

## ***Interactive comment on* “Simple physics-based models of compensatory plant water uptake: concepts and eco-hydrological consequences” by N. J. Jarvis**

**N. Jarvis**

nicholas.jarvis@slu.se

Received and published: 26 September 2011

I would like to thank Dr. Teuling for his interesting and thoughtful comments on my paper, some of which touch on philosophical questions related to modeling. I have responded to these comments by grouping them under several headings:

1. Dr. Teuling dislikes my use of the term ‘physically-based’ (he suggests I remove it from the title) and suggests that following my arguments, the Feddes model would be classed as a physically-based model, because it is based on measurable properties.

Firstly, I do not define a ‘physics-based’ model as one in which the parameters are

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



measurable, as I already made clear in my earlier response to Dr. Schymanski. By physics-based, I simply mean that the model can be derived from a physical law (in this case Darcy's law). By this definition, the model described by de Jong van Lier (2008) is indisputably physics-based, while the Feddes model is not. I prefer the term 'physics-based' to 'theoretical' (Dr. Teuling's suggestion) or 'mechanistic', simply because the model does not explicitly account for the adaptive plant (biological) responses that Dr. Teuling mentions. I therefore feel quite strongly that retaining the use of the term 'physics-based' is both justified and also valuable from a pedagogic point of view.

2. Dr. Teuling disputes the idea that physics-based models should be more trustworthy than empirical models, especially given the strong adaptive and dynamic nature of vegetation and root water uptake.

There are several aspects to this interesting question. Einstein said . . . . 'Everything should be made as simple as possible, but no simpler'. Clearly, the empirical Feddes-type approach is too simple, because it ignores an important feature of the system, namely compensation. So, to resolve this, researchers have added empirical descriptions of compensation to the empirical Feddes model (I cite several that have been proposed in the Introduction). A common argument for using empirical models rather than process-based or mechanistic approaches is that they are simpler with fewer parameters. The point I am trying to make is that the physics-based approach described in the paper is just as simple: it needs no more parameters than empirical approaches to compensation. It also has the advantage that the parameters controlling compensation in the model are physical entities that can in principle be measured and readily understood for what they represent. I believe that this should be a practical advantage in terms of the ease of parameterization of purely predictive (blind) simulations (i.e. at large scales). But perhaps even more importantly, it also helps us to better understand some of the mechanisms underlying the phenomenon we are trying to model! I agree, of course, that the adaptive (biological) responses of root growth and root water uptake are also important, which I why I discussed them quite thoroughly in the

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Introduction and in the conclusions. The entire final paragraph in the paper stresses the need to account for both the physical mechanisms and adaptive or plastic plant responses. However, I would argue that we are not likely to make much progress by modifying a Feddes-type model with an empirical description of compensation that introduces a parameter which is supposed to account for both physical mechanisms and plant plastic responses, but which can only be identified by calibration at a few well investigated sites. I think it would be better to use a simple physics-based model that properly accounts for the physical mechanisms underlying compensation and then couple this to a plant growth model to account for the plant adaptive responses (i.e. take an eco-hydrological approach or perspective).

3. Dr. Teuling clearly objects to the quotes from the review papers by Drs. Roose and Raats that I cited in the introduction where they make the claim that physics-based approaches should be more reliable.

I cited these simply as an introductory background to my work. They have no material bearing on the work I show, so I will delete them, even though I tend to agree with the sentiments they express.

4. Dr. Teuling further suggests that most, if not all, successful concepts in hydrology originate from empirical observations, citing Darcy's law and my water uptake model from 1989 as examples.

I don't really want to get into discussions about the relative strengths of deductive and inductive approaches to science, since both are necessary for scientific progress. But it is interesting that the two examples given by Dr. Teuling of successful models derived from empirical observations do, in fact, have a strong physical basis. Although Darcy developed his equation from experiments, it can also be derived by upscaling the physical laws governing fluid flow at the soil pore scale (the Navier-Stokes equations). I was indeed prompted to develop my root water uptake model by the failure of an existing (empirical!) model to match observations. But in doing so, I used some

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

physical intuition and reasoning, because I figured that the model was failing because it did not respect the physics of the process. As it turns out, despite the title of my original paper, the model I proposed does have a physical basis (as is shown in the paper). So, yes, I agree that many successful hydrological concepts have originated from empirical studies, but they probably would not have survived very long if they did not have some sound physical basis. If they contradicted physical laws in some important way, they would also conflict with observations.

5. Finally, Dr Teuling asks for some clarification on the relationship between the Jarvis (1989) model and the de Jong van Lier model (2008) and suggests that there are some inconsistencies: if the Jarvis model is a dimensionless version of de Jong van Lier how can it be less physically-based, and how it can give very different results?

This will be explained in the paper, but I can briefly recap here. If the de Jong van Lier model is expressed using dimensionless parameters, then it becomes equivalent to the Jarvis (1989) model if i.) the local stress function in the latter is re-defined as equation 14, ii.) the soil is homogeneous, and iii.) the plants are stressed. Under non-stressed conditions, the two models give different results because  $M_o$ , the matric flux potential at the root surface, is defined differently.  $M_o$  is a boundary condition for the flow equation (the equivalent in the case of soil evaporation is the matric potential at the soil surface, but this is much easier to deal with, since there is only one soil surface! . . . there are multiple root-soil interfaces in soil, which makes it harder to deal with). Even if two models have identical flow equations, they will give different results if the boundary conditions are treated differently. In this case, we do not know how  $M_o$  is distributed with depth, and  $M_o$  cannot be measured with current technologies. So, different assumptions about  $M_o$  give different results. The first simulation case study explores just this question. Thus, physics-based models are still only models. They are simplified versions of reality that cannot describe the whole system: they have boundary conditions which are sometimes unknown and must be approximated.

I have tried to explain the differences and similarities between the models more clearly

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

in the revised version of my paper, which should obviate the need for the schematic figure proposed by Dr. Teuling (actually, I do not even know what such a figure would look like).

Responses to small remarks 1. OK, but the model description has been completely re-written anyway. 2. Yes, OK. 3. Yes. As I make clear in the revised version, compensation occurs even under non-stressed conditions, when transpiration is at the potential rate. This is the kind of insight which follows from using physically-based approaches.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6789, 2011.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper