Response to Review of Hillslope hydrology under glass: confronting fundamental questions of soil–water-biota co-evolution

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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 authors describe the use of several point-scale and hillslope hydrologic models to investigate the best design for a set of experimental hillslopes at the Biosphere 2 complex near Tucson Arizona. The authors are conducting a study that is rare in the hydrologic science community. Modeling first and then using an experiment to interrogate that model of how the system should work. As the authors rightly note this approach should allow for the strong inference approach to science advocated by Platt (1964). The key objective of the paper as outlined by the authors is to explain how hydrologic models were used to design the experimental hillslopes that are to be constructed at the Biosphere. The authors meet this objective very well. I do however think more specifics about what the design process indicates about what specific questions can and cannot be answered by the hillslope that is ultimately constructed would improve the manuscript and make it a valuable benchmark to compare the hillslopes performance to over time.

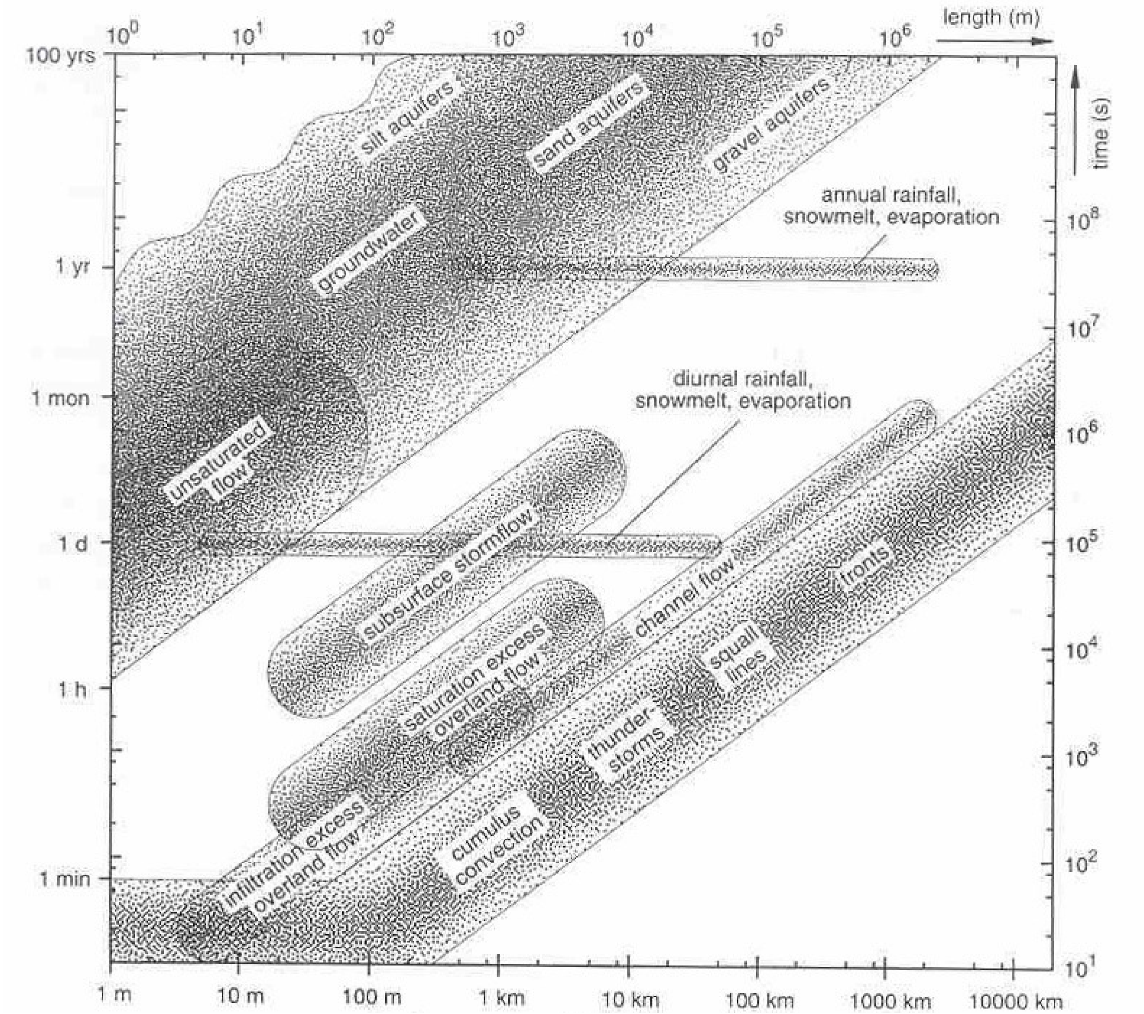
Specific Comments

The authors do an excellent job laying out the design criteria for the proposed hillslope. These objectives seem reasonable and defensible to this reviewer.

