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Hydrology and Earth System Sciences Discussions

Interactive comment on "Land classification based on hydrological landscape units" *by* S. Gharari et al.

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Review

The Discussion Paper (DP) (Gharari et al, 2011) reports on the application and validation of a HAND-based landscape classification to a temperate medium sized catchment in Europe. The DP is about an observational study, that is, it shows a statistical analysis of an observational dataset of thousand of field verification points vis a vis a very high resolution DEM. It also brings into the analysis the HAND model and other

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Interactive Discussion

Discussion Paper

descriptive topographical parameters. The DP aims to define, or help better define, landscape classes of terrain types, according to their hydrological or topographical nature, targeting to the generation of a useful topography-based framework for hydrological modeling. However, the DP is not about a new landscape classification based on hydrological units, as it does not offer any effectively new landscape classification framework.

This DP suffers from a generalized problem of authorship and crediting-of-previous-work, which casts shadows over an otherwise interesting and valuable independent validation of a HAND-based landscape classification, added of original analysis of scale and other values. Therefore, the review analysis of the DP merits and substance will, at this time, be shorter than it deserves.

Itemized Review

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 acronym stands for Height Above the Nearest “Drainage”, drainage meaning here that superficial runoff belonging to the perennial “drainage network”, that is, all streams, rivers and superficial water bodies. 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.

2. Distance to the nearest drain is a very ambiguous denomination, as distance can assume any vector direction (horizontal, vertical, hypotenuse or any other angle). That is why the HAND acronym stands for Height, because it is unambiguously the vertical distance, or the distance along the normal to the tangent plane touching the geoid surface. The expression needs to be better defined,

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maybe as horizontal distance, if it computes similarly to HAND but in right angle with it.

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.

4. It is important and useful to separate citation of the HAND papers. Rennó et al (2008) was aimed to the remote sensing community, it presents the HAND computational tool (or algorithm), and a preliminary landscape classification (for the Asu catchment only). On the other hand, Nobre et al (2011) was aimed to the hydrological community, presenting and formally introducing the HAND model, its physical fundaments and a complete HAND-based landscape classification, with definitions of terrain classes, their calibration and validation. 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. Authors should always strive to cite properly, saving time to the reader by accurately pointing to the best sources.

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).

6. Although the calibration of classes is interesting and perhaps generally useful, in

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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).

7. I have an issue with the English language when it comes to nominations and terms (why are we obliged to use inaccurate English terms to define terrains, when there might be much better denominations in other languages?). However, English is a *de facto* language of science, so one must try to be consistent and seek the best logical support for the term used. Wetland obviously is land that is wet, which basically says **nothing** of its innate hydrological nature besides that there might be moisture there. But moisture is everywhere. When we developed the terrain terminology employed in the HAND landscape classification (*water-logged* for example), we thought that *water logging* was a pretty specific process of water pooling in the upper soil porous media, but not forming any significant layer of free water on top of the mineral soil [which would spell a class of terrain making up the flood lands]. That is why we stressed the focus on the "stationarity" of soil water in the porous media as the criteria for defining terrains in the HAND classification scheme (see Nobre et al, 2011). The DP does not give attention to this. Although I understand the hydrologist fretting about "soil-water high frequency fluctuations", it is of essence if one considers the HAND-landscape classes, "to squint the eye a bit", so that detail will not rob attention from the crucial long-term soil-water behavior, more important in the evolution of terrains.

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

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assess terrain-relevant classes becomes of crucial importance.

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.

Authorship and Credits

One of the most fundamental and useful basis of the scientific method is the paradigm of incremental accumulation of knowledge. But accumulation of knowledge is only effective if every step on the ladder of inquiry is objectively used to move forward. That is why every prospective author should search the literature for what is available in relation to his (her) subject, taking all measures to adequately cite, use and credit previous work. Proper and fair crediting of previous work builds trust and incentivizes communities to cooperate. Mis-attribution of intellectual authorship on the other hand can have unwanted bad effects.

To help conceptualize the problem of this DP, let's call the original HAND authors Group A and the authors of the present DP as Group B.

Group A comes up with an innovative idea on landscape classification, based on normalization of topography according to local soil-water environments – the Height Above the Nearest Drainage – HAND; then pursues that idea for 10 years, developing the concepts and the theory, building a computational tool, applying and validating it for large areas and communicating all in publications at scientific journals [Nobre, 2011, Rennó, 2008].

