Short Comment on Paper HESSd-8-4927-2011

Interactions between pedology and hydrology: more shadows than lights in this paper
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Terribile et al. (2011) [hereafter refereed to as TCLMB] have developed arguments for stimulating effective interdisciplinary interactions between pedology and hydrology. They also presented four different case-studies to evaluate the potential and limitations of using pedological information embedded in soil maps for hydrologic applications at landscape scale. This paper already received comments from Johan Bouma (one of the referees), who recognized the good works that some Italian groups are carrying out on the topic, but also emphasized some weak points of the present paper. We would take this occasion to thank Johan for his appreciation towards the Italian works done in this realm of scientific research.

We commend TCLMB for discussing the valuable but sometimes controversial (e.g. Kutilek and Nielsen, 2007) issue of searching the way of how (… and to what extent) pedology can make its contribution to hydrology, allowing also for the important question of scale.

Apart from those highlighted by Bouma, it seems that some other statements require clarification.

The following sentences are an example (P.4929): "*Most authors and scientists select modeling strategies and components of simulation based on intuitive basis, or without any explicit explanation at all. This leads to a fundamental non-reproducibility of results since the most fundamental aspects of the modelling strategy are hidden.*". Instead, we believe that now more than ever before, hydrologic models are fairly well presented and explained, and the editors and referees are very conscious about the fact that the basic concepts of reproducibility should apply. Moreover, most models are now freely available and downloadable from internet, and this definitely increases their usage, finding of bugs, dissemination of results obtained in different situations and environments, and the exchange of experiences.

In the introductory section TCLMB says: "*The importance of soils further increases when hydrological monitoring data are lacking, such as when hydrological predictions are required in ungauged basins.*", but perhaps an explanation of why is desirable. Other attributes contribute to a reliable prediction of the hydrologic response of an ungauged site and identification of the factors controlling parameter variability requires the setting-up of suitable regionalization strategies and/or effective catchment classifications techniques.

At P.4941, L.16-21, it is written that: "*the analysis of water balance, using bucket-based models, might induce one to assume that a soil db provides high quality data for hydrological applications. Unfortunately this is not always the case because, for example, the AWC (Available Water Capacity, the reference water storage in the rhizosphere) is calculated on the basis of particle size classes by means of a PTF and not through direct measurement.*". We disagree with this statement, as hydrologists have learned that AWC should be at least retrieved from the knowledge of soil hydraulic properties (Meyer and Gee, 1999; Minasny and McBratney, 2003), if one would not or cannot make direct measurements (Romano and Santini, 2002).

Spatial variability issues.

Spatial variability of soil properties is a recurrent issue in this paper. Firstly, it seems that TCLMB emphasizes a bit more the spatial variability of soil water retention function only (e.g. in their cases #2 and #3). Rather, in most situations it is just the spatial variation in unsaturated hydraulic conductivities that makes the major discrimination among different soil types, especially when the hydrologic fluxes are the key variables involved in the predictions of the system behavior.

TCLMB is right when discussing about the use of simplified techniques (such as the pedotransfer functions, PTFs) to characterize the structure of spatial variability in a certain landscape. However, allowing also for some relevant results obtained specifically on PTFs (e.g. Romano, 2004), we believe that a more conciliatorily message might be given out to the readers.

If we understood well, TCLMB has also some concerns about the fact that spatial variability depicted by simplified techniques such as PTFs does not resemble that one showed by the measurements. The

Authors are referring to the landscape scale and a certain, let's say, smoothing out with respect to the variability exhibited by the observations can be seen as good at this scale. Instead, the rapid fluctuations often shown by the measurements can be rather a sort of disturbing noise and can make difficulty when interpreting the average behavior of an environmental system (since this can be the problem being tackled at that scale; e.g. Pringle et al. 2007).

On this point, and with more relevance to the use of PTFs, we would also point out that to estimate a soil hydraulic characteristic by PTF over an area, the interpolation can be done either before or after applying PTFs. However, the literature suggests that the method that interpolates first the basic soil properties (i.e., the input for the PTF) and then applies the selected PTF to the interpolated data for estimating the selected soil hydraulic property is superior to the other way round (e.g. Sinowski et al., 1997).

Case studies

It may be not completely clear what the Authors really want to show when discussing their case studies. We do have the impression that, beside the fine proposals stated in the abstract and in the introductory section, none of the case studies actually clarifies what is the best strategy one should follow for employing pedological data into landscape hydrology. None of the case studies shows what is the "*appropriate degree of complexity of the soil hydrological model to be applied*" neither how the pedological data should be employed for increasing the prediction performance of the hydrological models, namely reducing the uncertainty about the model structure and parameters.

