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Hydrology and Earth System Sciences Discussions

Interactive comment on "Future directions for hydropedology: quantifying impacts of global change on land use" *by* M. J. Vepraskas et al.

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This paper provides an interesting and practical outlook on the role of hydropedology in predicting global change on land use (focusing on septic systems vs. wetlands in this particular paper). Such kind of study should be pursued more, in my opinion, as we face future climate and land use changes. It also illustrates well how pedological and hydrological expertise and databases could be integrated to generate true hydropedological applications (as Dr. Bouma praised this paper). It is certainly important to make good use of the existing soil survey databases (including SSURGO maps available nationwide in the U.S.), hydrologic models, and future climate change scenarios to predict possible impacts of global warming on future land uses. The authors have

done a nice job in proposing a 6-step approach towards this goal. The two reviewers have pointed out a number of positive aspects of this article, and the authors have reasonably responded to the two reviewers' comments.

This paper, for the most part, is an idea oriented paper, with hypothetical illustrations for Southeastern Costal Plain in the U.S. While the senior author has published extensively in related topics, this paper could benefit further from a more concrete example to strengthen the proposed 6-step approach, even such an example may be taken from the previously published work. Since "techniques are currently available to perform each of these 6 steps" (p. 1754 line 25-26) and the related data are available, I imagine that it would not be too difficult to include such a real example? Such a real (and systematic) example will help remove readers' possible concern of "prematurity" of the presented idea, which, in its current form, reads more like a proposal.

Regarding the 6-step methodology itself, I hope the following comments would help the authors to further strengthen their proposed approach:

1. In the use of DRAINMOD (or another hydrologic model with similar capabilities) for predicting future water table levels and related soil drainage classes (e.g., 40 years later), it seems that daily rainfall and temperature data derived from future predicted climate scenarios are the only variables considered. I wonder about the likely changes in soils data and other model inputs in the 40 years timeframe? As I understand, DRAINMOD (or other hydrologic models) require a number of soils input data (such as Ksat). In 40 years timeframe, soil properties could be significantly changed (e.g., many dynamic soil properties caused by land use and land management practices). As indicated in p. 1744 lines 19-21, DRAINMOD model "input data include soil properties, site and drainage system parameters, weather data, and parameters characterizing the crop or vegetation." Therefore, it would be good and necessary to indicate more specifics regarding how various inputs of the model should be properly handled for reasonable prediction of future water table changes driven by projected climate changes.

2. As pointed out by both reviewers, uncertainty and its quantification are important for such future-oriented predictions as so many uncertainties are involved (see my additional comment #5). As Dr. Bouma suggested, “nobody can know what the future will bring so the authors would be well advised to stress the exploratory character of what they present.” Dr. West also pointed out the impurity of soil mapping units at the scale of SSURGO soil maps and that “In many cases, the different components of the map unit will have different drainage classes because of landscape differences that could not be shown at the scale of mapping.” Earlier studies have shown that compositional purities for soil map units represented by taxonomic units were commonly <50% and map units rarely comprise more than 40–50% of the soils named in map units (i.e., dominant soil components, as the authors suggested to go with) (see, for example, Lin et al., 2005a). Thus, a reliable estimate of the proportionate extent of map unit components within a soil map unit for probabilistic assessment of soil properties is needed, as suggested by Lin et al. (2005a) and many others (see papers cited in Lin et al., 2005a). Therefore, many readers could have doubts regarding the confidence level we can place on assigning one drainage class to each of the mapping units – unless clear evidence can be provided to prove otherwise (e.g., possible special cases in the chosen study area); otherwise, appropriate uncertainty should be reasonably quantified or at least described in a meaningful way. Another critical aspect of uncertainty that needs to be better highlighted in the paper is the “fuzziness” of the NRCS' designation and estimation of soil drainage classes for each soil series (p. 1746 lines 2-4 and Table 1). As the authors correctly pointed out, period of seasonal saturation are not clearly defined in the NRCS drainage classes. Even the estimation of soil drainage classes is “on the basis of perceived depth to seasonal high water table” (p. 1746 lines 7-8). So, it would be helpful, probably required, to describe how NRCS' method of estimating soil drainage classes. One additional important aspect of uncertainty quantification to keep in mind for any modeling exercise is the error propagation through the all steps involved, as errors may accumulate to the level of unacceptable level after the proposed 6-steps – if such error is not properly controlled or quantified.