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At a later date, group B, picks up the computational tool and, employing the ideas put forth by Group A, applies them to another region, in another continent, comparing performance in landscape classification. The comparison, among other things, aggregates analysis of scale and has considerable merits. However, Group B presents a manuscript with their comparative findings serving as a basis to claim the introduction of a *new* system of landscape classification based on HAND. In this system they define hydrological units that match very closely (if not identically) the terrain types defined and validated earlier by Group A. Further on, Group B credits the inspiration for their landscape classification system to one of his co-authors, citing his conceptual paper published earlier, said paper also axially based on the original ideas put forth by Group A.

Group B does cite the papers of Group A, for the HAND metric and for some ecological validation; but the citation in this case is almost incidental and does not credit and recognize Group A as being the originators of the term HAND and its use in terrain classification based on landscape normalization. Throughout the manuscript the citing of Group A papers is widely absent, even though Group A papers carry the *first* and *only* available validation data about landscape classification based on normalized terrain.

The HAND model validation should be in the central objective as it is central in the abstract, throughout the study and in the main conclusion of the analysis reported. The DP is mostly "about" the validation of the HAND model, and yet the title does not reflect that content. HAND, the term coined by the HAND authors, is mentioned in the abstract without citation. If the reader has never heard of the term HAND, then the impression gained on reading the abstract alone is that Gharari et al were the discoverers of its importance. The citations to the HAND papers come in secondary, almost

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accessory position in the introduction. The core discovery in the basis of the HAND model is not mentioned in the introduction, namely that normalization of topography by the drainage-network solves the challenge of landscape classification with high hydrological relevance.

Moreover, the DP authors chose neither to credit nor even to use the theoretical physical foundations of HAND, presented and analyzed in detail in Nobre et al (2011). If someone wants to replicate the HAND-based classification described in the DP, that person will be left with very little in this DP to understand what HAND is about. And if he(she) searches out the original HAND papers, he(she) will discover that a HAND-based landscape classification had already been proposed, done and validated previously. In fact, 18 thousand square kilometers have been mapped and verified using a calibrated and validated HAND-based landscape classification (Nobre et al, 2011), a classification that in essence is not different from that the DP claim to have developed.

Pretention of authorship on a landscape classification scheme that is already published, conjugated with a systematic denial of necessary credits to previous works pose questions about a bias in intellectual authorship.

The DP cannot pretend to have built a HAND-based landscape classification from scratch as it does now. The work effectively done and reported in the DP is about validation of an existing landscape classification method (HAND + Slope) [proposed and demonstrated by Nobre et al. (2011) and Rennó et al. (2008)], and exploration of other related metrics for landscape classification (slope + horizontal distance and HAND + distance). In summary, most of the interesting and valuable work reported in the DP builds on top of the work of others. Really original in this DP is only the scale

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analysis.

Even though this referee has further critical contributions to make to the analysis and discussion of this DP, the problematic instances in this regard are too serious, and require action. Spending time in further detailed review can only be justified if the DP is fully re-written to remove authorship bias, to fit the claims to the work effectively done and to properly credit previous work. In the best tradition of science, discoveries should be effectively used to prevent dissipation of energy in re-discovering what is already known. I've added two documents in the discussion as support for the re-write. Given the content of the DP, the new manuscript title should include the words "HAND" and "Validation". Also the Abstract should inform what HAND is. It is indisputable that Nobre et al (2011) and Rennó et al (2008) papers were the first in the public domain to coin and use the term HAND and to use HAND and slope to define landscape units of hydrological (not just ecological) significance. This should be clearly acknowledged - ideally with a citation in the abstract. Failing that - right at the beginning of the introduction. In the Introduction a full credited description of HAND concept (not only as metric), model and classification is in order. Explicit mention to earlier HAND landscape classification and its results (Nobre et al, 2011) is basic as well. And throughout the new manuscript, every mention of work already done must be credited. In the discussion there should also be intercomparison between Gahari et al results and the results described in Nobre et al (2011), specially considering the very different environments.

To wrap it up, the re-write of Gahari et al must give recognition to the HAND authors, and then build on that, showing that the HAND landscape classification works in Europe too, advancing through the apt comparison with other metrics, highlighting the very interesting scale analysis. The new approach could also point to and further explore in the discussion possible improvements in the HAND-based landscape classification,

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useful to hydrological models, the subject of the HESS special issue. Given the confirmation of the HAND + slope as the best terrain type descriptors, putting this finding in the context of the use by hydrological models could be further developed. And here again, the deterministic nature of HAND can represent a great advance for modeling (as discussed in Nobre et al, 2011).

Conclusion

Overall the analysis presented in this DP is good, the work has many merits that certainly deserve publication in HESS. However, due to the problem of authorship and credits exposed above, as well as for a missing deeper analysis of the physical fundaments of HAND as applied to this catchment, in its present form this DP should not be considered for publication. I am looking forward to see this work again in a new manuscript.

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