Scientific/technical issues

1. Page 2, lines 24-25 (and line 3 in the abstract): I am not so convinced of this. I would prefer to use a physics-based model even if it did have two or three more parameters, as long as they were, in principle, measurable. The limiting leaf water potential is quite well known, at least.

2. Page 4, lines 6-8: the sentence starting … “Using h seems ….” is wrong, as the authors know well enough. It is immediately contradicted by the text at lines 14-21 on the same page (and by the results shown later in the paper). This sentence should be deleted. The remaining text just says that various forms have been proposed for the α function, but that making α depend on M is physically the most plausible. This is quite sufficient.

3. Page 5, equation 8: ho(z) is not defined, as far as I can see?

4. Page 5, lines 20-22: yes, it would be good if you mentioned this phenomenon by its name: hydraulic lift or hydraulic re-distribution. You could also cite Jarvis (2011) here, since he discussed and clarified the relationship between water uptake compensation and hydraulic lift in some detail (see the text in relation to equations 13 to 15 in the final version of this paper, not the HESS discussion paper that you cited: see point 4. under “Presentation”)

5. Page 6, lines 1-3: I know what you are trying to say here, but it is not so well expressed. You could replace i.) “ …. is only relevant” by ”…. it only needs to be explicitly addressed ….” and ii.) “… becomes less important” by “… is not necessary”. This would help, but you could also add a sentence at the end of this saying that the effects of compensation can nevertheless be explicitly discriminated and identified in physics-based models. This is demonstrated in Jarvis (2011) in the text related to equations 13 and 14 in that paper (again, in the final version)

6. Page 6, lines 11-12: “In principle, any definition of α is applicable…”. Yes, perhaps, but it does make a difference to the results of course, as you demonstrate very well later in the paper! But what is definitely not debatable is that Jarvis (1989) used a threshold type function for α based on water content (degree of saturation). The reason for adopting this approach was discussed by Jarvis (1989) in relation to the experimental evidence available at that time and no other type of function was considered. The fact that you adopt a Feddes-type function means that in the rest of the paper you cannot refer to this model as the Jarvis (1989) model. It is a modified Jarvis (1989) model, in exactly the same way that JMm is also a modified Jarvis (1989) model, where the threshold water content function is replaced by a threshold function of matric flux potential: in other words, you investigated two different modified Jarvis models and you should refer to them as such, both in table 1 and throughout the rest of the paper, including the abstract (perhaps you could call them JMm1 and JMm2?).

7. Page 6, line 28 to page 7, line 4: this is a little vague. You followed quite closely what Skaggs et al. (2006) wrote in this section, but since they wrote their paper ten years ago, it is now much better established exactly how the original Jarvis (1989) model departs from physicality. This was clarified in the papers by Jarvis (2010, 2011), which you also discuss in the following section. There are two aspects to this:

i.) the choice of function for α. The threshold function chosen by Jarvis (1989) doesn't make complete physical sense, as the local resistance to uptake should in principle increase continuously as the soil dries (e.g. like equation 18). Jarvis (1989) discussed this choice in terms of the overall resistance to uptake being dominated by an air gap between soil and root which might only develop after a certain critical water deficit was reached: this choice was strongly influenced by experimental studies which showed such an effect. Also, at high soil water contents, the overall resistance to uptake in the soil-plant system would be dominated by plant resistances, which may be more or less constant. Thus, a threshold function might be a good choice from an empirical point of view. In this respect, it can also be pointed out here that the authors also adopt a threshold α function in the PMm model. This model is the one the authors finally recommend, because it works best, although it can certainly be criticized on the same grounds (i.e. that it "affronts the definition of α").

ii.) Compensation under non-stressed conditions. As you point out, under non-stressed conditions the Jarvis (1989) model does give a different uptake distribution compared with the de Jong van Lier physical model. However, it is wrong to imply that the Jarvis (1989) model does not predict any compensation under non-stressed conditions (page 7, line 4). Under non-stressed conditions, water uptake is increased by a factor of $1/\omega$ in all layers (regardless of the pressure head distribution) to maintain transpiration at the rate demanded by the atmosphere during soil drying. It is also not wrong in principle to link compensation to plant stress (page 7, line 3): the onset of stress certainly does affect the nature of compensation: this is demonstrated in Jarvis (2011) in the text following equations 13 and 14 for the physics-based model of de Jong van Lier (2008).

For the above reasons, I strongly suggest that you delete the text on page 6 line 28 to page 7, line 4 and replace it by a short sentence that simply states that the Jarvis (1989) model departs from complete physicality in some respects and that this is explained in the following section. Then at the end of the next section (i.e. after equation 21) you can briefly summarize how the Jarvis (1989) model departs from physicality, based on the comparison with the physics-based model that is represented by equation 14-21. This will be very much clearer.

8. Page 7, lines 5-12. The parameter h_3 does not exist in the Jarvis (1989) model (see lines 10-11 especially). I think this paragraph can be deleted (or perhaps moved to the results and discussion section). At the very least, readers should be reminded that the original Jarvis (1989) model does not use a Feddes-type α function.

9. Page 8, line 22: you should add the limits for λ here. If compensation means that water uptake increases from sparsely rooted layers, then λ must lie between zero and 1. Also, you should replace "deeper soil layers" by "more sparsely rooted layers" to be strictly correct.