3. The authors' suggested use of toposequences, as Dr. Bouma concurred, is a wise way to link to soil drainages. It is in this very good point that I have the following questions for the authors' further consideration: First of all, I'm not sure the authors' sort of definition of toposequences in p. 1746 lines13-14 (“Hillslopes that contain soils with drainage classes ranging from well to poorly drained are called toposequences”) is consistent with the statement in p. 1747 lines 6-7 (“Toposequences in the Coastal Plain have soils with similar drainage classes, but the soils differ in the textural classes of subsoil horizons”). Or I missed something here? Second, and this is related to the above comment #2 as well as Dr. West's comments, there is a mismatch or significant gap between the existing soil map units and the desirable toposequences for modeling – because landscape differences could not be shown at the scale of SSURGO soil maps (see, for example, the discussion in Lin et al. 2005b). I cite a very revealing comment from Hall and Olson (1991) on current soil maps: “Much effort has been expended on taxonomic classification of soils during the last few years but the importance of proper representation of landscape relations within and between soil mapping units has been virtually ignored. The same mapping unit is often delineated on convex, concave and linear slopes. This mapping results in the inclusion of areas of moisture accumulation, moisture depletion and uniform moisture flow within a given mapping unit.” Third, I wonder why the authors would suggest “separate hydrologic models will have to be developed for the two or three drainage classes of interest in a given toposequence” in order to estimate water table levels in key soils of a single county and “to develop separate models for each drainage class of interest in each soil textural class family” (p. 1747 lines 17-20). Perhaps here I misunderstood what the authors meant by “separate models”? If that is the case, could the author clarify this? Otherwise, I thought in order to address various drainage classes in a toposequence, either a hillslope-scale (or catena-scale) model is needed or the same model (such as DRAINMOD) may be ran over different drainage soils so there is no need of developing “separate models for the same toposequence with different drainage classes.

4. As Dr. West commented, further clarification is needed with regard to the author's

reference to "family particle size class" and its connection to soil physical and hydrologic properties. First of all, we should clarify that a soil series' "family particle size class" refers to a soil profile's "control section" (a specifically defined portion of a soil profile) rather than the entire soil profile, which may have very different A, other parts of B, and C horizons' textures and structures as well as the soil positions in a landscape (see Soil Survey Staff, 1999). So even with the same family particle size class for various soil series, they could have significantly different properties that possibly make the drainage classes quite different from each other. Second, I concurred with Dr. West's comment that it is an oversimplification of complex soil-landscape system by implying particle size is the only controlling property, which may lead readers to false assumptions about how data from modeling exercises can be extrapolated to broad regions with varying soil properties. Thus, the authors are suggested to modify the general statements in p. 1747 lines 12-16. The authors themselves also pointed out that "It is not known at this point whether models developed for soils in a single drainage class for one textural class family will be applicable to similar soils in other toposequences for that same family" (p. 1747 lines 25-27). If so, could additional suggestions (beyond replicating model calibration studies) be provided by the authors based on their experience and insights?

