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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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1 Introduction This paper investigates a possible proxy for soil moisture in hydrological modelling. The authors have addressed the importance of soil moisture for hydrological modelling and identiïňĄed that the root zone is a very important dynamic in the unsaturated zone. One of the current challenges in hydrological modelling is deriving the soil moisture content. Remote sensing is a potential indirect way to derive the soil moisture content, since microwave remotely sensed data can be used to retrieve soil moisture (Jackson and Schmugge, 1991). Remote sensing indices usable for this purpose include the NDVI (using near infrared and red bands), NDWI and NDII

(both using short wave infrared and near infrared) which differ slightly in range of SWIR wavelength according to the terminology suggested by Ji et al., 2011. The researchers chose NDII, since it uses more relevant properties of the wavelengths, allowing it to directly detect water stress in plants (Steele-Dunne et al., 2012). It is known from previous research that the NDII is linearly related to the EWT (equivalent water thickness) in plants, which in turn has an allometric relation with VWC (vegetation water content) (Yilmaz et al., 2008a). NDII was calculated with MODIS imagery for 2001-2013 in the Upper Ping River Basin, Thailand. Rainfall and runoff data was also collected and used for a FLEXL model (HBV-like model) to calculate soil moisture. The NDII and soil moisture were plotted against each other to ïňĄnd a relation and identiïňĄed the R2 to verify how strong this relation was. The authors claimed that they found a signiïňĄcant relation between NDII and soil moisture.

Using remote sensing data for soil moisture is a very relevant topic nowadays, which the authors correctly recognize. The useful properties of wavelengths have been known for decades now (Jackson and Schmugge, 1991; Wagner et al., 2008) and the NDII has been used (under a different name, but with the same properties) since the 80's (Kimes et al., 1981; Hardisky et al., 1983). While NDII directly relates to plant water stress, it is indirectly linked to soil moisture, especially during droughts (Yilmaz et al., 2008a). Since the SMEX experiments, there have been many studies to link real soil moisture data to remotely sensed indices (Jackson et al., 2004; Yilmaz et al., 2008b) which makes now a better time than ever to combine knowledge about soil moisture and remotely sensed microwave data to apply this to hydrological modelling. Soil moisture is very important to hydrological modelling, as it has direct interactions with weather, climate, the biosphere and the unsaturated zone (Vereecken et al., 2008). Vereecken et al., 2008 also recognized that there were unexplored possibilities to apply remotely sensed microwave data to soil moisture determination. Therefore, this paper has the potential to be a pioneer in an interdisciplinary approach utilizing remotely sensed data for hydrological modelling which can be applied to a wide range of hydrological problems.

Reply: We appreciate a very nice literature background given by the reviewer. This is very helpful to improve the introduction part.

Even though I agree with the belief that NDII could serve as a good proxy for soil moisture in hydrological modelling, I believe there are still problems with the methodology which should be addressed before the writers can really make such claim. These problems include the absence of a threshold to evaluate the performance of the proxy, incorrect reasoning behind the relation between NDII and soil moisture, no motivation on why MODIS and FLEXL were used and the seasonal trend in data has inïňĆuenced the coefïňĄcient of determination (R2). Moreover, the writers do not give any overview or recommendation on using NDII as a soil moisture proxy in practice.

Reply: We do believe that the simulated Su, representing the root zone storage, can be used to compare with the NDII. We prefer to call Su the root zone storage instead of the soil moisture. To avoid any confusion, we may change the title to be something like "The Normalized Difference Infrared Index (NDII) as a proxy for root zone storage".

The most important improvements I would like to see in this paper are: a better connection between NDII and soil moisture, a more critical look at the methods and a recommendation about how other researchers could use NDII as a proxy. Therefore I would advise major revisions by the authors to this paper before being accepted.

2 ScientiïňĄc points:

In general, it would have been better if the authors had a hypothesis about the relationship. Before conducting this study they must have had an idea about how NDII and soil moisture were related. Moreover, there were requirements set up beforehand (for example an R2 of at least 0.8) the authors would have a concrete limit for which the relation would be accepted or discarded.

Reply: We hesitate to compare the NDII with observed soil moisture. The reasons were already given in the answers for M. Van Tiel (in the section 2.1: Use of model vs.

observations).

We will set a roughly lower limit of R2 value that would be accepted or discarded in the revised manuscript.

