We believe that open discussion between scientists is a beautiful thing and a key issue in open access journal such as HESS and then we must thanks Chirico and Romano (hereafter refereed as C&R) for having devoted time and energy in reading and commenting our manuscript producing an additional revision to the manuscript which will indeed help us in ameliorating the text.
They provoked a stimulating discussion by writing interesting comments that unfortunately we consider not always matching the focus of our manuscript. They concluded their comment with the following strong statement "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."
This statement deserves special attention and in the following lines we provide our answers to their strong statements.

General Comments
Let's start from basics; the title of our contribution is "Potential and limitations of using soil mapping information to understand landscape hydrology". With this title we only aimed to address the following issue:
"Despite these developments and broad conceptualizations, the crucial point as to how the soil data from typically available soil mapping databases can be usefully employed by the hydrologist has yet to be addressed. This question implies detailed knowledge of the quality and quantity of information embedded in and behind a soil map.
This work produced an analytical evaluation of the potential and limitations of soil data obtained through soil surveys and soil mapping…"
To the knowledge of the authors, this is the first attempt, in the international scientific literature, aiming to an hydrological interpretation of "all" soil features as presented in standard soil maps database (<u>i.e. not obtained from soil maps specifically prepared for research purposes</u>) as made by soil surveys in most countries. Considering that often this type of soil information is the only one available in many countries, we leave to the editors and the scientific community to evaluate whether or not our manuscript is a valuable contribution to hydropedology serving hydrology.
C&R in their criticism did not produce any comments on this initial and indeed fundamental part of our manuscript (see tables 2, 3, 4a, 4b, 4c). We believe that if our contribution has to be considered not valuable then this core component has to be criticized, even dismantled, but cannot be simply ignored.
Nevertheless, as requested by both reviewers, we will indeed improve further this part of our manuscript.

As stated in the manuscript, we present some case studies which we believe important:
"This evaluation is made from a landscape hydrology viewpoint and is also developed through the following Italian case…" but it must be made clear that the presented case studies serve to show some examples (partly published as in case studies 1,2 and partly not yet published as case studies 3 and 4) to highlight approaches for making use of real world soil maps in a landscape hydrological perspective. This has been acknowledged by J. Bouma in point 2 of his revision when referring to our case studies he wrote: "…This has been done before in literature, but the link has most often been made with abstract databases rather than with soil maps…".
On the detailed criticism concerning our case studies we reply in the "specific comments" section.

Overall we understand our paper could raise too high expectations. This could have motivated the disappointment of C&R. These expectations would indeed overestimate our ability, if one supposes that in this paper we may solve hydrological problems and paradigms that hydrology did not solve in the last 25 years…!
Between these major issues (quoting C&R):

- *how the discrepancies between "soil observation scales" and "model scales" have been resolved;*

We think that this request is out of the contest of this paper. In the posted question are comprised <u>several fundamental, still unsolved, hydrological problems</u> mainly concerning scale and spatial heterogeneity. Basically they come from the fact that most of the advanced watershed models (i.e. distributed physically-based models) are based on the physical governing equations valid at small-scale (e.g. Richards' equation) while their application at larger scales have to tackle (i) the effects of spatial heterogeneity, (ii) the non linearity of many hydrological processes and (iii) the process interactions at all scales (Mc Donnell et al., 2007). Among others, benchmark papers on this discussion were those of Beven (1989) and McDonnell et al. (2007).

For the reasons given above, we do not think to go in further details on this issue risen by C&R but an original contribute can be found in Basile et al., (2003, 2006) in which they developed a method, based on the hysteresis concept, aiming to convert water retention and hydraulic conductivity functions measured in laboratory ("soil observation scales") to the field scale which is the scale of the model application ("model scales").

- *Spatial variability issue: Spatial variability of soil properties is a recurrent issue in this paper...*

It is true that spatial variability in different ways is addressed directly and indirectly in all case studies and of course we agree with C&R on the importance of this topic. It is also known that today there is a huge scientific literature describing soil spatial variability (geostatistics, ANN, etc.) and (sometimes) its implementation in hydrological simulation modeling, but it is also true that most of these works are based on research purpose databases.
But it is also rather evident that this problem, viewed from an hydrological perspective, it is still far from being solved (Beven 1989, and McDonnell et al. 2007) and our contribution does not aim to specifically contribute to this hot topic.
Therefore our work shows ways to handle the spatial variability issue but it cannot go in too much details since, standard soil map databases have not as many spatial information (because of their high cost) as typically required for a rigorous description of the spatial variability.
We will anyway improve this part of our contribution giving more emphasis to this issue.