5. Scaling is another critical issue in the proposed 6-step approach. Although this is an active research area across many disciplines, and I myself don't necessarily have a good solution, the authors could better address this issue through the about comments #1-4. The scaling issue is implicitly included in nearly every step of the proposed 6-step method in this paper, especially step 4 in extrapolating modeled data and step 5 in determining future climate change. In p. 1750 line 16 onward, the authors pointed out that work is needed before the $\sim$12 km by 12 km resolution predicted future climate data can be readily implemented in most current local hydrologic modeling tools (such as DRAINMOD) at the site scale (i.e., downscaling issue). Assuming appropriate climate data can be selected, then the computed hydrologic parameters of interest for a given site/soil and its drainage class under new climate conditions need to be

carefully "upscaled" to a map unit and then the entire county (see the comments #2-3 above). Overall, the scaling issue is related back to the comment #1 above regarding the essential part of this model-based exercise, i.e., adequate quantification of all sorts of possible uncertainties involved rather than leaving the users (e.g., planners and other non-technical experts) to either make false assumptions based on the final delivered maps or complaint about the unreliability of the final products obtained from the proposed 6-step method (which will then defeat the ultimate purpose of using the proposed 6-step). Future climate is certainly uncertain (including the frequency and magnitude of extreme weather events, as the authors realized), but future land uses are nearly equally uncertain and soils properties could be significantly alternated under drastic human disturbances. Thus, perhaps adding a new section before the Summary section to address all of the above raised "pitfalls" (i.e., the comments #1-5) would make the paper more scientifically grounded and less biased.

Additional misc. comments are listed below (in the order of paper sequence) for the authors' further consideration:

6. p. 1741 line 21: 1.8 million per year?

7. p. 1742 lines 10-11: While the authors pointed out that waste disposal and wetlands are mutually exclusive soils, reading maps such as Figs. 1 and 5 one gets the impression that the soils are used either for septic systems or wetlands in a county wide map. In other words, one might think that all those areas not suited for wetlands are suited for septic systems; or vice versa, all those not suited for septic systems are to be in wetlands. Is this true? In reality, we know that land use is much more complicated. For example, for non-wetland soils, if there are other water restrictive layers such as degraded argillic horizons (functioning as aquitards) or fragipans or shallow depth to bedrock etc., then there are also problems with use for septic systems. Am I not right? Or perhaps none of such soils exist in Southeastern Costal Plain in the U.S.? Also, it would probably be better to exclude urban areas and other built areas (such as highways) from the final delivered maps.

8. p. 1743 lines 9-11: It would be good to provide a brief justifications for the stated "a minimum separation distance of 45 cm is required for sandy soils, but only 30 cm is required for all other soil textures" in North Carolina. Although a reference is provided, many readers of HESS would probably wonder how these numbers were determined. The same could be said about the duration of seasonal high water table by monitoring, i.e., "at least 14 days of continuous saturation for 3 out of 10 years" (p. 1743 lines 18-19).

9. p. 1743 lines 27-28: I'm not sure this statement is corrected ("water must fill all pores in order to exclude atmospheric oxygen from the soil"). In my own experience, field soils are rarely 100% saturated because of entrapped air bubbles etc., and there is a possibility of preferential gas flow and exchange with the atmosphere.

10. p. 1744 lines 19-20: It would be good to indicate more specific soil properties in parenthesis. As indicated in the above comment #1, the authors are suggested to clarify how they would run the model regarding various inputs needed in predicting future water table change.

11. p. 1745 lines 20-21: I have trouble understanding this sentence: if "long-term water table levels can now be estimated for virtually any soil," why then there is a need of extrapolating data "to similar soils across broad geographic areas"? Why couldn't planners simply identify the soils in their areas and directly use the estimated long-term water table levels to portray climate-change impacts?

12. p. 1746 lines 19-21: It is not clear enough to me how drainlines are placed within the upper 30 cm of the soil for the alternative septic system? Could the authors provide a bit more details or use an added diagram (perhaps added as part of Fig. 2) to visually illustrate the alternative septic system?

13. p. 1748 lines 3-4: please define "benchmark water table signature."

14. p. 1748 lines 17-21 "2.4.4 Problem to be resolved": it would be good to elaborate

and expand this session, as this is a critical issue as the authors themselves pointed out. The current form of this session is too simple.

15. p. 1750 line 9: please sell out "LLNL-Reclamation-SCU."

