthermore, the authors claim that this NDII can be used in the future especially for predicting discharge in ungauged basins. I think it is a missed chance that the authors did not show this. It is not clear what the authors mean with predicting discharge in ungauged basins. Will it be used for calibration or not? If NDII will be used for calibration, the authors could have use the NDII to calibrate the model for one of the runoff stations and see if this gives reasonable results compared with the runoff data. If the NDII is not used for calibration in ungauged basins but just as input data, they could have test the model with NDII input for one of the other non-calibrated six basins and see if the results are reasonable. Or use the NDII as input for the model without calibrating in one of the basins with a runoff station and compare the outcome with observations. However, Gao et al.(2014), shows that the FLEXL model did not perform well without calibrating. This adds questions again for the use of the model, especially by saying that it can be used for ungauged basins, without saying that NDII as proxy for soil moisture is used for calibration, so that a model needs to do all the work.

Reply: This manuscript shows the clear relationship between NDII and Su, especially in dry seasons. This is highlighted in our title, and throughout the entire paper. It is very likely that assimilating NDII information into hydrological model will improve the prediction in ungauged basins. Particularly if the Su can be updated at the start of the wet season, then predictions of the first flood hydrographs could improve substantially. However, it is beyond the main scope of this study to do this now, and we will do it in our follow-up research. This will be clarified in the discussion.

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Besides that, the graphs showing the correlation do not all allow to plot this exponential function. Especially in the wet season, low values for root zone soil moisture corresponds with several values of the NDII. This shows that soil moisture is insensitive to the NDII. The second part of the wet season plots, for high NDII, show a big scatter. If the NDII is high, soil moisture could be any value. Fitting these exponential functions is therefore not appropriate here. My suggestion is to give the uncertainty in soil moisture prediction for a certain NDII value. From this it would immediately be clear that the annual and wet season correlations are not well correlated and could not be used to derive the root zone soil moisture content. This would also change the conclusion that NDII could be used to derive dry conditions in wet seasons since this is absolutely not clear from the graphs.

Reply: The reviewer is correct. The relation between NDII and Su in wet seasons has higher uncertainty than in dry seasons. As soon as there is no moisture stress in the root zone, then the moisture content in the leaves will not reflect any tension, and the correlation between root zone storage and NDII is lost.

The saturation effect on the NDII is discussed but could be elaborated more. The graphs in figure 6 should be used to indicate where saturation occurs. It is clearly visible that in the wet season the same NDII could have different values of root zone soil moisture, indicating that increasing soil moisture does not influence the NDII anymore. This part should be omitted from the dataset to make a meaningful function for lower soil moisture values. The way it is presented now, shows that an exponential function is used to glue two different functions, for high and low soil moisture, together. Also the study of Yilmaz et al. (2008) shows that the relationship between NDII and VWC changes under water stress conditions. Since VWC is related to the root zone soil moisture, it should also be taken into account here. The changing relationship cannot be presented by one correlation line in the graph.

Reply: The reviewer gave a very valuable suggestion. Within soil saturation period, increasing soil moisture does not influence the NDII anymore. We will omit these data

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points from the dataset to make more meaningful for lower soil moisture values. Once we separate those data points from the dataset, we may see more clear correlation.

What is also remarkable is that the values of NDII and Su on the axes do not differ that much for the annual, dry and wet seasons. If one looks at the three correlation graphs for one basin, the maximum soil moisture is somewhat higher for the wet season, but in general, the soil moisture conditions that occur in the dry season do also occur in the wet season. The division in dry and wet season is based on rainfall amounts but it is strange this this is not reflected in soil moisture storage values. A value of Su from 200 mm shows a much larger spread in NDII for the wet season than the dry season. This is also true for lower values and I do not think that saturation already plays a role here. I expect a more thorough discussion here between the dry and wet season and the possible reasons for the differences.

Reply: We classified the time series into wet and dry seasons. But there are still dry events within wet seasons, and wet events within dry seasons. In all situations, when the soil moisture drops under a certain threshold, the moisture stress will impact on the leaf water tension and hence change the NDII. While close to saturation, the graph becomes more scattered because water can be drawn from the root zone without much suction pressure. However, we shall more extensively discuss the differences between the dry and the wet season in the revised manuscript.

2.4 Lack of detail Another major point is the lack of detail in the whole paper. First, the choice for the model is not explained and also the reliability of the model to simulate root zone soil moisture is not discussed. This is quite essential for trusting the results of the paper and is not general knowledge since the model cannot be found easily on the internet.

