

***Interactive comment on “Unsupervised  
classification of saturated areas using a time  
series of remotely sensed images” by  
D. A. DeAlwis et al.***

**Anonymous Referee #1**

Received and published: 22 July 2007

- 1) Does the paper address relevant scientific questions within the scope of HESS?  
YES
- 2) Does the paper present novel concepts, ideas, tools, or data? YES
- 3) Are substantial conclusions reached? YES
- 4) Are the scientific methods and assumptions valid and clearly outlined? YES
- 5) Are the results sufficient to support the interpretations and conclusions? YES
- 6) Is the description of experiments and calculations sufficiently complete and precise  
to allow their reproduction by fellow scientists (traceability of results)? Probably NOT

7) Do the authors give proper credit to related work and clearly indicate their own new/original contribution? YES

8) Does the title clearly reflect the contents of the paper? YES

9) Does the abstract provide a concise and complete summary? NO

10) Is the overall presentation well structured and clear? YES

11) Is the language fluent and precise? NO

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Not always

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, see comments below.

14) Are the number and quality of references appropriate? Yes

15) Is the amount and quality of supplementary material appropriate? n/a

#### General comments

The paper presents a new method to map wet areas in the Catskills area of New York using the Normalized Difference Wetness Index (NDWI). NDWI has been used before but, besides the general sensitivity of Landsat Band 5 to water, it is not so clear why it works, or why it would work better than other band combinations. Here, also, NDWI seems to provide information on the wetness distribution within the Townbrook watershed. It would have been nice if the authors had somehow tried to see if other combinations would have given better/other results.

The paper suffers to some extent from the same problem that many modeling-remote sensing comparisons have. The conclusion is always that there seems to be a good correspondence between the satellite images and the modeled area but one does not know why the errors are where they are, if there are structural errors, to what extent

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parameters have been tuned to provide similar patterns (in this case moisture class limits and number of NDWI classes). One can not blame the authors for that; it is just a general problem with this type of studies. Fortunately, here one has tried to come to some quantification and the result (78% accuracy) does look good. Unfortunately, the same does not seem to hold for the comparison with actual field data. It is clear that no one-on-one comparison is possible but the careful quantification of the differences between models and NDWI is not repeated for the differences between field observations and NDWI. In the text, an accuracy of 75% is claimed for the latter, but this does not seem to be substantiated by Figure 6. Perhaps this can be clarified.

The applicability elsewhere of the presented method in its exact present form is probably limited. The paper, would, however, encourage researchers to see what a NDWI time series can do for their area, and as such the article should be published.

#### Language comments

The language is rather sloppy and the text should be carefully edited. There are a number of near-homophones that the spellchecker does not catch (sighting/siting, transact/transect, ...). More sloppy language examples are given below under "specific comments". I do not insist on perfect English but after a while one simply gets the impression that the author does not worry too much. Then, perhaps, the sloppy language reflects sloppy research organization, which would then in turn start to reflect poorly on the whole effort. For example, the text states that, according to Table 3, NDWI wet underestimates the saturated area, whereas, in Table 3, it actually overestimates. Are the column headings swapped or is the text wrong? Another example, in the text it says: "In vegetated areas, absorption by leaf water occurs in the SWIR and the reflectance from plants thereby is negatively related to the leaf water content (Bowman, 1989; [...])" Then in the references, Bowman is given as: "Bowman, R. A.: A sequential extraction procedure with concentrated sulfuric acid and a dilute base for soil organic phosphorus, Soil Sci. Soc. Am. J., 53, 362-366, 1989." I admit that I did not run to the library to see if that article by Bowman does say something about SWIR reflection but

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it probably is a wrong quote. I surely did not check all references, this one just caught the eye, but such errors may give rise to further suspicion, which would be a pity (and unnecessary).

### Specific comments

The abstract repeats to a large extent the justification of the study, not the content. Please change that because the abstract is often the only part researchers see in a database. Those looking for saturated areas are already convinced of their importance but would like to know what can be found in the paper.

Some remarks about the text on the first few pages (I am no editor/native English speaker so I stopped): p 1664, 3: "sighting" should be "siting". p 1664, 23: Is it really "Information" that is an important parameter? p 1664, 24: Split infinitive. p 1665, 13-14: This conclusion does not follow from the text above, better omit. (The word "thus" could, in general, be removed quite a number of times.) p 1665, 23-24: "knowledge" must be "known"? etc.

p 1678: There seems to be some confusion about what is overestimated. According to the table, the NDWI overestimates but according to text NDWI underestimates, and an explanation for this is given. Please fix this. In addition, it may not hurt to define "producer" and "user" accuracies (in remote sensing literature, these are often referred to as errors of omission and commission).

The comparison between measured profiles and model outcomes, including Figure 7, does not seem to be useful here. Model validation is not the objective here and even if it serves to increase the trust in the models, we can not really use that information to value the NDWI product.

In the discussion, it would be worth mentioning that Landsat 7 images are no longer being acquired. Alternatives may be mentioned such as Landsat 5 (expensive), Aster, IRS (?).

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Figure 6 is not very clear: It seems the wet NDWI part is much larger than the mapped saturated area. What one would like to see is where NDWI and GPS overlap and where they do not (both omission and commission).

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 1663, 2007.

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