Hydrol. Earth Syst. Sci. Discuss., 6, C292–C295, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C292/2009/


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.

**L. West (Referee)**

Larry.West@lin.usda.gov

Received and published: 13 April 2009

General comments: This paper is well written, presents a novel and interesting approach for evaluating the effects of potential climate change on depth to seasonal water tables in soils, and should be published. More importantly, the paper outlines how these changes can be interpreted in terms of increases (or decreases) in area of wetlands and soils suitable for onsite systems. The presentation of predicted changes for one map unit strengthens the paper even though many assumptions were made to generate these predictions. The authors adequately point out data gaps and issues that need to be addressed before these kinds of evaluations can be made to quantitatively predict changes in soils that may impact function for specific uses. The important focus of the paper is that although they can be substantially improved, methods are



available to predict the consequences of climate change on soil properties and function, and these can be used to improve land use decisions and long-term ecosystem distribution.

Specific comments:

p. 1743, l. 5: Most states do not have different separation distances from trench bottom to the water table for different textures.

p. 1744, l. 5-8: The wetland hydrology parameters given here are for jurisdictional wetlands as defined in the U.S. I think it is important that the authors make this clear in the paper since that there are areas that many consider as wetlands that do not meet the jurisdictional definition for hydrology. This does not affect the authors proposed methodology or conclusions of the paper. It should be noted that other criteria could be used to define wetland hydrology depending on the users' objectives.

p. 1745, l. 25-28 and other sections of the manuscript: The authors' concept of using soil survey data to extrapolate model results to a larger landscape is valid and important. However, they should attempt to better explain and clarify the composition of map units. Soil survey map units are recognized to contain more than one component (soil series or phase) in most cases. The SSURGO database lists the components of the map unit and the approximate aerial percentage of each. 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. Because of differences among the components, the authors should consider suggesting a method to evaluate the components to derive a single value of drainage class or depth to water table to be used in the model extrapolations. Several different methods have been used to aggregate map units including the dominant soil or condition, most limiting soil or condition, and weighted average based on aerial percentage of the different components. While this does not appreciably alter the concepts, proposed methods, or conclusions of this manuscript, it is important that the reader understand that how the map unit data are aggregated for extrapolation may alter the amount and distribution of projected changes resulting from climate change.

p.1746, l. 10: The reference for Soil Survey Division Staff, 1993 is not in the reference list.

p. 1747, l. 6-8: Is subsoil texture the only characteristic that differentiates soils within a particular drainage class? My understanding of these landscapes is that thickness of sandy A and E horizons is also criteria for series differentiation. I also assume soils on floodplains are differentiated from upland soils such as those discussed here. I suggest the statement be altered to better clarify soil differences recognized in the region. Also, I think the criteria you are referring to as the differentiating characteristic is "family particle size class" and not "textural class family" or other terms that were used elsewhere in the manuscript.

p. 1747, l. 11-16: I did not understand the point the authors are making in this paragraph. Soils in the same family particle size class do not always have similar physical properties. This may be the case for the soils in the area described in this paper, but soil physical properties are strongly influenced by clay mineralogy, organic C content, surface area, and soil structure. Implying that particle size is the only property influencing other physical properties over simplifies a complex system and may lead readers to false assumptions about how data from modeling exercises such as was described in this paper can be extrapolated to broad regions with varying soil properties.

p. 1747, l. 24: There are only four particle size classes in Table 2 with a subdivision of the fine-loamy family based on family mineralogy class. I suggest the authors state that the divisions used are selected combinations of family particle size and mineralogy classes common in the region.

p. 1748, l. 13-14: It is not clear how the drainage class will be assigned or more precisely how the depth to low chroma colors will be determined. I assume current drainage class will be determined from soil survey databases and not from results of



the model simulations, but this should be clarified in the text.

p. 1750, l. 27-p. 1751, l. 1: This is an interesting choice for the initial depth to the seasonal high water table – 30 cm which is the boundary between somewhat poorly and poorly drained. Table 4 indicates that the seasonal high water table is predicted to be at 35 cm under Low CO2 conditions. I would not interpret this as indicating the soil as "wetter" although the proportion of years unsuitable for conventional and alternative onsite systems does increase. I suggest additional clarification of how the site was interpreted as "wetter".

p. 1752, l. 7: I suggest that the SSURGO database be referenced here since it is digital data that would be easier to use for display of changes.

p. 1752, l. 9-13: I would think properties other than drainage class and family particle size class, e.g. mineralogy, should be considered to identify similar soils. Should all physical properties be considered in the extrapolation or only those properties that are input variables into the simulation model?

p. 1753, l. 19-20: In this sentence, the authors switch from discussing map units which as stated in the preceding paragraph are composed of more than one soil back to drainage class for a single soil. As noted above, it would be good if the authors would suggest a method to aggregate multi-component (soils) map units with each component potentially having a different initial drainage class into a single drainage class for the map unit. Dominant component? Most limiting component? Weighted average?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 1737, 2009.