16. p. 1752 line 9: please define "model domain."

17. p. 1752 lines 16-18: This sentence is unclear to me.

18. p. 1753 lines 26- onward: Yes, SSURGO maps are designed for county level assessments, but I'm not sure SSURGO data are appropriate to support assessments at municipality and subdivision scales. Based on my own experience and numerous published results (e.g., Lin et al., 2005a,b and the related cited references therein), I doubt SSURGO maps can help determine local areas most suitable for development and on-site waste systems. This doubt is linked to my comments # 2-5 above. General patterns of a large area may be OK, but not site-specific local areas. In fact, SSURGO maps are designed for county-level general land use planning purpose only, not site-specific applications.

19. p. 1761 Table 2 and Fig. 4: Could similar information be provided for real toposequences typical of the study area? I personally would prefer such typical toposequence data in this table instead of a list of series that may or may not be in the same toposequences – since toposequence is the key unit for the proposed modeling-based prediction and extrapolation. The same might be said about Fig. 4: instead of an idealized sketch, a (or several) real toposequence typical of the study area would be preferred, in my opinion.

20. p. 1763 Table 4: I'm a bit puzzled by the change from current conditions to Low CO_2 scenario for parameters 2, 3 and 4a: I thought if predicted average water table is to decrease (from current 30 cm to 35 cm), shouldn't the proportion of years unsuitable for either conventional or alternative septic systems be decreased rather than increased as the current Table 4 suggests?

21. p. 1764 Fig. 1: please indicate the blue areas are water?

22. p. 1765 Fig. 2: Could the distance or scale be added to this diagram, especially with regard to the required minimum depth to water table? This figure is apparently based on the classical assumption that all soil matrix will contribute to adsorbing or filtering the discharged wastewater. However, more and more we recognize the importance of preferential flow in heterogeneous and structured soils, including possible fractures in saprorites (and even rocks) that the senior author himself has studied in the past. This may be another uncertainty that should be brought up in the discussion.

23. p. 1766 Fig. 3: This seems to be actual data adopted from Daniels et al., but it was not completely clear to me whether it is actual monitored data and if so for how long time period? It would be good to clarify on this.

24. p. 1768 Fig. 5: I wonder whether this final map could be predominated by topography and thus its overall general pattern may be predictable by topography alone? So, it would be good to compare with topography map.

I applaud the authors' ending statement that "the potential uses for such climate-change maps are so large, that these limitations should be viewed as critical challenges rather than insurmountable obstructions" (p. 1755 lines 3-5). So perhaps authors could find that adding a concrete example to illustrate the proposed 6-step approach is not that difficult? I hope the authors would not be uncomfortable with the extensive comments provided above as the intension of all these comments is to help the authors further sharpen their proposed innovative idea. Thus, I hope the authors would take the above comments into consideration and provide a new revision of the paper for final publication in HESS. If the authors disagree with any of the above comments or suggestions or have a better solution, I would be glad to hear. A justified rebuttal will help readers' understanding as they read this paper since all the review comments will be published openly online.

References cited:

Lin, H.S., D. Wheeler, J. Bell, and L. Wilding. 2005a. Assessment of soil spatial variability at multiple scales. Ecological Modelling 182:271-290.

Lin, H.S., J. Bouma, L. Wilding, J. Richardson, M. Kutilek, and D. Nielsen. 2005b. Advances in hydropedology. Advances in Agronomy 85:1-89.

Hall, G.F. and C.G. Olson. 1991. Predicting variability of soils from landscape models. p. 9-24. In M.J. Mausbach and L.P. Wilding (eds.) Spatial Variabilities of Soils and Landforms. Soil Science Society of America, Madison, WI. Special Publication 28.

Soil Survey Staff. 1999. Soil Taxonomy – A Basic System of Soil Classification for Making and Interpreting Soil Surveys. 2nd edition. USDA-NRCS Agricultural Handbook No. 436. U.S. Govern. Printing Office, Washington, DC.

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