It strikes me as odd to the use of a proxy for a natural process, without actually measuring the process. This can only be justiïňĄed if the model is well-veriïňĄed. The model is a HBV model called FLEXL, which is a lumped model. Gao et al., 2014 evaluates the model used for this study, along with other types of FLEX models (FLEXD, a distributed model and FLEXT and FLEX(TO) topographical) in which the performance of FLEXT and FLEX(TO) was better than FLEXL and FLEXD . The latter both overestimated runoff due to underestimation of evaporation, which is a common problem of HBV models (Fenicia et al., 2011). FLEXD did not add anything to the lumped model, unless the area is rather heterogeneous. FLEXL therefore does not sound like a good model to verify the data in this study. I think the authors should consider the variation in land use in the area to assess the heterogeneity. I believe it is essential that the choice for using a model and the veriïňĄcation of the model are motivated in the revisions.

Reply: This question has been addressed in our reply to M. Van Tiel. We would like to repeat the answers as follows. "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 lumped model. The point is base flow in the dry seasons is contributed from groundwater reservoir, and almost independent of root zone. The better performance of FLEXT than FLEXL in baseflow simulation is because it considers the interaction between groundwater reservoir and wetland, 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 FLEXL model was developed based on the variable contribution area theory, which has been tested and widely used in tremendous catchments all over the world.“

Initially connecting NDII to soil moisture sounds like a safe and useful assumption. However when looking closer at the introduction I believe that the relation between these two is not explained thorough enough in this paper. Yilmaz et al., 2008a (which the writers also cited) states there is a linear relationship between NDII and equivalent water thickness (EWT). EWT is a measure of foliar water volume per leaf area, so purely water stored in plants dependent on the surface area of the leaves. The total amount of water is the vegetation water content (VWC), of which EWT is only a fraction. The size of this fraction depends on the plants themselves. You were correct to say VWC and EWT have an algometric relationship, however land-cover classiïňĄcation is recommended to ïňĄnd an appropriate algometric relationship (as stated in the conclusion of Yilmaz et al., 2008a). I feel like the inïňĄltration, and thus the root-zone hydrology, in this area is spatially variable, as seen in Sharma et al., 2007. The authors should evaluate if either the area has homogeneous vegetation or what the error is by neglecting the role of land-use.

Reply: We compared the catchment averaged NDII with the root zone storage obtained by a lumped hydrological model. Catchment heterogeneity such as land use is included neither in NDII nor in the lumped hydrological model, which is a fair comparison at our interested catchment scale. Further investigating catchment heterogeneous impacts on NDII and Su will be conducted in follow-up research.

I agree with the writers that MODIS provides a good source of remotely sensed data for NDII calculations, but I would like to see motivation for choosing MODIS. Hunt et al., 2011 used MODIS imagery for an NDII study and discussed what makes MODIS imagery good for this purpose. I would recommend the authors to use this reference in your motivation.

Reply: This reference is a very good source of information. We will use it in our revised manuscript.

I believe the wet season data has no usable trend, unlike what the authors claim. The data in the wet season is heavily scattered except for the dry spells. It seems that if the driest data from the wet season is removed, there would be no trend in the remaining data. Besides identifying how well the relation is using only R2 you could add information about standard deviation and/or certainty intervals (and perhaps include the interval into the scatter plot to visualize the certainty). When looking at the scatterplots, a certain NDII can still result in many different soil moisture content values. I think the authors need to prove the signiïňĄcance of the results, especially in the wet season.

Furthermore, the model performance is in general explained poorly. The ïňĄgures in the research only identiïňĄed the seasonal trend in your data. Identifying R2 in a dataset dominated by seasonality will generally yield high R2 but does not predict the explain variation in the data. Usually hydrological modelling includes the removal of seasonality in data (SchaeïňĆi and Gupta, 2007). Therefore I would like the authors to either remove the seasonality to prove the signiïňĄcance, or justify why they have not removed the seasonality.

Reply: We will remove the impacts of seasonality from the time series of NDII and Su. The results will be presented in our revised manuscript.

The last general remark I want to make is that the title suggests that the paper provides an actual use for using NDII as a proxy, however the actual use of NDII as a proxy is completely left out from the paper. Even though the authors identiïňĄed trends to relate NDII to the soil moisture, they need to elaborate on how NDII can be used as a proxy. This recommendation should at least include if the relation from this study is applicable to other areas, or how other researchers could derive a usable relationship between NDII and soil moisture content.

Reply: It is a good idea to assimilate the NDII information into hydrological model to increase model prediction. But it is to some extent beyond the main scope of this study. This study provides a strong evidence to support the relationship between NDII and Su, especially in dry seasons. And we discussed why the vegetation is a good indicator for soil moisture, rather than an obstacle. Assimilating this information into hydrological model will be conducted in further research. We will provide further information on applying the NDII as a proxy for the root zone storage in our revised manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 8419, 2015.