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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hillslope hydrology under glass: confronting fundamental questions of soil-water-biota co-evolution at Biosphere 2" *by* L. Hopp et al.

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General Comments The paper describes a set of hillslope experiments that will be conducted at Biosphere 2 and numerical modelling work that was done to support the experimental design. The large scale and high degree of experimental control is proposed to provide unique insights about the evolution of semiarid hillslopes under different climate conditions. The paper is generally well written in terms of clarity and the significance of these experiments is important enough that the broader scientific community should know about them. Overall, they appear to be interesting and most

likely insightful experiments. However, there are some problems and considerations that should be addressed before the manuscript would be acceptable for publication in HESS. One important problem is that the paper does not discuss the hypotheses that the experiments are designed to test. Some of my other comments center on the evaluation of infiltration excess overland flow. This type of flow is common in semiarid systems and has important implications for erosion during the experiments. However, I have reservations about the modeling approach used and the text does not adequately discuss how and why infiltration excess overland flow will be avoided. These and other specific comments and editorial corrections are discussed below.

Specific Comments

After reading the paper I realized that something was missing. The discussion of Platt on page 4 line 103, indicates what the problem is. The authors note that they have tried to follow the Platt's approach of strong inference when developing their experimental design. While the approach described is consistent with Platt's recommendations about leading researchers debating the merits of the experiments, this is really a secondary thing, and the paper overlooks the most important part of what Platt was promoting. The paper never explains what the hypotheses are that the experiment will test. The central idea of strong inference is to develop a set of alternative hypotheses and then design experiments to test them. As described, the experiments look good and there was clearly a lot of thought that went into the design, but the lack of explicit description of how the science questions led to the development of testable hypotheses is a problem, especially because the authors claim to be following the strong inference approach. Without such discussion it could be suggested that the research may fall into the trap of collecting low-information data that the authors state they are trying to avoid.

On page 3 line 87 it is noted that the hillslopes will be allowed to evolve for an anticipated period of 10 years. While this is likely a funding driven constraint, it would be worth commenting about the fact that a limitation of the experiment is that 10 years is

still a very short timeframe, especially because many important hydropedologic (and other) processes occur over much longer times. For example the impacts of vegetation on surface soil formation/alteration will barely have started. Such a consideration will have impacts on how well the hypotheses and science questions can be tested.

Section 2.2.1, pg 10. This subsection is a bit confusing. Why were these particular codes used? They don't give the reader the impression that they were optimal for addressing the design problems being evaluated. Wouldn't something like HYDRUS 2-D have been as easy to run and avoid the uncertainties about a fixed unsaturated zone (but see comment below about overland flow)?

Section 2.2.1 and 2.2.2. I also have questions with the overland flow modeling approach in general. First, it would be clearer if the overland flow discussion was oriented focusing on 1) modeling and evaluation of saturation excess overland flow and 2) modelling and evaluation of infiltration excess overland flow. Both occur in semiarid systems and the experiment is supposed to be designed to avoid both (at least the manuscript gives this impression). Second, the models used appear to be weak for simulating infiltration excess. One reason is that the input data don't appear to consider precipitation intensity which is the real driver of this kind of flow. I am a fan of HYDRUS, but do not think it is such a good choice for evaluating overland flow/erosion, and I think Simunek would agree. Why weren't simple simulations with something like WEPP or other curve number approaches used to evaluate the importance of overland flow/erosion? I think they might do a much better job than the 1- and 2-D approach used. Something like KINEROS might be even better because the zero order catchment topography could be simulated. It was noted that erosion of the experiments could be catastrophic, why was so little emphasis placed on better simulations of overland flow and erosion?

Figure 3 shows the simulation results for the indicator of saturation excess overland flow versus the design criteria. Where are the results and discussion about infiltration excess overland flow? Rs is only an indicator of saturation excess overland flow. Do the model results show any infiltration excess flow? If not, is it because of the soil

hydraulic properties can accommodate the precipitation input, or is it a function of the limitation of the chosen model and the lack of rainfall intensity in the input data?

Section 2.4 and earlier. There doesn't seem to be any discussion about the design including a seepage face at the toe slope other than it will have a seepage face. This is a major design issue and perhaps a pragmatic design decision, but it will impact the moisture distributions in the experiment as the HYDRUS simulations show. At least some discussion should be devoted to this issue. For example, I don't think the experiments are meant to represent a hillslope draining to the side of a ditch, but effectively that is what the experimental design will mimic. Another issue is that it is not clear how the seepage face will be designed. Does the soil just run into some kind of support screen or will a transition to gravel or sand be made? Such factors will affect how water builds up along the seepage face, affect the rates of subsurface lateral flow, and the potential for development of saturation excess overland flow in the toe-slope.

As a final comment I was surprised that no reference was made to the landfill cover demonstration plot literature. Artificial hillslope work was mentioned, but the landfill cover literature was never discussed. Several swimming pool or box type experiments with important similarities the ones proposed here have been done over the last few decades in semiarid systems using a variety of soil types, with and without vegetation. Such literature would I think be quite useful in the design process and offer some real world examples of how these systems behave. Such experiments have been conducted in semiarid parts of the U.S.A. at the Hanford (e.g., see papers by Glendon Gee in VZJ), Los Alamos (e.g., see papers by J.W. Nyhan in JEQ and VZJ), and Sandia National Laboratories (S. Dwyer). Swimming pool/box type experiments have also been done in Germany (e.g., S. Wohnlich) and probably elsewhere. Was this kind of information used in the design process or was modeling the primary evaluation tool?

Editorial Comments

Figure 1 doesn't add much and could be removed.

Pg. 10, line 282. Figure 4 shows HYDRUS results, not the rainfall input scenario as indicated. Also Fig. and Figure are used inconsistently, and Fig. 4 is cited before Figure 3.

Pg 14 line 47, spell out degrees

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 4411, 2009.