**General Comments:**

This technical note is a companion paper to a model inter-comparison (https://doi.org/10.5194/hess-2021-265) testing the efficacy of the Vegetation Optimality Model (VOM) to conventional TBMs for five savanna sites on the North Australian Tropical Transect (NATT). In this technical note, the authors perform a sensitivity analysis on the VOM at a single site, Howard Springs, in response to updated model input data, soil hydrology, optimized grass rooting depth, and an imposed free drainage condition required by the HESS companion paper. The authors perform these updates to the Howard Springs VOM originally run in Schymanski et al. (2015) (Sc15 from here on) one-by-one to detail their influence on evapotranspiration (ET), gross primary productivity (GPP), foliage coverage, and soil water dynamics. Overall, the authors found that changes to soil texture (and, thus, water storage) as well as the groundwater levels had the most significant impacts on mean annual GPP and ET. Overall, ET was substantially less annually (~19%) due to the free drainage conditions, whereas, GPP was essentially the same (~3% change) due to compensating effects from soil texture change and free drainage.

Overall, I commend the authors for taking on the tedious, yet important task of disentangling the effects of model updates on resulting outputs and revealing interesting model sensitivities to soil hydrology that are often taken for granted. I do think this paper can serve as a blueprint for assessing model changes and highlights interesting soil-vegetation dependencies for future study. Primarily, my comments focus on clarifying the introduction/motivation for this work and bolstering the interpretations of soil hydrology model updates on fluxes with results from the Supplement.

Similar to Reviewer 1, I was confused by the motivation in the introduction. The authors appear to conflate the motivations for the HESS model inter-comparison and this technical note. I have provided suggestions for clarifying the relation between the two papers and sharpening the motivation for this technical note. I understand the authors have already addressed some of my comments in their response to Reviewer 1, but I have included them for completeness.

Although this technical note does well to synthesize VOM sensitivity, I thought that the authors' analysis of the effects of soil texture and drainage updates on fluxes ignored significant changes in root water uptake, plant water use strategy, and fractional cover shown in Supplement S1. Furthermore, I feel the authors' under-utilize the large amount of information in Supplement S1 (contains 54 figures but only referenced generically twice). Therefore, I have tried to ask clarifying questions and refer to figures in the supplement that could possibly explain ambiguities in the authors' interpretations. I hope this helps the authors create a more complete picture of the VOM sensitivity to soil hydrology and highlight interesting results contained in the Supplement.

All other comments relate to clarifying methods, formatting figures, and other technical corrections. I hope the authors find these comments helpful and I look forward to their responses.

**Specific Comments:**

Lines 6: Instead of saying "… a range of updates to previous applications of the VOM have been made for increased generality and improved comparability with conventional models", you should explicitly reference the HESS companion paper. Then, the following sentence should define the purpose of this technical note in relation to the HESS paper. I think it is best at the outset to clearly differentiate the

HESS paper and this technical note. As written, it makes me think you are going to perform both the work of the HESS paper and the sensitivity analysis in this technical note.

Line 7: The wording “To assess in how far the updates….” is confusing. I would suggest simplifying it.

Line 30-35: Can you cite either sources for the optimality theory or empirical evidence for some of these assumptions for the interested reader? The assumption that maintenance costs of plant organ functionality are transferrable between species is interesting. Is there any evidence you can cite for this point?

Lines 38-43: I feel these lines could be assimilated into Lines 29-37, where you introduce the optimality theory. I think you can condense this and maybe introduce the optimality theory by introducing the VOM. As Reviewer 1 commented, Lines 40-42 are redundant.

Lines 42-53: This text appears to be the motivation for the HESS companion paper and not this technical note. You introduce the NATT sites and TBM issues raised by the Whitley et al. (2016) inter-comparison study (Wh16 from here on). Next, you state your goal is to determine if the VOM can alleviate the Wh16 issues by running the VOM at the different NATT sites with the same conditions as Wh16. This is very confusing motivation and goal for this technical note given: 1) you only use the Howard Springs site and 2) there are no comparisons with the results from Wh16. Here is what I suggest to clarify the motivation for this technical note:

1) Explicitly reference the HESS companion paper and state its goals, which are to see if VOM can address the Wh16 shortcomings. Also, I would more clearly define the Wh16 shortcomings that the HESS paper attempts to address.
2) State the purpose of this technical note in relation to the HESS companion paper.
3) State the motivation for doing this systematic model sensitivity analysis with Sc15? Why is this important? Does this technical note provide additional insights into the conclusions of the HESS companion paper? What are they?

