Hydrology and Earth System Sciences Discussions

Interactive comment on "Potential and limitations of using soil mapping information to understand landscape hydrology" *by* F. Terribile et al.

Anonymous Referee #2

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I consider the topic treated by the paper to be one of the most important in hydrology; therefore, I approached the Terribile et al paper with enthusiasm. However, I must confess that I found difficulty in reading it. I believe that this difficulty lies mainly in two aspects: firstly, English usage; secondly and above all, a lack of organization in the presentation and sequencing of information within the paper, for instance: - there was no indication of what the various case studies analyzed in advance to presenting them, obliging the reader, therefore, to read them in order to achieve a summary understanding of the paper; - a lot of non-standard acronyms are used that I did not know and that, I assume, are unknown to most readers of a hydrological journal; - many models were used in the various case studies, as any case study uses its modeling chain, but in just one case(specifically the fourth case) is some explanation of this chain given.

As a result, at the end of my reading, the differences between the various strategies used to collect, combine, and use data within the various models was not clear to me.

Figures and graphs were not very useful either, in my opinion, since I found many of them were not informative enough.

However, from what I could understand, the results could be important, and I believe that hydrologists need to grasp the influence of soils on hydrological processes more than they currently do, and make full use of all the information available in their modeling. Therefore, although I believe that the paper is not publishable in the present form, I strongly recommend the Authors to pursue its publication with an appropriate rewriting, where all the information provided is better organized.

I would suggest, in redrafting the work, a merging of sections 1 and 2. In this case, section 1 could be made more concise and schematic, in view of its merging with section 2 (which, however, remains insufficient to convey all the information required to understand the procedures followed). Rewriting section 3 and partly integrating it with section 4 would be also necessary.

Finally I would eliminate one case study for a greater and more complete description of the other three.

Detailed Comments

pg 4929 - Consider the first paragraph of the paper, from "There has been . . .. " to "basin", as an example. It contains the statement that modeling strategies in hydrology are often implemented on a intuitive basis, leading to subjective, non-reproducible, conclusions. The Authors should note that I used the term "often", where they use "most"; I believe the former to be more appropriate and less debatable than the latter. The Authors should take care with details of this type, which are scattered throughout the paper. Also, in the same page the phrase: "In such a framework . . .. basins" should be appropriately rephrased. In the following lines of the same page, the concept that

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the Authors propose is that the relatively new-born discipline of hydropedology aims to reduce subjectivity in the use of soil information in hydrology. However, among the many advances claimed only PTF (the first of a long series of unexplained acronyms) is mentioned. Others should be added. Furthermore, I would like less generic statements about what hydropedology does, and what it is about.

pg 4930 - What is "landscape hydrology" ? The term is new to me and I cannot associate it to any meaning.

pg 4930-31 - Here digital soil mapping (DSM) is introduced. At the bottom of page 4930 a generic definition is given. However, by the end of page 4931 the description has not yet made clear to the reader specifically what DSM does, nor does it allow the reader to reproduce the procedure itself. Admittedly, I am not a pedologist, however, the differences between the collection of data presented here and the standard way of producing soil maps is not clear enough. There are two concepts here that need to be highlighted: on the one hand, the old soil-mapping techniques are not suitable for giving quantitative indications to hydrologists; DSM, on the other hand, could help. In section 2 a more detailed but, in my view, not yet satisfactory description of DSM and the classic techniques is given. An appropriate merging and development of these two sections could result in a more fluent Introduction.

pg 4931 The sentence " For instance . . . mental . . . manner . . .. described.", should possibly be deleted.

pg 4932 - Section 2.1 - This introduction should probably be placed in the Introduction. Equation $s = f(c,l,o,r,p,t, \ldots)$ could probably be better explained just in words. Also, the sentence "(i) soil formation has a strong mechanistic basis" does not seem very significant to me in the context in which it is used - I would delete it.

pg 4932 "environmental catchment hydrology" does not exist. Catchment hydrology does exist; however, the sentence is not true with this respect. Classical catchment hydrology usually ignores all geological data. Modern distributed modeling uses local

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measures or, maybe naively, PTF. Certainly there are no methods to distribute spatially the local hydraulic characteristics of the soil derived from sample measures except for PTF; on this point I can agree.