10. Page 9, lines 23 to 26: I wonder what it is about your modification to the Li model (the use of the matric flux potential in a threshold function) that resolves the conceptual difficulties with the original formulation that you described earlier on page 9 at lines 3 to 8. As far as I can

see, the same objections should be equally valid for this modified version as for the original model. This should be clarified and the text modified accordingly.

11. Page 11, lines 13-28: As I understand it from table 3, you only have a maximum of two parameters to calibrate for all the models, while each parameter is constrained within known limits. This means that a "brute force" grid search for optimum parameter values would be preferable to the method you chose, since you could be sure of avoiding risks of finding local minima (although it might be slower). I am sure there is no need to repeat the calibrations, but maybe you could mention this?
12. Page 13, lines 23-24: yes, this may be why a constant value of ω_c often seems to work quite well. Maybe you could add a comment to this effect, and also refer to your equation 20 and cite Jarvis (2011), where this aspect is discussed in detail.

13. Page 15, line 18: You should replace "either R or M" by "both R and M". But this sensitivity to M is in principle also present in the empirical models that include M. Why is it more important for JMII? Is it because this model is not calibrated? Or is it because of the different type of function? I can believe that predictions of JMII are, in comparison with the empirical models, more affected by the value of M_{max}, which must be a very uncertain parameter, not least because the Mualem-van Genuchten model of soil hydraulic properties is known to have an incorrect form close to saturation (since it does not allow for a maximum size of pore in soil). These questions should be clarified.

14. Page 15, line 25: it could also be noted (perhaps by referring to equation 20) that $\omega_c > 1$ is not physically unrealistic.

15. Page 16, line 3: This is misleading. The Feddes function for α is not part of the Jarvis (1989) model.

16. Page 17, lines 1-2: it is confusing that different symbols are apparently used for one of the parameters in the Li-type models. In equation 25, λ is used, whereas in the text here and in table 5, l is used, while in table 4 l_m is used. I believe they are all the same parameter?

    If I understood it correctly, I don't see how you can write that the optimal values of λ follow a logical relation to R and Tp (line 1). In many cases, and especially for low root densities, values of l (i.e. λ?) in table 5 are larger than 1, which implies to me that compensation is working incorrectly in these scenarios (it is decreasing uptake in the more sparsely rooted layers). Also, in table 4, it is stated that l_m (i.e. λ) was constrained to take values less than or equal to 1. If I understood it correctly, the results in table 5 suggest that this was not actually the case in practice.

17. Section 4.2, table 6: can you give the total precipitation and potential transpiration here? It's good to get a rough idea of how much stress occurred in these simulations.

18. Page 18, lines 10-12: you did not test the Jarvis (1989) model (see earlier comments).

19. Page 18, line 12: I did not get a good understanding of why the JMII model does not work so well for high R–low Tp scenarios (i.e. high compensation). I would have thought that, in principle, it should work OK. Please briefly explain what you think the reasons are for this.

20. Lines 16-18: I think this is too optimistic, as this test was not a very tough one. You had the same plants (identical roots) and the same three soils. How would it look if you had simulated different scenarios (soils, plants)? I think you would need to re-calibrate the empirical models. How useful is that?

<u>Presentation</u>

1. Abstract, lines 13-14: "Incorporating a newly proposed reduction …". It is not clear to me what you mean by this sentence.

2. Page 2, line 5: you could replace …"derived from" by …"extensions of".

3. Page 2, line 11: "Accordingly, plant water uptake increases …" would be better.

4. Page 2, line 16: in the reference list, you have cited the HESS discussion paper for Jarvis (2011). This must be replaced by the final published version of the paper. The author and article title are the same, but the volume and page numbers must be changed to volume 15, pages 3431 to 3446.

5. Page 2, line 28: delete "… quite incomprehensible and… ". I am not sure what you mean by this, but it's not needed anyway. It's enough to say the limitations are not well understood.

6. Page 5, lines 17-19: yes, this is important. It is discussed in Jarvis (1989, 2011), which you could cite to support this paragraph.

7. Page 5, line 19: it would be better to replace "achieved" by "maintained"

8. Page 5, line 23: the end of this sentence (starting with … "and that it can be…") is confusing and not necessary. It can be deleted.

9. Page 6, line 27: yes, but this could be written a little bit better as: "Equation 12 describes an analogy to stomata functioning (Jarvis, 1989, 2011), giving this model some physical basis. This is demonstrated in the following section."

10. Page 7, line 14: you could replace "numerically" by "mathematically". As you show, it's the actual equations that can be made identical, not just the results of calculations.

11. Page 8, line 16: add "(Jarvis, 2011)" after "conditions"

12. Page 9, line 2: don't you mean the Jarvis (1989) model? I think so, because Jarvis (2011) focuses almost exclusively on the de Jong van Lier (2008) model.

13. Page 10, line 24: data

14. Page 16, line 32: you could refer the reader to equation 20 to illustrate this. It would also be better to write “smaller” than “less than one”.

15. Page 18, line 13: using the word “predicting” is a little misleading here. It would be better to write “matching”. You are just calibrating against another model. Prediction is a whole different ball-game!