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Hydrology and Earth System Sciences Discussions

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

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1 Summary

The paper emphasises the importance of water uptake from sparsely rooted deep soil layers when the upper soil profile is depleted. The author calls this "compensatory water uptake" and claims that many land surface schemes do not account for it and hence may lead to a bias in the modelling results. In order to illustrate the importance of compensatory water uptake, he presents results obtained from two similar models,

one with and one without explicit parameterisation of it. He shows that the proposed parameterisation of compensatory water uptake commonly lead to altered soil moisture distribution, increased capillary rise in the presence of a shallow ground water table and increased transpiration rates and leaf area estimates in the model.

2 General comments

The paper reads well and the data presentation is clear. However, I struggled a lot with the concepts described in this paper and therefore failed to see its significance.

The author points out that physics-based or mechanistic approaches to modelling root water uptake would give us more confidence for making predictions than empirical approaches and he claims on several occasions that the approach presented here is indeed "physics-based", without defining what he means by this term. To me, "physics-based" means that it is based on widely tested universal laws, such as Ohm's law, which was mentioned in the introduction. This in mind, I failed to see what is more "physics-based" in Equation 3 (with "compensation") compared to Equation 1 (without "compensation"). The only difference is an additional factor (α_2), which in my view, has no clear physical meaning. The physical meaning I get out of Equation 1 is the following: The rate of root water uptake in a soil layer of thickness Δz is a linear function of a driving force (E_p) and a conductivity term that depends on the amount of active roots (R) and the pressure head or water content in this layer (α). If E_p was indeed a force or a potential gradient, then this would be a very straightforward equivalent to Ohm's law, as described in the introduction. However, potential evaporation (E_p) is a flux and therefore the physical meaning of α in Eq. 1 or $\alpha_1 \times \alpha_2$ in Eq. 2 escapes me. The whole concept of representing "compensatory" water uptake by an additional factor in Equation 1 does not make sense to me and I believe that it is not consistent with the author's own description of compensatory water uptake in the introduction (p.

6791, l. 25–). Here, the author describes it as a result of increasing hydraulic gradient between the wet soil layers and the plant as plant water potentials decrease. The obvious way to represent this in a physically based model would be to simulate the changing gradient in plant water potentials and make the root water uptake a function of this gradient. This is what was done e.g. in Schymanski et al. (2008), who also analysed an additional compensation by shifting root distributions.

The second alternative, represented by Equations 6–, does not have a clear physical meaning to me, either. In Eq. 6, I am unable to attribute the units of ρ (L^{-2}) or the integral of hydraulic conductivity over a range of matric potentials to either driving forces or resistances. I am also not clear where in the description compensatory root water uptake is introduced and what would be the equivalent without it. Therefore, I do not see how either of the models presented in this manuscript would achieve a physically meaningful representation of compensatory root water uptake as described in the introduction.

In order to justify the claim that the model represents a "physics-based model" (e.g. p. 6791, l. 4), the author should first define what he means by this term and then explain the physical meaning of all the equations and variables. Then, I believe that the reader would benefit from a discussion of the physical constraints and biological degrees of freedom related to root water uptake, before being presented with a model summarising them into more or less empirical functions.

In the conclusions, the author explains that physics-based models are promising for global modeling because "their parameters are, in principle, all measureable". Except for ω_c, most of the variables are indeed expressed as functions of some potentially measureable quantities. However, whereas physics-based models generally fulfil this criterion, the reverse is not necessarily true, i.e. that models with measureable state variables and parameters are physics-based. The author would avoid most of my criticism articulated above if he concentrated on the advantages of measureable parameters, not the model's foundation in physics. However, before assessing the utility of

the model for global simulations, one has to elaborate on how the model can be sensibly be parameterised at the global scale and whether the "parameters" are indeed parameters or whether they vary in space and time.

Physics aside, I cannot tell from the paper whether the proposed representation of compensatory uptake represents an improvement to the root water uptake model. In Section 2.1, "degrees of compensation (...) were simulated by varying the effective total root length". It is beyond me how varying root length alone could represent the effect of a changing potential gradient between the plant and the soil as described in the introduction. I do not see the relevance of comparing Eq. 11 and Eq. 15 in this context. The results merely suggest to me that the model using Eq. 11 is more sensitive to root length than that based on Eq. 15 and that both behave differently. The author points out that one simulates hydraulic lift while the other does not, but I do not see the relevance of this for compensatory uptake if I understood correctly what the author means by this term. In Section 3, the author compares the root water uptake representation from the SiB model with the second model proposed here and finds that the former is largely insensitive to the depth of the water table during a dry period, whereas the latter is very sensitive to the presence of a shallow water table. The author quotes observations that phreatophytic vegetation draws more than 50% of the water uptake from the capillary fringe or groundwater and compares this with the 2% and 54% drawn from the deepest 50 cm of the root zone in the SiB and the new representation respectively. I would class this as encouraging evidence to perform further research, but I have some doubts whether the root distribution parameterised here is characteristic for phreatophytic trees. If this is not the case, the new model could give the "right result for the wrong reasons".

In the last example (Section 3.2), the author runs the models for a range of climates across an aridity gradient of 0.74 to 3.73 and finds that the new model simulates increased evapo-transpiration rates by 5–7% compared to the original model. A comparison of the simulated with observed discharge/precipitation ratios reveals that the uncompensated model over-estimates discharge/precipitation in dry climates, whereas the compensated model more strongly under-estimates discharge/precipitation in wet climates compared to the uncompensated model. As the author admits, this comparison can only be used as a “reality check” whether the simulated water balances are reasonable, and even as such I am not positively surprised by the results, as the models are constrained by the energy (E_p) and mass balances (precipitation). The test or “reality check” would be more meaningful to me if the author compared the results to the Budyko curve and evaluated which of the root water uptake models reproduces the curve better or even explains some of the scatter around the curve. However, the last paragraph of the discussion mentions that the simulated ratio of actual to potential evaporation ranges between 0.84 and 0.92 across the investigated aridity gradient and the author concludes that an assumption of such a constant ratio may be a reasonable simplification. I cannot see how this result can be true as it would violate the mass balance and therefore cannot be reconciled with the results in Fig. 9. The driest catchment here has an aridity index of 3.37 (Table 2), which is equivalent to a ratio of precipitation to potential evaporation (P/E_p) of 0.27. A ratio of $E/E_p = 0.84$ for this site would mean that actual evapo-transpiration is three times higher than annual rainfall! Perhaps the author got confused with the results in Section 3.1, where evaporation by a riparian forest was simulated, with a constant water table and hence an open mass balance. Regardless, the conclusion that prescribing a constant E/E_p is reasonable across aridity gradients is wrong and misleading, as shown in this simple calculation.

3 Specific comments

1. Eq. 1: Please explain the physical meaning of the equation and the variables.
2. P. 6794, l. 24: I did not understand this comment until I re-read the introduction and realised that “compensatory uptake” refers to the increase in water uptake

as a result of decreased plant water potential. Please provide a clearer definition of what you mean by “compensatory uptake”.

3. P. 6795, ll. 1–: Please explain how the multiplication by α_2 represents the process defined as “compensatory uptake”. How does it account for changes in plant water potential?
4. P. 6795, l. 9: How is the value of ω_c obtained and what is its physical meaning?
5. Eq. 6: The p is referred to as ρ in the text. Please check. Please describe the physical interpretation of the equation and the variables.
6. Eq. 7: Please explain the physical meaning. Is there a well-known analogy in physics?
7. Eq. 11: What is the physical meaning of M_0 and the given definition?
8. P. 6799, ll. 21–24: Please explain the relevance of Eqs. 11 and 15 for simulating the effect of compensation.
9. P. 6800, ll. 5–7: Please explain how a variance in effective total root length is supposed to represent varying degrees of compensation if compensation is defined as the effect of varying plant water potentials.
10. P. 6805, ll. 3–8: It was not explained how capillary rise was represented in the model (just the upwards flux from the water table or any upwards flux between soil layers?). How is it affected by the root uptake model?
11. P. 6804, ll. 11–19: This is very similar to the approach used by Specht (1972), where vegetation cover was optimised to maximise water use while avoiding stress. This would appear more relevant than the references given in lines 11–12.

12. P. 6805, ll. 22–29: Do phreatophytic trees have the same root distribution as in this model? Otherwise this could be the "right result for the wrong reasons".

13. Section 3.2: Were the same models used here as in Section 3.1? Please clarify.

14. P. 6807, ll. 25–27: Unless I misunderstood, it would be clearer to say that β in Eq. 17 was fitted to represent the data.

15. P. 6810, ll. 6–14: A constant ratio of actual evapo-transpiration to potential evaporation (E/E_p) with varying aridity can only be expected for energy-limited climates, but not for dry climates, as investigated here. The driest catchment here has an aridity index of 3.37 (Table 2), which is equivalent to a ratio of precipitation to potential evaporation (P/E_p) of 0.27. A ratio of $E/E_p = 0.84$ for this site would mean that actual evapo-transpiration is three times higher than annual rainfall! Please clarify where the error lies.

16. P. 6810, ll. 16–17: Yes, I was very confused by the use of "compensatory uptake" in this very paper, so it would be good if the author explained what he means by it and what others do.

17. P. 6810, ll. 21–26. How can ω_c be measured? To support this statement, a table with the parameters used and their descriptions would be helpful. I am also very uneasy about calling vegetation properties such as leaf area index or root distributions "parameters" as they change in time. To derive an appropriate functional form for M_0, one would have to measure it or derive it from some fundamental principles. The author should explain a bit better what it means in the physical sense to allow the reader to gauge how its value might be best measured or derived.

18. P. 6812, ll. 6–9: Based on this article, I would not know how to even distinguish between models that do and those that do not account for compensatory water

uptake. In addition, I did not see any convincing data here that would show that one model leads to a bias while the other does not. They do show different results, but they both might be biased in opposite directions.

19. P. 6811, ll. 23-25: Before drawing such conclusions, the author should have clearly defined what he means by "physics-based", explained how the proposed model fulfills these criteria and showed the critical advantage of this property over models that are not physics-based.

References

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Specht, R. L.: Water use by perennial evergreen plant communities in Australia and Papua New Guinea, Australian Journal of Botany, 20, 273–299, 1972.