- *Best strategy to employ pedological/soil data into landscape hydrology*

Recurrently C&R asked us to provide the best strategy for employing pedological/soil data into landscape hydrology or aggregate/disaggregate soil mapping units for hydrology, etc. We feel that if we had really solved this issue for any general case, possibly hydropedology would not be useful anymore because anyone would know how/when using a strategy A instead of strategy B.
Again at this moment hydropedology and then also our contribution is far from being able to provide definite answers on this topic while many general conceptualization are indeed available. Once again our work only aims to address the issue written in the title "Potential and limitations of using soil mapping information to understand landscape hydrology".
Anyway we indeed recognize the importance of this issue and, therefore, we try to improve as much as we can will improve this part of our contribution.

Specific comments (here we reported in boxes and in italics the comments of C&R and our reply below)

> *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 that 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.*

We agree with C&R that hydrological models are accessible and transparent more than before, but what it was originally meant in the paragraph was different. The reproducibility concept referred to the methodologies behind the models and not to the possibility to have access to them in order to reproduce the (same) results. It is a matter of HOW and not WHAT results are (re-)produced. At this regard particular importance assumes the choices made on the parameterization and the approximation of the equations. We wanted to stress the necessity of an effort to make explicit these choices in order to let other modelers value them. Similarly, a soil classification is a model which should be not only accessible but also explicable to scientific community.

> *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.

Since our contribution is placed in an HESS special issue on catchment classification and PUB we did not think to provide further explanation on the importance of soil information aiming to reduce uncertainty in hydrological predictions. It is well known in fact that PUB research is encouraged to look for new methods based primarily on improved understandings and description of physical processes within and around the hydrological cycle. To this respect soil classification can also play a role helping in catchments classification (see general statement of this special issue).

Needless to say, we can easily further implement these concepts in the introduction.

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).

Basically C&R and the authors are saying the same things. The statements "might induce one to assume.." or "Unfortunately this is not always the case…" would tend to explain the concept. Anyway we will further improve the readability of the text.

Spatial variability issues)

Spatial variability of soil properties is a recurrent issue in this paper. Firstly, it seems that TCLMB emphasize 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.

C&R ask to treat the unsaturated hydraulic conductivity spatial variability instead than treating only the variability showed by the soil water retention. We really do not understand the motivation of this remark with respect to the objectives of our paper (see above). Anyway, we think that C&R expect our contribution on the issue and then in the following lines we provide a very short comment concerning the relationship between water retention (WR) and hydraulic conductivity (HC) variability.

All of us agree that HC comes from the contribution of relative hydraulic conductivity, KR, and saturated hydraulic conductivity, KS. KR, in turn, directly reflects the pore-size distribution (in practice the water retention curve) after a corrective parameter, l, is introduced to account for the tortuosity, connectivity and correlation of the pores. Accordingly, any contribution of KR variability to the spatial pattern of unsaturated fluxes has to be mainly seen as a contribution of the variability of soil water retention characteristics.

The question to be answered now is about the relative importance of KR (or WR) and KS variability on determining the variability of fluxes in unsaturated soils.

Moreover, the KS and its variability, showed in the database but also that measured for specific research purposes is very often overestimated because -) frequently measured on small samples in the laboratory with an inadequate REV and -) for the occurrence at field scale of WR hysteresis loops. This issue recalls the discussion on "soil observation scale" and "model scale" and the laboratory/field scaling procedure of the KS (Basile et al., 2003, 2006).

Actually, most previous studies of unsaturated flow in porous media exhibiting heterogeneities have focused on the KS (Dagan and Bresler, 1983; Russo, 1984; Smith and Diekkruger, 1996; Indelman et al., 1998), assuming that the higher the input uncertainty of a given parameter, the larger its effect on predictive uncertainty of the flow process being investigated.

This is not always true because the contribution of each parameter of the constitutive hydraulic relationships depends not so much on its coefficient of variation as on the sensitivity of the model to the parameters, on the boundary condition and on the flow process being observed (Lu and Zhang, 2002; Coppola et al., 2009).

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).

The main goal of the paper is to discuss about the potential and limits of the pedological information embedded in soil maps and correlated database. Then, we surely believe that PTF is one of the most important (and probably the widely applied) tools to approach hydrological problems at landscape scale. At the same time, the limits of its application cannot be hidden.
Specifically, for the question posed by C&R, nobody knows a priori if the PTF smoothes "a disturbing noise"(sampling/measurement error?, spatial variability?), or smoothes a well-defined process (i.e., preferential flow, fingering paths, lateral fluxes, etc.) and how great is the impact of this smoothing on landscape hydrology and especially on flood forecasting. We believe that the best (right) answer is surely based on real field measurements of state variables and fluxes across the catchment. Unfortunately, this is, after many years of high quality research in hydrology, the main limitation in testing advanced and available theories and relative numerical models. And, according to our understanding, this is one of the very basic reasons behind the proposal of this Special Issue, that follows the very interesting discussions during the last EGU Leonardo meeting held in Luxembourg in November, 2010. Hence, turning back to the topic, the great advantage of hydropedology is exactly the help in distinguishing these processes, as we try to demonstrate in the first part of our paper (see also Bouma, et al., this issue).

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).

Needless to say.

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.

C&R assumed that authors are able to solve an hydrology paradigm not solved in the last decades, namely "clarifies what is the best strategy one should follow for employing pedological data into landscape hydrology". This is not the case of our contribution where we never wrote/refer to the concept of "best strategy". Our case studies are all based on soil map databases as normally available through standard soil survey (which is never made for a purely hydrological purpose); we shaped these data in order to get and to prove that it is indeed possible to get hydrological sound information aiming to landscape hydrology.
We thanks anyway C&R for their comment and we will improve this part of our contribution (according also to the remarks of the Reviewers) giving more emphasis to this issue.

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".*

We partially disagree. The case study#1 shows exactly what C&R assert. However, in the light also of the comments of the two referees, we shall "dry" the manuscript and therefore we will consider to remove some of the quoting referring to the sentence of Grayson and Bloschl (2000) "the right results for the right reasons", better focusing the discussion on the presented case studies.

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.

The point above made by C&R concerning "the reduction of the prediction performance of Method #5 with respect to Methods #4 and #6" taken directly from our published paper on Geoderma (Manna et al. 2009) is indeed minor for the sake of this work.

Anyway for the sake of the argument we have to recall that Manna et al. (2009) made an itinerary of Land Evaluation modeling approaches using 9 different methods having an increasing complexity. The first 6 methods were based on the standard soil map database available through the Regione Lombardia. Then is not surprising that we processed this database as much as we could, including the averaging of N. points for each mapping unit, and not only a comparison between method #5 and method #6.

Then we are happy that our results are in agreement with those of Heuvelink and Pebesma (1999) which, differently from us, could use in their simulation exercise at the Allier study area more than 60 observations for less than about 150 ha !

Therefore, as clearly shown in the Manna (2009) paper "the most appropriate strategy" has to be chosen accordingly to the data availability, the budget to improve the database with further analysis (hydraulic properties, spatial variability study, etc.), and the performance required by the final user (typically, the funding agency).

With respect to the request made on "aggregate or disaggregate soil map units" we have to stress that in our case study1 the theme of aggregation or disaggregation was not treated. Nevertheless, as we have shown in case study 2, it is indeed possible aggregate and disaggregate soil mapping unit on the base of the hydrological functional properties concept.

Finally, we agree that a better definition of "representative soil" should be provided in the text.

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.

The procedure developed, and shortly reported, is fully described in a couple of papers published several years ago: Basile and D'Urso (1993) developed a methodology to couple a quasi-physically based PTF (Arya and Paris, 1981) to a representative soil profile. This procedure was extended to an irrigation district (D'Urso and Basile, 1997) to classify "soil hydrological unit" This classification was the basis of several papers devoted to the water management (D'Urso and Minacapilli 2006; Bastiaanssen et al., 2007). We will take on board these suggestions.

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.

No doubt can arise in any soil hydrologist that a very simplified index method, just based on the averaged soil profile texture classification, can describe the reality better than a physically-based procedure based on detailed measurements of hydraulic properties along the soil profiles. It should be pointed out that we are just talking about the contribution (role) of the soil in assessing groundwater vulnerability. The overall performance of the index method, taking account many other factors than soil, can be better, worst or similar, depending of the data availability, the specific environment and, especially, the money availability (see the case study#1). Turning back on the specific case study, we have to remark that the purpose of the example was on the original contribution of pedologist on the base of the soil's knowledge (andic soils or inceptisols with andic feature). In fact, the presence of clay minerals having a low degree of crystallinity strongly affects the particle-size measured by standard methods which, greatly underestimating the clayey and silty fractions.

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-km2 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. [We must apologize, but we didn't fully understood the meaning of this sentence].

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.

Some of the questions posed by C&R in this section were already discussed above in the general discussion. Here we have to add that the comparison of the simulations of the FAO soil map with the soil-landscape

units did not aim at show the (predictable) different performance, but instead to measure the knowledge gained by just using the landscape soil units. The FAO soil map is taken here as the “ground zero” of the knowledge which one could eventually get, while the soil map proposed is the sustainable compromise which a model could possibly aspire. We know also that results based only on integral variables would have not been able to prove in a rigorous manner the thesis, but to be honest the subject should more appropriately treated in a dedicated paper instead of a paragraph.
Finally, according also to the remarks of the Reviewers, we will improve the conceptual and practical itinerary from the FAO map application to the eight landscape-unit map.

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