Lines 52-62: These lines seem to lay out the updates applied to the new VOM from Sc15. I think these can be briefly summarized here as they are laid out in detail in the methods.

Lines 63-66: This seems to be closer to the point of this technical note. You must add clearer context motivating this analysis (see my comment on Lines 42-53). Additionally, I feel this paragraph is missing implications. What is the importance of this work for future modelling applications or use of the VOM?

Sect. 2.1: It would be helpful to justify why you picked Howard Springs out of the five NATT sites used in Wh16 and your HESS companion paper.

Sect 2.2: I had a hard time figuring out why some equations were included and others were not. I can certainly understand not wanting to repeat the longer methodologies in Sc15 and other publications; however, I think a few key equations for root water uptake, photosynthesis and soil water transport can give the reader a better feel for the optimized parameters in Table 3. Alternately, you could write a sentence or two at the beginning of the section explaining which equations you are showing and why. I have noted below where I thought additional equations may help.

Line 88: An issue pointed out by Wh16 was the representation of C4 grasses in TBMs. Was the seasonal vegetation represented as C3 or C4 in VOM? If represented as C3, you should probably justify this simplification.

Sect. 2.2.1: For both photosynthesis and root water uptake, it may be helpful to at least include the main equation(s) for each. In particular, the equations that contain the optimizable variables from Table 3 (electron transport rate, root surface area, etc).

Lines 105-106: It may be helpful to the reader to briefly explain the difference between your method and the traditional Cowan and Farquhar method. In line 111, you mention soil water marginal cost; which is obviously different from the water marginal cost.

Lines 113: What is your modeling time step? Hourly? Daily? Here, you mention diurnal variations in G_s, but, in Table 3, the time scale is shown to be daily. I would explicitly state the time step for your model somewhere.

Line 116: Do you mean the root systems are adjusted in terms of finding the optimal parameter values for the 30-year simulation or they are dynamically adjusted during the simulation? I assume the former given the following sentence; however, it may be good to clear this up.

Line 120: Photosynthetic capacities and root surface area distributions are vague since the equations for each are not shown in Sect. 2.2.1. It does seem that you wanted to avoid explaining the whole photosynthetic model and root water uptake equations, but it may help the reader to include at least the main equations in Sect 2.2.1. Then, here you can specifically refer to photosynthetic capacities and root surface area by their parameter names in Table 3.

Line 121: Here you are saying these vegetation properties vary on a daily basis. Does this mean the time step is daily? Earlier, you stated stomatal conductance varies sub-daily (Line 113). Please clear up the time step.

Line 123: Does this mean that you run 27 (3x3x3) separate parameter sets for the day and pick the best one? What are the justifications for performing this type of optimization on these parameters? Maybe briefly discussing the results of previous Schymanski papers that introduced the short-term optimization would be helpful.

Line 136: Is it realistic to use citrus plant parameters to represent evergreens? Is the solution very sensitive to this assumption?

Line 159-160: Wherever you say, “for consistency with other model applications (Whitley et al., 2016)”, you should instead explicitly state these updates are required for your HESS companion paper.

Line 165-166: As with other comments, I think more details on subsurface and soil evaporative fluxes would help readers understand how the roles of the soil textural changes, free drainage condition and soil evaporation play on flux changes in the new VOM.

Lines 176-177: Here, the input data all seems daily; however, G_s was previously mentioned as sub-daily (line 113). Can you clarify the time step that you use?

Line 198: Can you explicitly define FPC and how it is derived from M_a,s and M_a,p?

Lines 281-283: Wouldn't the perennial vegetation also suffer from reduced root water uptake due to lower K_sat? According to Table 2, deeper layers have much lower K_sat. I can understand increased water storage in the deeper layers due to soil texture changes, but I would think soil-to-root flow resistance also increases with the finer textures. In fact, it appears Figure 1.41b in Supplement S1 shows reduced perennial ET due to K_sat. Can you explain?

Line 292: I believe you mean the dry season. Otherwise, you contradict your previous statements.

Sect 3.2 title: The title "Resulting Differences" needs some work. This section does state the overall difference in annual ET and GPP for the "new VOM" in the first sentence. Then, the section goes into the mechanisms for said differences with Figs. 5-8. So maybe state in the title what the differences are between?

Lines 304-305: "… for consistence with free drainage conditions in other models" should be changed to reflect that this update is required by your HESS companion paper.

