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Hydrology and Earth System Sciences Discussions

HESSD

4, S756–S759, 2007

Interactive Comment

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

Anonymous Referee #3

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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 (in general, see comments)

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)? YES (considering

the limited space for technical issues).

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? YES

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

11) Is the language fluent and precise? YES

12) Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? YES

13) Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? SEE COMMENTS

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

15) Is the amount and quality of supplementary material appropriate? N/A

Comments: I've enjoyed reading this interesting article. I provide some comments for discussion that can also be used by the authors to improve their article:

1. The (general) assumption that atmospheric water vapor is homogeneous over relatively small areas and on clear days could be wrong, particularly in areas with abrupt topography in which valleys have more atmospheric water content. In the future, the authors could check if the estimated field of path radiance has any trace of spatial structure that could be related to terrain patterns.

2. I do not like using the NDVI courses to derive landcover maps at such a detailed scale in general, because NDVI phenology is often not enough information to discriminate among some landcover types. Nevertheless, it could be appropriate in this particular case, as the landcover types are very distinct. Unfortunately, the authors do not

mention the classification method they use (are they using supervised classification in this case?) and, most important, they do not validate their landcover map. Also, how sensitive are the results (detected HAAs) to errors in the landcover map? If they are sensitive, devoting more effort to produce a better landcover map in the future would make sense.

3. I think that the statement in p. 1667 “However, the clustering portion of unsupervised classification operates without a priori information of the wetness index classification and groups samples based on the inherent similarity of individual spectral signals” is not correct. If the authors were really using the spectral signals, they would be including SWIR and NIR reflectance in the classification along with the rest of bands, and thus both classifications would not have been independent. Maybe the authors meant “of individual NDVI time courses”?

4. Including the contingency table for SMDR wet and VSLF wet in table 3 would be a good idea: do both simulations agree more between them than with NDWI wet? Also, although the authors are right at stating that the disagreements can be abstracted (not sure this is the correct term here) from the user’s accuracy, the reader would prefer the authors just adding a column to the tables.

5. I understand that field validation is more difficult and I’m not a field hydrologist, but I think that you could make a better analysis of the field data you have to validate the NDWI wet results. At the end you are paying a disproportionate attention to the simulation results. Figure 6 is insufficient, why not providing a contingency table equivalent to Table 3?

6. I miss a spatial analysis of the disagreements between the different methods (at this point, we cannot speak of errors because the field data are scarce), which could provide interesting clues on why the disagreements are where they are and, with fortune, about their nature. Perhaps the authors could consider a deeper validation and error analysis for a future article?

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

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