pg 4933 - (iv) "soils can differ greatly" -> soil can present a great spatial variability

pg 4933 - Remove as unimportant: "In the above equation . . .. spatial soil analysis".

pg 4933: "Despite these well known . . . management". The passage from the conceptual tools offered by the CLORPT to the production of soil maps remains unclear. A concise review of papers to be read should help the common hydrologist in understanding. Because this material is actually present in the paper, it is probably just matter of reorganizing it in a more linear way and putting the appropriate phrases and citations in the appropriate places.

pg 4933 bottom. Here the DSM are mentioned again, then returning a few lines later to the soil mapping procedure that is followed according to the CLORPT concepts. This jumping back and forth between topics should be avoided: reorganize it!

pg 4935 - Point 2 description is missing a verb.

pg 4935 bottom - Point 3 probably needs some references.

pg 4936 - "With these considerations . . .. 2-3 years"; this is unnecessary information in the context of this paper.

pg 4936 bottom and pg 4937 - "Depending . . . covariates"; this phrase is too generic to be useful.

pg 4937-40. Information obtained following the procedure in section 2 seems to be at field scale. What is the reason, therefore, to mention global soil maps, which were probably obtained with different procedures, and then open a discussion on the geographical scale of available soil maps at this point in the paper? In my view, this information should be relocated to the Introduction (before section 2) to justify the sub-

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sequent focus on detailed DSM work that can really cope with hydrology. These same arguments apply also to section 4. Otherwise, it appears that soil databases are a nonsensical collection of information at different scales, collected with different methods, and substantially a waste of time and money of which it is useless to discuss - of course, neither I nor the Authors believe this to be the case. Instead the Authors should describe what a useful soil database must contain , and they could also give a little waring that existing databases should be used with care.

pg 4941 - A section about the hydrological significance of soil mapping is due, and this position in the paper is the right one. The arguments used in the second part of section 4, from the beginning of this page, seem appropriate to this scope (on the other hand, the discussion on soil coating I would eliminate). However, what is written in Table 2 does not encounter my expectations, it being generic and not sufficiently explained. I personally need explanations about this table (its terms meaning little to me) and not comments like: "Analysis of the table clearly shows ….. applications". In fact what it shows remains unclear to me. The same applies to most of the contents of all the tables presented.

pg 4942 - "Some examples of interaction", I suggest that the title be changed to "An application of pedological information to hydrological forecasting", or something similar. The current introduction to section 5 should be placed elsewhere; in my opinion it should be included as a subsection in section 4. Section 5 itself should begin, rather, with a rephrasing of the paragraph beginning "The latter concept ..". However, a more detailed introduction to the case studies is required so as to understand the rationale behind the cases chosen. A simple list could be sufficient for the purpose, for example: "In case (I) we did THIS for THESE reasons, and used THESE models and THESE data. Most of the information about the case study can be found at …. In case (II) … etc". The problem I had with the case studies is that, except for Case 4 to a certain extent, all of them use a complex machinery of data and models, but seldom are the inner workings of the models explained or indicated sufficiently. The reader, therefore,

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is not in a position to understand whether the discrepancies between observations and modeled results are due to weaknesses contained in the soil data or in the models' structure. A brief description of the models used should be present.

pg 4944-45 - LE should be better defined. At least a reference to where the procedure is explained is necessary. The case study, for the scope of predicting maize biomass production, uses nine methods that remain mostly unexplained. The Authors cannot expect a reader to judge the correctness of their conclusions based on the cryptic information that they give. Besides, with the information presented, the study remains largely unreproducible by others, which should be the primary scope of any scientific communication.

I will not go into the details of the other cases, 2,3 which show the same weaknesses as case 1.

With regards to case 4, the description of the methods is clearer, and follows the standard way of presentation more closely. However, the forecast obtained is poor, and will not satisfy hydrologists, who would be obliged to look for other datasets to improve their forecasting. Therefore, should the conclusion of case study 4 be: at the time of writing, inference of hydraulic characteristics from hydropedological data remains a science in its infancy and is not yet able to provide reliable estimates of discharges ?

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