TCLMB cites many times the statement "*the right results for the right reasons*", which actually was originally conceived to stress the need for a consistent set of field data to adequately validate a model in space and time. For example, spatially distributed data are required for validating state variables and/or fluxes simulated by a spatially distributed model. As a direct consequence, the amount of validation data (in terms of both quantity and quality) should increase as the model complexity increases. It is our opinion that none of the presented case studies reflects this issue. At the end, the way with which pedological data have been employed in these case studies, or can be exploited in hydrological models, still appears as something one could be foretold from a "crystal ball".

Below we will comment the individual case-studies more specifically.

Case study #1

As acknowledged by the Authors themselves, case-study #1 is not strictly devoted to address a hydrological issue *per se*. Still, we have some concerns about the interpretation of the relevant results. These concerns could also arise from some difficulties we found in understanding all of the details of the presented application, which probably was not fully illustrated for the sake of conciseness. The Authors could add some details in supplementary materials as annex. As stated above, it is well known in all fields of system modeling that the predictive ability of a model is related not merely to the model complexity, but also to the data employed for parameterizing and running the model itself. Thus not surprisingly, "*the predictive ability evolves discontinuously with respect to model complexity*" in the modeling exercises presented in case study #1. It is also well known that employing averaged observations of soil properties in a prediction model (a simple regression PTF or a more comprehensive model) provides generally worse results than averaging the results obtained with individual soil observations (Heuvelink and Pebesma, 1999). Therefore, one could expect the reduction of the prediction performance of Method #5 with respect to Methods #4 and #6. A better service to the hydrological community would have been done by explaining what is the most appropriate strategy (possibly an objective and reproducible strategy) when one has to select, aggregate or disaggregate soil map units or by clarifying what is the "representative soil" utilized in some of the methods presented.

Case study #2

This case study presents a method for identifying "*hydrologically homogeneous units*", already published in a previous paper. Beside the map illustrated in Figure 4b, nothing is presented for showing the added value of the alternative technique from a functional perspective, i.e. for planning and managing irrigation at the district level.

Case study #3

TCLMB presents here the application of a process-based method and an index method for assessing groundwater vulnerability. Similarly to case study #2, no data are shown for illustrating the relative performance of these two approaches neither one can get any idea about the efficiency of the procedure followed for implementing the process-based model as compared to less or more complex procedures.

Case study #4

In a different way from the previous cases #2 and #3, in this case-study measured data are employed for evaluating the prediction performance of the TOPKAPI hydrological model.

The discharge observed at the outlet of a 150-km^2 catchment is compared to that one predicted using the following two different sets of parameters:

- parameters derived from the FAO/UNESCO Soil Map of the world, at the 1:50,000 scale;
- parameters derived from field surveys and laboratory measurements, coupled with a digital terrain analysis for identifying soil-landscape map units.

It should be pointed out that the scale of 1:50,000 can be not suitable for retrieving some soil properties that are strongly correlated with terrain features. Provided that (*i*) a model, such as the TOPKAPI model, is highly sensitive to terrain attributes as defined by the digital terrain analysis and that (*ii*) some key model parameters are generally highly correlated with the terrain attributes (such as "soil transmissivity", "soil depth", or "soil capacity"), one could expect that the discharge simulated using the latter set of parameters is relatively closer to the available observed data.

TCLMB claims to evaluate "*to what extent a parsimonious identification of soils (forms) can help in interpreting the hydrological complexity hidden in flood forecasting (functions)*". This would be of great interest and the paper would provide a valuable service to the hydrological community. However, to effectively meet this aim, it would have been desirable that the paper had illustrated issues such as:

- how the pedological knowledge gained from field campaigns and laboratory analyses has been employed for parameterizing the model; what are the innovative approaches and why these are more efficient than others;
- how the discrepancies between "soil observation scales" and "model scales" have been resolved;
- how sample soil hydrological data have been used for the "*determination of soil hydraulic properties at the eight soil-landscape units*" and then to parameterize a model with a support scale much larger than the support scale of the observations;
- how the intra-unit variability has been described or aggregated.

At this point, the following question springs to mind: To what extent the Authors are able to prove that the eight soil-landscape units represent the most effective level of soil catchment description for discharge prediction? Namely, "*to what extent a parsimonious identification of soils (forms) can help in interpreting the hydrological complexity hidden in flood forecasting (functions)*"?

This case study represents a classical example in which the hydrologist is not able to verify to what extent the distributed model is providing "*the right results for the right reasons*". That is chiefly because the only information provided is the integral catchment response, whereas nothing is said or known about the state variables and fluxes simulated across the catchment.

As a final and general comment, one would ask whether this paper makes an important contribution to hydropedology serving hydrology. We do not think so.

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