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Interactive Comment

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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Note for the authors and editor

The following review was written by a student of the MSc programme Earth and Environment at Wageningen University. As part of the course Integrated Topics in Earth and Environment, students are asked to prepare a review of a scientific paper. The supervisor of this review process is Ryan Teuling. The manuscript by Sriwongsitanon et al. was one of the manuscripts that was selected for this exercise. The review is written as an official review in order to comply with the course guidelines, but it should be considered by the authors as a regular comment which they can use to improve the manuscript. I hope that this comment will positively contribute to the review process and that it will help the authors to improve their manuscript for possible publication in HESS.

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 identified 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 FLEX^L model (HBV-like model) to calculate soil moisture. The NDII and soil moisture were plotted against each other to find a relation and identified the R^2 to verify how strong this relation was. The authors claimed that they found a significant relation between NDII and soil moisture.

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Using remote sensing data for soil moisture is a very relevant topic nowadays, which the authors correctly recognize. The useful properties of wavelengths has 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.

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 FLEX^L were used and the seasonal trend in data has influenced the coefficient of determination (R^2). Moreover, the writers do not give any overview or recommendation on using NDII as a soil moisture proxy in practice.

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 Scientific 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 R^2 of at least 0.8) the authors would have a concrete limit for which the relation would be accepted or discarded.

It strikes me as odd to the use of a proxy for a natural process, without actually measuring the process. This can only be justified if the model is well-verified. The model is a HBV model called FLEX^L , which is a lumped model. Gao et al., 2014 evaluates the model used for this study, along with other types of FLEX models (FLEX^D , a distributed model and FLEX^T and FLEX$^(TO)$ topographical) in which the performance of FLEX^T and FLEX$^(TO)$ was better than FLEX^L and FLEX^D . The latter both overestimated runoff due to underestimation of evaporation, which is a common problem of HBV models (Fenicia et al., 2011). FLEX^D did not add anything to the lumped model, unless the area is rather heterogeneous. FLEX^L 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 verification of the model are motivated in the revisions

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

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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 classification is recommended to find an appropriate algometric relationship (as stated in the conclusion of Yilmaz et al., 2008a). I feel like the infiltration, 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.

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.

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 R^2 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 significance of the results, especially in the wet season.

Furthermore, the model performance is in general explained poorly. The figures in the research only identified the seasonal trend in your data. Identifying R^2 in a dataset dominated by seasonality will generally yield high R^2 but does not predict the explain variation in the data. Usually hydrological modelling includes the removal of seasonality in data. (Schaefli and Gupta, 2007). Therefore I would like the authors to either remove the seasonality to prove the significance, or justify why they have not removed the

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seasonality. 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 identified 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.

3 Specific remarks

Page 8421, line 14: Source seems outdated, a more recent source would be relevant since remote sensing and hydrological modelling develop a lot throughout the years.

Page 8423, line 18: Avoid using different units for wavelengths. Wavelengths are introduced in nm, but on page 8424, line 9 (eq. 1) and beyond, μm is used. It would be nicer to read if all wavelengths were listed in the same unit.

Page 8424, line 14: “and this” can be replaced by “which”.

Page 8425, line 24: Adding a sentence to introduce the list would make the text easier to read.

Page 8425, line 3: This sentence is also hard to read, consider a different construction.

Page 8426, line 16: By the end of the list, I did not know what the sentence was about. I would suggest a different construction or ending the sentence after the list and continuing line 19 with something like “These efficiencies are used to evaluate (. . .)”.

Page 8428, line 19: Write numbers as numbers, only low numbers should be written as words. Especially since you write “23” later on, forty-two should be 42.

Page 8430, line 3: Interpretations should not be part of the results. Results should be

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limited mostly to facts.

Page 8430, line 16: “do not normally occur in the same year” is too much interpretation, it would be better phrased as “did not occur within our measurement period”.

Page 8431, section 4.3: This section could be more concise in general.

Page 8432, line 15: This sentence sounds like the NDII/S_u pattern in dry spells is unexpected or special due to the words “However, even”. I would advise rephrasing this.

Page 8434, line 3: typing mistake in source: (Source].

Page 8434, line 6: Source from 2006 is described as “until now” however, the paper is nearly 10 years old by now. Try to find more recent source on this.

Page 8435, section 6: Conclusions may be made stronger. Currently there is still some repetition of the results, whereas more interpretation would be encouraged.

Multiple tables and figures are rather small and hard to read, these include: table 3, table 4, figure 2, and figure 5 to 8. I hope these will be enlarged for the final version.

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