1) A simple design that enables lots of people to answer different questions.
2) Relevance to the local semi-arid climate.
3) Spatial variability must be present.
4) Hillslope must have subsurface flow
5) Enough soil moisture so plants can grow
6) Technical feasibility.
7) Limit surface erosion

These design criteria themselves though start to limit the universe of questions that will be answerable with the hillslope once it is constructed. The authors do not touch on the limitations that these design criteria themselves place on the experimental space which is available. With full knowledge of the limitation of space in any paper it would be nice if the authors addressed what was ruled out and ruled in with these design criteria briefly after covering the design criteria in the paper.

A way to possibly help the community visualize the limitation imposed by the various design criteria is to place the design criteria within the context of a commonly used descriptive figure in hydrology.



[*Grayson and Bloschl*, 2001]

The Red box seems to me the domain that the hillsope will operate within. Other design factors may further constrain or expand this region. A comment from the authors about this would be appreciated.

This is a very good point, and while we cannot discuss in detail (due to space limitations in the paper) the processes that the proposed hillslope design may facilitate or exclude *<u>we added a paragraph to the discussion, section 4.1, touching on this issue.</u>* *We argue that the proposed hillslope design is able to answer questions that center around how subsurface hydrological, geochemical and biological processes interact to create heterogeneity and temporal/spatial structure.*

The authors then step into how they used models to further refine the model design and settle on a specific hillslope design (slope, climate, soil texture etc.). They first use a simple wetting front model and then use a Boussinesq model of the hillslope. Next they move to developing a 3D HYDRUS model of the system. In their discussion of these models they do not clearly outline the conceptual model that informed the specific application of these models. Such a description might include the major physical parameters and processes involved to greater specificity than is currently done in the manuscript. A simple table might be a useful approach to addressing this concern. My concern here is that the reader is not offered a unified vision of where the authors are going by using the multiple models in sequence as they did. I wish to emphasize however that I think the use of multiple models was robust and is an approach to be recommended for other modeling applications.

Our conceptual model of flow processes that are likely to occur in the hillslopes include lateral subsurface flow (saturated or near-saturated), unsaturated flow and potentially infiltration excess overland flow to some extent (although this type of flow needs to be avoided as best as possible to not risk the stability of the hillslopes). The parsimonious modeling approach that was taken in the first step and that consisted of a decoupled 1D multiple wetting front model and a 2D Boussinesq type model is capable of simulating infiltration, the formation of a saturated zone and saturated lateral subsurface flow. It runs fast and is computationally not demanding. It was therefore deemed suitable to be used to identify primarily a soil texture that would support the design criteria of lateral subsurface flow, minimizing overland flow and providing sufficient soil moisture for vegetation. The findings from this first step served as basis for the application of the more complex model HYDRUS-3D. HYDRUS-3D is a 3D spatially fully explicit physics-based flow and transport model. To test a range of soil textures, slope angles and soil depths would have taken considerably longer. Therefore this model was only applied after the parsimonious modeling had identified a suitable soil texture and range of soil depths and slope angles so that the number of HYDRUS simulations could be reduced. HYDRUS modeling results were analyzed for spatial and temporal patterns of soil moisture and subsurface flow generation, according to the design considerations B2-B5. <u>We restructured section 2 (particularly sections 2.2. and 2.3) and briefly outline the major hillslope hydrological processes that we expect which justifies the selection of our models (section 2.3) so that our rationale for using two different models in a consecutive mode becomes clearer.</u>

More worrisomely in terms of supporting the ultimately advocated design is that the HYDRUS model version appears to come out with different results than the parsimonious model. I draw this conclusion from lines 9 and 10 on page 4426 of the paper. I may be misreading this statement and this statement is only saying that the HYDRUS model did not develop saturation due to the constraints introduced by the parsimonious model simulations. In either case some clarity needs to be offered. If my first interpretation is correct the authors need to state why the differences and how this difference may alter their overall design conclusions.