Reply: The flexible modelling framework does not serve as a one-model-fits-all. In contrast, it serves as a powerful tool to understand hydrological processes. The model in this study is developed based on the flexible modeling framework. We chose the

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Xinanjiang curve to calculate the runoff generation, which is widely used in a large number of catchments all over the world and at global scale. A similar parametrization has been implemented in many hydrological models, i.e. Variable Infiltration Capacity (VIC) model, Probability Distributed Model (PDM), HBV and ARNO model. This is the core of the FLEXL model. Moreover, the reliability of FLEXL is also shown in the simulated hydrograph. In Gao et al., 2014, FLEXL is not as good as FLEXT in model transferability test. However, this does not mean the simulation from FLEXL is not reliable in calibration. We will interpret the model in details in the revised paper.

Second, the choice for the study area is not explained. As I already mentioned I do not understand why no observations are used. If the authors choose an area without observations, explain at least why this area was chosen. I get the impression that this area is chosen because of its clear seasonality with wet and dry seasons. This would improve the R<sup>2</sup> values, but this is no good reason to choose a study area. What is also missing in the area description is the type of vegetation. The type of vegetation determines the root zone moisture uptake and can therefore result in spatially different root zone soil moisture values (Jayawickreme et al., 2008). It would be interesting to see if NDII is able to see these differences.

Reply: The distinctive dry and wet seasons in the upper Ping River basin is one of the main reasons to choose these catchments. In addition, these catchments are all well covered by forest. And they are not influenced by snow and ice. Avoiding the influence of other factors, these catchments are ideal cases to concentrate on the relationship between Su and NDII. Furthermore, we will clarify the possible case specificity of these tropical forested catchments. We agree that more studies are required in catchments with different climate and land surfaces to further validate this finding. We shall be happy to do so in follow-up research. We agree that different vegetation will have different root zone moisture uptake and different root zone soil moisture values. But at catchment scale, it is probably difficult to take the details of this level into account. We will discuss it in the revised manuscript and carry out related research in follow-up

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studies.

Another example is the relationship between soil moisture and vegetation, which is very important in this research, but not explained. In the results one sentence is included about the suction pressure, which relates the vegetation water content to the soil moisture, but this should be made really clear in the introduction. What also misses is the derivation from canopy water content to vegetation water content. An allometric equation is needed to convert the canopy water content to the vegetation water content which includes also the stems of the vegetation. I suggest to describe this equation, or if this was not used, not to use vegetation water content but canopy water content instead. Next point is the use of NDII instead of NDVI. Wang et al. (2007) already studied the connection between NDVI and soil moisture. It is not clear what extra information the NDII could give or why the NDII is used instead of the NDVI. I suggest to explain this or compare the results with Wang et al. (2007) to see if NDII is a better indicator.

Reply: NDVI is connected to photosynthetic activity, and hence to transpiration. As a result, the NDVI could be a proxy for the moisture flux from the root zone (the transpiration). This is already done in many publications. The NDII, however, is a proxy for the moisture storage in the root zone. The combined use of NDVI and NDII is therefore very valuable to test the performance of hydrological models. There is nothing new about using the NDVI as a proxy for transpiration, but using the NDII for the moisture state is new. Of course, the flux and the stock are related. If there is a high moisture flux (transpiration), then there should also be sufficient soil moisture in the root zone. However, the relation between stock and flux are not linear (often a kinked line is used, with the inflection at a percentage of the root zone storage capacity, making it a threshold process), so they cannot be used interchangeably.

Going into the plant physiology, is beyond the intention of this paper. It would be very interesting to do so, but we fear that this is a subject for other researchers. We will use the term “vegetation water content”, which is closer to the meaning of NDII. We did test the relationship between Su and NDVI, it is not as good as NDII. This is for

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two reasons. Firstly, the relation between NDVI and Su is not linear, being a threshold process. Second, NDVI is affected by cloud cover during the wet season while NDII is not really affected. We will add results in the revised paper.

Furthermore the hypothesis is not clearly formulated and this makes it difficult to understand what the objectives and aim of this paper are. The aim indicated in the paper is to prove the effectiveness of the NDII as indicator for root zone soil moisture. As I said earlier this should not be proven only by showing seasonal correlation. A clearly formulated hypothesis would overcome this problem: Is NDII a better explanatory variable than the day of the year for explaining root zone soil moisture storage. However also this word 'storage' is questionable since the research is rather about the variability in root zone soil moisture. It is not clear if the authors are interested in the variability of root zone soil moisture over the year and spatially or how much the storage is itself. If this is stated both clear in the introduction it will help improving the clarity.