Sect. 3.2: Is there a reason you only focus on the overall differences between mean annual GPP and ET for Sc15 and the new VOM? I do like how you explain the differences by looking at soil water mechanisms. However, a novel part of the VOM is prediction of vegetation properties, whose changes are not really addressed in this section (besides the optimized root depths in Figure 6). Looking at Sects. 2 and 3 of Supplement S1, it would appear the changes implemented in the new VOM caused major changes to plant water use strategies in perennials (through soil water marginal cost and max electron transport rate Fig. S1.52) as well as fractional cover (Fig. S1.49). These seem like interesting and large changes that should be addressed and used to bolster the claims already made in this section (see my comments below). Furthermore, these large changes in plant water use strategy due to seemingly minor hydrological changes (although clearly not!) could provide interesting points for Sect. 4.

Lines 311-313: Figs. 6c and 7 do indeed show increased water storage under the new soil textural updates; however, they do not necessarily show greater water availability to plants (i.e., transpirable water) resulting in higher perennial ET. In Figure 6c, the new VOM rooting depths are over a meter shallower than Sc15, meaning comparing the water content in the top 5 m exaggerates the difference in transpirable water since the new VOM roots do not access much water below 3 m. Fig. 7 attempts to offer more support to your claim as the new VOM deep retention curve is more gradual over a larger range of water content compared to the homogenous assumptions in Sc15. However, this does not tell us that the additional soil water volume in the root zone is transpirable. The effect of soil textural change on transpirable water in the VOM is determined (to the best of my knowledge) by resistances in soil water uptake, plant water use strategy that determines stomatal sensitivity to soil water potential, and fractional coverage (foliage). From Supplement S1, the reductions in K_sat for the new VOM reduced perennial ET (Fig. S141b), indicating higher resistance in root water uptake. However, changes to both soil texture parameters and K_sat seemingly create a more efficient plant water use strategy (S1.38d and S1.42d) with fractional cover (S1.49e). Therefore, the question is, does the increase in perennial ET from soil texture changes result from greater transpirable water availability due to increased soil water storage? Or altered plant water use strategy? Or both? I think this argument needs to be fleshed out further to truly understand the mechanism for the buffering effect of soil texture on perennial ET.

Lines 313-315: I could also make the argument that the potentials in Fig. 6d and e look dissimilar. I like the figure, but I think it requires a bit more discussion. The new VOM obviously has a shallower rooting depth, which concentrates moisture depletion to 3m compared to the deeper dry-down in Sc15. Also, the onset of dryness appears to occur much sooner in each year for the new VOM compared Sc15. As mentioned in my previous comment, the soil textural differences also affect root water uptake and stomatal sensitivity to these potentials. Therefore, it is not apparent that the differences in timings and water potentials are strongly similar in how they affect ET and GPP. As with my previous comment, I think this argument must be fleshed out further using some of your results from Supplement S1, Sections 1.10-1.11.

Lines 325-327: Update accordingly with your response to my comments on Lines 311-315.

Lines 336: You should state that you made this assumption for the companion paper to compare to Wh16. Also, I do think this finding does coincide, at least partially, with the first of Wh16's deficiencies: Water Access and Tree Rooting depth. It may be good to highlight this point.

**Technical corrections:**

Line 82: Place comma between reference 3 and 4.

Sect 2.2.1: You should reference Fig. 2 somewhere in this section.

Line 92-93: "…maintenance respiration, projected cover, and leaf area turnover and maintenance."

All Equations: It may improve readability to add a centered dot (\cdot in latex) to represent multiplication. The subscripts and letters run together, especially in Eqn. 3-5.

Line 143: Should be "cost factor"

Line 149: "…based on a sensitivity analysis for Howard Springs (see also Supplement S2)."

Line 226: Remove "at each site separately."

Figure 3: I would increase the font size of the axis labels and titles. It is very difficult to read when printed out. Also, the range for the y-axis are inconsistent for panels a) and e) compared to the rest. I would find a way to keep consistent range or zoom in on each figure relative to the max difference. For instance, I feel we lose the message of panel b) because the range is so large yet a key conclusion is GPP can change by about 20%.

Figure 3 caption: I would just state mean annual in the first line and then you can remove "mean annual" from all the following sublabel descriptions. Also, in line 4 of the caption, the word "projected" is misspelled.

Fig. 5: Missing x axis labels. Also, I would make the legend label "VOM" to be "new VOM" for consistency with other figures

Fig. 7: In legend, it is more helpful to put the layer depth and not number, e.g., 20 cm instead of layer 1.

Fig. 7 caption: Line 2, you say "multiple red lines", I believe this is a mistake.