There were discrepancies in the occurrence of saturation in the two models, which arose from three sources: 1) difference in model structure, 2) differences in soil parameters, 3) differences in climatic forcing. With regard model structure, the simple model did not account for a range of processes, including capillary rise and the variable moisture of the unsaturated zone that can significantly affect the position of the water table. The detailed modeling used soil parameters from the Carsel and Parrish (1988) soil catalogue, which predicts a higher hydraulic conductivity for loamy sand than the Rosetta class averages. (While this appears inconsistent it is not unreasonable to adopt a higher design conductivity given the high variability in conductivity values within a class, and the fact that these are design suggestions for a soil that will be manufactured, rather than estimates of a real soil). Thirdly, the climate scenarios used in the parsimonious modeling were refined for the HYDRUS 3-D modeling to include a more accurate representation of the timing of the seasonality of evapotranspiration relative to precipitation, and to include diurnal variations. The improved representation of seasonality led to higher potential ET rates coinciding with water availability to increase the proportion of water lost as ET in the shallower soils. Together these three aspects led to a much reduced water table in HYDRUS-3D results, and allowed for a shallower soil design. <u>We have included text clarifying these reasons for the discrepancy in the text (section 2.3.2).</u>

Having constructed the 3D model the authors take it for a test drive with some simple climate sensitivity studies. The precipitation approaches they take provide information about how climate patterns of wet and dryness in the region influence the subsurface hydrologic response.

The authors then discuss how the modeling has informed the design of the hillslope. This discussion in general is satisfying.

A concern of mine however is the lack of specificity offered in the questions that hillslope might answer hydrologically. The authors list several questions at the end that the hillslope might answer. Similar to other places in the manuscript these questions are overly general. Given, as the authors argue, the groundbreaking nature of the Biosphere hillslope experiments I would like to see more specificity in these questions and would, uncharacteristically for myself, prefer some carefully couched hypotheses that can be falsified with the hillslope. The hillslope experiment promises to be very useful for the earth science community. It will only be as useful as the robustness of the hypotheses and questions that we use the hillslope to address. I wish the authors would be more specific as this paper provides the authors the chance to clearly state what they think will happen. The paper would thus serve as a good benchmark to compare actual experimental results from the hillslope over the years as the experiment evolves with time.

This is a good point. However, we feel that it is too early at this point in the design process to formulate specific hypotheses. Some major experimental questions, e.g.

the temporal design, are still open. The design modeling presented here is supposed to help refine the design. Furthermore, it will be used to support the generation of specific hypotheses and to design experiments that test these hypotheses. We agree that the questions listed in the discussion, section 4.3, are still fairly general. But they delineate the main topics of interest that the experiment aims to provide advanced understanding of and they can be used for the formulation of specific testable hypotheses. This will happen in the next stage of the development of the experiment. <u>We added some text at the end of the introduction to clarify this point.</u>
Despite the lack of specific hypotheses at this stage, in our opinion, these first modeling results can still be used as benchmark to compare actual experimental results to. Even if some of the design specifications change it can still be tested if the predicted hydrologic behavior of the hillslope (e.g. the lack of subsurface flow response to individual events) or spatiotemporal patterns of soil moisture generally agree with experimental observations.

Technical comments -

Most of these are stylistic comments and are simply this reviewers opinion.

Page 4412 line 9 Is spontaneous really the right word?

We used "spontaneous" here meaning that the formation of flow pathways will not be induced or controlled. We think that spontaneous describes best this fact.

Page 4414 line 27 delete the in front of Biosphere 2

<u>Was corrected accordingly.</u>

Page 4414 line 28 delete some of the and delete finally

<u>Was corrected accordingly.</u>

Page 4414 line 29 replace the word soil with the word parent material

<u>Was corrected accordingly.</u>

Page 4415 line 1 the word complex can be deleted

We think that this pairing of initially "simple assemblages" leading to a "complex hillslope system behavior" that is characterized by biophysical and geochemical interactions and feedbacks is helpful to describe the anticipated hillslope evolution and the overall experiment questions. We therefore did not delete the word "complex".

Page 4415 line 8 1 perhaps scientists involved is better than scientific community

Was corrected accordingly.

Page 4426 line 27 - Entire sentence is used with Fig. 4 instead figure 4 could simply be parenthetically called out at the end of line 2 on next page

Was corrected accordingly.

Page 4427 line 3 - similarly here Figure 5 could simply be called to at end of second sentence in this paragraph with this first sentence being deleted as unnecessary.

Was corrected accordingly.

REFERENCES

Grayson, R. B. and G. Bloeschl (2001), Spatial modelling of catchment dynamics, in *Spatial Patterns in Catchment Hydrology: Observations and Modelling*, edited by R. B. Grayson and G. Bloeschl, pp. 51-81, Cambridge University Press, Cambridge.