Reply: We will do the deseasonality in the analysis. We are more interested in the temporal variation of root zone storage (Su). This will be clarified.

Next to that it is also not clear on which timescales we are looking when explaining the relation between Su and NDII. On the short timescales, an increase in soil moisture causes an increase in evapotranspiration. This causes the EWT to go up and to measure a high NDII. At the same time the soil moisture will go down, because it is used by the roots. In this case a high NDII corresponds to a low soil moisture. This effect is nowhere explained and should be mentioned. A time lag of 10 to 5 days from Wang et al. (2007) show that this effect could be visible in the 8 day averaged NDII and Su values, if the vegetation would be modelled dynamically.

Reply: We will show the comparison results between Su and 8-days-later NDII. If it is not better than the original one, this will answer this question.

What is also missing is the explanation of landscape characteristics. These are shown in the figure 2 and mentioned in the conclusion, but is not taken into account in the

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analysis. Soil type, vegetation and topography determine the spatial distribution of soil moisture (Baroni et al., 2013). The occurrence of bare soil between vegetation can influence the NDII (Dasgupta & Qu, 2008). All these variables are interesting to compare with the spatial distributed maps of the NDII. It can then be discussed if the NDII is able to detect the spatial distribution according to the landscape characteristics.

Reply: We agree it is worthwhile to show the results of spatial NDII and landscapes. This study is focused on catchment scale, which combines all factors together. Decomposing the possible influence of topography, vegetation and soil type on NDII is beyond the main interest of this manuscript. It will be carried out in future research.

2.5 Structure and outline In several places, important information is missing. Also the outline of the paper is not clear at all. Results are not clearly results but also show interpretation. The discussion does not contain the shortages of the study and the use of results. I suggest to explain in the discussion if the NDII only works in the growing season (Yilmaz et al., 2008) and what other papers found about saturation. Next to that it is also important to discuss what would happen if the NDII detects bare soil and if the whole catchment or subbasin could be used to derive an averaged NDII value. Can the results of this paper be of use in the tau-omega model (Yilmaz et al., 2008)? Also a more elaborate study on the future where hypothesis for other climates and regions are given is needed and perspectives should be given on how the results could be implemented in hydrological model and what it would improve.

Reply: We will separate results clearly apart with discussion. Diverse land covers, including bare soil, will impacts on NDII. However, the NDII data at catchment scale has implicitly considered this diversity. Since large proportion of bare soil cover will draw down the catchment averaged NDII value. For the details of vegetation phenology impacts on NDII values, we will do more investigation in follow-up research. To our knowledge, all conceptual hydrological models considers catchment as a bucket, or several buckets, therefore our method has widely application to understand the characteristic of catchments.

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The introduction does not contain all the relevant information, mentioned above in lack of detail. The conclusion is more a summary of the research and does not give an in depth answer on the objectives and hypothesis (which are missing or vague formulated). Furthermore, the definitions of root zone soil moisture is changing in the paper which is confusing. Sometimes just soil moisture is used, then soil moisture storage or soil moisture deficit. Also the reservoir representing the root zone soil moisture in the model is confusing. The name of this reservoir is unsaturated zone, while the root zone is only part of this unsaturated zone (see figure 1). From this it is not clear if the dynamical part is explicitly modelled. I think it is only a definition issue, but it should be made clear what is modelled and where the NDII is assumed to show a correlation with. If the results would be compared to observations it should be clear what part of the soil is modelled. I suggest to rename the reservoir to root zone reservoir.

Reply: We will further clarify the objectives and hypothesis of this study in the introduction and conclusions. All terms will be used consistently. We will give a clear definition of  $S_u$ , and use the same definition as the root zone storage in the whole revised manuscript.

Lastly the title is not well chosen. The proxy for hydrological modelling is only discussed in the discussion part of the paper and is therefore not the main content of the paper. The paper does actually show that the NDII can be used as a drought indicator for soil moisture droughts, because it only reflects the root zone soil moisture content under water stress conditions. The amount of root zone soil moisture is not determined, whereas the variability over the seasons and over spatial scales is. Also, the hydrological model part in the title sounds very promising, but is not because of the use of models as methods in the paper, as explained by my first major point. The title should better reflect the actual contents of the research. An example is naming the study area in the title.

Reply: We may change the title to something like “The Normalized Difference Infrared Index (NDII) as a proxy for root zone storage”.

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Specific comments Reply: All the specific comments will be addressed fairly in the revised manuscript.

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