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

B. Newman (Referee), b.newman@iaea.org
Received and published: 22 July 2009

Our responses to every comment (printed in normal) are given (*printed in italic*) on the following pages. Revisions to the manuscript in response to comments from the reviewer are underlined.

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.

The main objective of this paper is to describe the modeling work that was done to support the physical design of the hillslopes. The overarching research questions of the B2 hillslope experiment are not focus of this paper but were addressed in a recent publication by Huxman et al. (EOS, 2009). They are briefly mentioned in the introduction to illustrate the overall experimental goals and to put the design criteria for the hillslopes into context. In the case of Biosphere 2, the multiple working hypotheses center around how biology disables our ability to predict flow and how different combinations of slope hydrology and biological presence, absence and amount can be used to test and falsify ecohydrological hypotheses. The design modeling presented in the paper provides the basis to specifying the questions that the hillslopes will be able to answer and to generate falsifiable hypotheses that can be tested in the B2 hillslope experiments. <u>We added text to the introduction to clarify this point.</u>

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.

We agree that the timeframe of 10 years is short with respect to hydropedologic and ecohydrological processes and limits the range of hypotheses that can successfully be tested. This has been subject of discussion and was incorporated in the design criteria (see criterion A2 – simplicity and effectiveness). Observing the coupled evolution of a vegetation-landscape system remains an experimental challenge. On the other hand, work in the landfill and mine cover system field has shown that engineered soil covers that are exposed to environmental conditions experience alteration of hydraulic properties due to increased heterogeneity within a few years (e.g. Benson et al., J Geotech Geoenv Eng, 2007). Geochemical modeling work (see companion paper by Dontsova et al., HESSD) has suggested that we will see some weathering and the formation of secondary minerals within a few years, resulting in altered hydrological behavior. Also root penetration and associated changes in pore size distribution should occur within several years. <u>We have added some text in the introduction to further stress that the temporal (and also spatial) scales of the B2 hillslope experiment limit the questions that can be explored during the experiment.</u>

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)?

The purpose of the initial modeling was, as we state on p4421, to be able to rapidly sample a large region of the parameter space. These particular codes were used for this purpose for three reasons. One was expediency - members of the design team *were familiar with them, and so they could be set up and used to get rough answers in a short period of time. These answers were always preliminary, and were never meant to substitute the more detailed investigations that followed them. Secondly, and in a similar vein, the codes ran very quickly, and could be batched together. Thirdly, decoupling the unsaturated and saturated zones had the advantage of allowing us to rapidly examine the roles of each domain in controlling the flow through the hillslope. The distinct limitations on hillslope discharge arising from recharge vs lateral flow rates could be quickly evaluated. <u>We have updated the text to reflect these considerations.</u>*

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) modeling 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?

Our design modeling work did not include an overland flow discussion because – as stated in the design criteria – overland flow needs to be minimized in order to keep the soil on the hillslope. We agree that overland flow, particularly infiltration excess, is a dominant runoff generation mechanism in semiarid systems and that this process will have its own effects on water flow and sediment and solute transport. In that respect, this experiment will not reflect real semiarid hillslopes; however, the focus of this experiment is not surface erosion/deposition but how subsurface hydrological, geochemical and biological processes interact to create heterogeneity and temporal/spatial structure. The hillslopes are not meant to necessarily represent any real hillslope system but provide a basic simple set-up that will allow us to understand these interactions. The semiarid climate was chosen to obtain a climate representative for the surroundings of B2 and to keep costs down.

Overland flow will be minimized through selection of soil properties and rain intensity. Rather than simulating surface runoff and erosion the design modeling presented in the paper focused on identifying a soil texture that would meet the design criteria B1-B5 and at the same time allow to minimize overland flow. Rainfall intensities will be controlled such that neither infiltration excess nor saturation excess are likely to occur. The HYDRUS modeling indicated that – under the climate scenarios used – neither saturation excess nor infiltration excess should occur if loamy sand-type textures are chosen. We agree that HYDRUS is not suitable for simulating overland flow – it does not have that capability – but overland flow was not a hydrological process we intended to simulate.

Whereas HYDRUS was the ideal model to analyze lateral subsurface flow and spatial patterns of soil moisture in three dimensions, thus providing a benchmark for comparison with actual experimental results.
Geomorphological modeling efforts have been initiated to simulate surface erosion and sediment transport but were not part of this paper that focused only on the hydrological modeling work to date.

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?

As stated above, the simulation of overland flow processes was not focus of the modeling work presented in this manuscript. The initial modeling evaluated the importance of recharge vs. lateral subsurface flow to identify suitable soil textures. The HYDRUS modeling that used only loamy sand properties did not indicate the occurrence of infiltration excess (and although HYDRUS does not explicitly simulate this process the code handles infiltration excess by removing the excess amount from the domain and documenting this in the output files). Rainfall intensities were considered in the input data – the models were run with hourly rainfall 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.

It is true that the toe of the hillslope has not been designed yet. The design modeling assumed that the hillslope just ends at the toe, exposed to the atmosphere. Indeed, both options are discussed (porous screen vs gravel/sand interface). The second option is favored but poses additional challenges as we will have to account for exchange processes within this gravel/sand layer to interpret water chemistry of the hillslope seepage. We agree that this is an important design decision, influencing hillslope behavior, and additional simulations may become necessary once the toe design has been finalized. <u>We added text to section 2.4 (Recommended design) clarifying this point.</u>

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?

This is a very good point, and we agree that the landfill and mine cover system field provides valuable results of the evolution of engineered soil systems, particularly in semiarid systems. Scientists with experiences in this field have been involved in the planning workshops and design discussions. The design of the B2 hillslopes was different to the typical design approach for cover systems in semiarid regions in that lateral subsurface flow was an important component of the water balance of the hillslopes to be designed. Cover systems in semiarid regions are primarily designed as store-and-release covers where the infiltrating water is stored and subsequently removed by evapotranspiration. Lateral subsurface flow is not a major design objective. The B2 hillslopes on the other hand will be designed such that they store sufficient water to sustain vegetation but also generate lateral subsurface flow as response to bigger storm events. Of the design parameters that needed to be chosen before the construction can begin, surface and subsurface topography and permeability of the base were decided prior to the bulk of the modeling efforts presented here. Slope angle and soil depth were evaluated in the design modeling but then decided based mainly on technical and engineering constraints. Soil texture was the main parameter that was evaluated with our modeling efforts. <u>Section 2.2 was restructured to clarify which parameters were decided prior to the modeling and based on the modeling.</u>

<u>Editorial Comments</u>

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

We find this figure helpful to clearly illustrate that the hillslope design is a compromise that needs to accommodate philosophical, scientific and technical considerations. We have therefore decided to leave it in the manuscript.

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.

Figure 4 does show the rainfall input scenario in the plots of the hydrographs. We agree, however, that the main content of Fig. 4 are the hydrographs and the saturation patterns. "Figure" was spelled out at the beginning of a sentence and in the figure captions and was abbreviated as Fig. anywhere else. Figure 4 is only cited before Fig. 3 to point the reader to the rainfall series that was used in the design modeling – results shown in Fig. 4 are not described in the text before results of Fig. 3 are mentioned.

Pg 14 line 47, spell out degrees

This was corrected.