

Interactive comment on “Land classification based on hydrological landscape units” by S. Gharari et al.

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Reply to Comment by A. D. Nobre

Reply to "Review"

We really appreciate the critical comment on our work which we shall use to improve our work. The reviewer comments were addressed in detail as follows:

1- There is a difference between "drain", as in local drain direction of a DEM cell and "drainage" which is confused throughout the DP. The HAND

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acronym stands for Height Above the Nearest "Drainage"... Nobre et al. (2011) define accurately the use of the HAND term, please refer to that for proper use and stick to the original definition.

Indeed, we shall stick to the original terminology for HAND as mentioned by Rennó et al. (2008); HAND is "Height Above Nearest Drainage" .

2- Distance to the nearest drain is a very ambiguous denomination . . .

This is a good point. By distance we mean the horizontal distance; we shall clarify this term in the final version of the paper.

3- The uncertainty analysis for class inclusion, with the added fuzzy approach, has the merit of ascertaining the validity of the field verified classes. However, the HAND model has a deterministic nature, so it is always good to keep it in perspective when applying statistics.

The author mentioned that the HAND has a deterministic nature, which is indeed true. Height Above Nearest Drainage is deterministic and can be determined by the HAND algorithm (Nobre et al., 2011). But the classes which are classified through HAND cannot be considered as deterministic, which is our approach in the paper. We believe that it does not seem realistic that the soil and vegetation type and assigned hydrological features change abruptly above and below a specified threshold throughout a catchment. But it seems more logical that the hydrological behavior changes gradually from one dominant landscape to another. This is the idea behind the fuzzy approach.

Indeed, as the reviewer mentioned in section 3.3.3 of his paper (Nobre et al., 2011) different observed points may be classified differently within a DEM cell. This may be because of the difference between the observation resolution and DEM resolution, as mentioned in Nobre et al. (2011) paper. The question that arises is how to classify a DEM cell in such a situation. Is it because of errors in the data or in the classification

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model? Or is it really because there might be different classes within a cell of DEM, in particular at coarser resolutions? We believe that the second assumption is more logical and realistic.

4- It is important and useful to separate citation of the HAND papers. Rennó et al. (2008) was aimed to the remote sensing community ... On the other hand, Nobre et al. (2011) was aimed to the hydrological community... The data for landscape classification presented in both Rennó et al. (2008) and Nobre et al. (2011) is one and the same, so a much more informative and useful referral to the data is Nobre et al. (2011). Although Rennó et al. (2008) is a better citation for the algorithm itself, with its mathematical formalization, it is not the best citation for the data nor to the HAND-based landscape classification. . .

Indeed we cited Rennó et al. (2008) for the HAND algorithm. The algorithm is very well explained in Nobre et al. (2011) and there it is also formally introduced. However, the paper of Rennó et al. (2008) was the basis for our work, since we became aware of Nobre et al. (2011) only when we had already completed our analyses. However, we also refer to Nobre et al. (2011) for its application into hydrology, as we already did. The link to Nobre et al. (2011), which was published 29 of June 2011, was provided by the reviewer himself.

5- It is no surprise that the results of HAND and slope are best parameters to describe landscape units with hydrologically similar properties, as HAND and slope capture underlying physical conditions which are deterministic drivers of soil-water dynamics, thus also of terrain effects. All the more strange then that the DP has not dealt with the deterministic physical side, as explored and explained in Nobre et al. (2011).

The reviewer wrote that it didn't surprise him that HAND and slope work better than any other model. We argue that, given the conceptual nature of the problem, it is

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difficult if not impossible to a-priori state which model would work best for our problem, in the absence of any posterior scrutiny. This is the main rationale behind the testing of alternative model structures.

Indeed, figure 19 of Rennó et al. (2008) showed a relation between distance and HAND, which indicated that the relationship between distance and HAND may be important and should be investigated, which was done in the DP.

In section 5.3 of Nobre et al. (2011) the authors mentioned some review of past work which indeed showed a correlation between distance and catchment characteristics. This is another indication that is important to investigate which of the two parameters are better indicators of desired landscape classification.

6- Although the calibration of classes is interesting and perhaps generally useful, in its inception there appears to be no regard to the underlying deterministic nature of the phenomena that generates landscape classes with hydrological significance. Once again it appears to me as fundamental that the DP considers this, as pointed out in Nobre et al. (2011).

As we elaborated earlier, we believe that the perception that nature is deterministic does not contrast with the fact that a conceptual classification may be fuzzy. Not only perception of classes such as hillslope and plateau are as subjective as the definition of hot and cold, but the variability of hydrological processes and the conceptual nature of the problem can be well assimilated by the fuzzy approach. The main idea behind our paper is that the three landscape classes: plateau, hillslope, and wetland, represent three dominant runoff generating mechanisms: deep percolation, rapid subsurface flow (storage excess subsurface flow) and saturation excess overland flow. This is the beauty of HAND, that together with slope this is such a powerful landscape indicator. We have used the terms plateau, hillslope and wetland because these are names that can directly be associated with the landscape type. In principle we could use other names, but as long as the definition of our terms is clear, this is not so

important.

7- Problem with English explanation of wetland or waterlogged.

For us, the term wetland makes it immediately clear that groundwater is close to the surface. The term wetland is clear to a wide variety of professionals and can be easily associated with its hydrological function.

8- The DEM resolution analysis is by far the most innovative and valuable part of this DP. The well-crafted definition of a best-suited DEM resolution for a hydrologically accurate HAND-based classification of landscape terrain is in my view the strongest result here. This finding is of great significance, especially for hydrological modeling, but not only. As the HAND model and landscape terrain classification based on it spread, knowing what is the best DEM resolution to assess terrain-relevant classes becomes of crucial importance.

We thank the reviewer for this positive comment. We are aware that this study was based on in situ visual observation and not based on measured data. It would be nice if in the future this work could be repeated with hard data, like piezometers measurements as used by Nobre et al. (2011).

9- I still would have other issues to discuss, like the connection of rainfall/runoff concept proposed for class definition with HAND defined ground-water properties for example. But these only after there is action for the issue below.

We would be happy to hear the reviewer's opinion on the rainfall/runoff conceptualization, after the paper revision.

Apart from the fact that this is not the general objective of the DP, we also would like to stress that the dominant runoff processes referred to in the DP (and in Savenije (2010)) were conceptualized for the Wark Catchment in Luxembourg and the conceptualization

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may be completely different in other environment or climate regime.

Once again we would like to thank your critical comments on our paper.

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