Response to Referee #2

We would like to thank also Referee #2 for the helpful and insightful review. Referee #2 brings forward several valid points that we will improve on in our revised manuscript. Below, we will address the comments of Referee #2, with the referee comments written in italics.

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.

Thank you for this comment. Even though we indeed had already a critical look at our introduction after the comments of Referee #1, this comment of Referee #2 only emphasizes the importance of it. For that reason, we will work on the introduction in order to clearly state the motivation of this technical note in relation to the accompanying paper in HESS. In addition, we will add references about the importance of using benchmark datasets in modelling. At the same time, we will clarify that we find it important to trace back the origin of differences in model outcomes, in order to fully understand and explain new model results.

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.

This is an important point, and we will elaborate more on these changes in the main manuscript, as well as the findings in the Supplement. More specific, we will add a paragraph on the changes in vegetation properties. At the same time, in order to address also some of the specific comments, we will do additional runs with the VOM-v0.5 with the water use parameters of VOM-AoB2015.

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.

We will change this entire sentence to: "Several updates to previous applications of the VOM have been made for the study in the accompanying paper of Nijzink et al. (2021), where we assess whether optimality theory can alleviate common shortcomings of conventional models, as identified in a

previous model inter-comparison study along the North Australian Tropical Transect (NATT) (Whitley et al. 2016)"

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

We will change this to: "Therefore, we assess in this technical paper how..."

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?

This theory was actually formulated mainly by the previous papers of Schymanski et al. (2007), Schymanski et al. (2008) and Schymanski et al. (2009). Hence, the references are limited to these studies, and an important goal of the accompanying HESS-paper is also to test this theory and the accompanying assumptions. Of course, other optimality approaches do exist, and we will add some additional references to give some more background and context.

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.

We will remove the redundant lines, but decided to keep the introduction of the VOM at the start of this paragraph.

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?

We agree with these suggestions, and will re-write this part of the introduction.

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.

We will re-write these paragraphs, but will actually add more detail as Referee #1 asked for a clear description in the introduction about the current shortcomings and necessary changes to boundary conditions of the previous version of the VOM. We believe this also addresses point number 3) in the previous comment.

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?

We will add more context in this part of the introduction and will add more about our motivation to do this analysis.

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.

We used Howard Springs as this was used previously by Schymanski et al. (2009) and Schymanski et al. (2015). Especially Schymanski et al. (2009) contained a detailed comparison with the flux tower observations at this site, and Howard Springs is for that reason a good benchmark case for the VOM. We will clarify why Howard Springs was used.

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.

Thank you for pointing this out, the selection was too much focused on the HESS-paper. We will review the presentation of the equations for the technical note as suggested. Hence, we will include equations of the electron transport capacity, CO2-assimilation, leaf respiration and the root water uptake.

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.

The seasonal and perennial vegetation in the VOM were both modelled as C3-plants. We will clarify this, and add a sentence about the consequence of 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).

We will include the equations for the electron transport capacity, the CO2-assimilation, leaf respiration and root water uptake for completeness.

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.

In fact, there is no difference, we only formulated an additional relationship between the soil water suction heads and the marginal water costs of assimilation λ. Originally, Cowan and Farquhar stated

that λ should be sensitive to soil water, whereas we use here the water suction head. This was originally done by Schymanski et al. (2009) with the expectation that plants would more likely sense the water suction head, instead of the total available water. We will add some sentences to clarify this.

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.

The VOM runs with hourly meteorological input data, down-sampled from daily if necessary. Here, we used daily data, which was then converted into hourly data by the VOM. The VOM itself has a variable time stepping approach from hourly to sub-hourly, which depends on the states in the model. The length of the sub-hourly time step is set in a way that the states in the model do not change more than 10%. The vegetation properties in Table 3 are adjusted at a daily timescale, i.e. the vegetation property is changed at the end of a day. We will add some more explanation about this in the section "Model optimization", as well as "Short-term optimization".

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.

We will rephrase this part. These parameters here are indeed optimized for the full period and kept constant, but the root systems, in terms of fine root surface area, also adjust on a daily time scale in order to satisfy the canopy water demand. Hence, the root depths are optimized for the full period, but the root surface areas are adjusted on a daily time scale.

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.

We will make this more specific, and use the 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.

These variables change at the end of a day, the time step of the VOM is variable from hourly to sub-hourly. Only stomatal conductance varies on a hourly time step. We will also clarify this in the section about the short-term optimization.

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.

We run 9 separate parameter sets indeed. The root surface area distributions are adjusted based on the canopy water demand, and these are therefore not parameterized with three different values. We will clarify this.

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

We agree that there is no evidence that citrus plant parameters represent evergreens, as also acknowledged originally by Schymanski et al. (2008), but believe this is currently the best assumption we can make. We will add a short sensitivity analysis in the supplementary material regarding this parameter.

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.

We will change this accordingly.

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.

We will add more details about these processes as well, with several sentences in the section about the water balance. We will explain that the VOM uses a discretization of the Buckingham-Darcy equation for the fluxes between the layers, as well as that the soil evaporation is a function of the soil saturation in the top layer.

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?

We used daily meteorological input data, but the VOM runs with time steps of hourly to sub-hourly. Hence, the daily time series are converted into hourly time series by the VOM. We will clarify this in this section too.

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

Here, we take it as the sum of the perennial and seasonal component: M_a,s + M_a,p. We will add this here explicitly.

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?

This is indeed true, lower K_sat affects both perennial and seasonal soil-to-root flow. However, the effects of a lower K_sat seem to be compensated by the increased soil water storage, as a result of the new soil structures with higher field capacity in the deeper layers. Hence, even though resistivities may increase, these can still be off-set by higher matrix potentials, that drive the flow. However, we agree

with the other comments of the referee, that we need to closely look at the changes in the water use strategies as well.

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

We will correct this.

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?

We will change this to "Comparing VOM-v0.5 and VOM-AoB2015: resulting differences and underlying mechanisms".

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.

We will change this accordingly.

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.

This is indeed a good point. We focused on the differences in GPP and ET initially, as ET and GPP are the most rich and reliable from the observed flux tower data. For that reason, these are good for an assessment of how the final model outputs changed, but we found many more interesting aspects on the way, similar as the referee here. We will add an extra paragraph in our results were we look at the changes in vegetation parameters.

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.

Thank you for this comment, it involves fair questions that we will have to pay attention to. In a first step, we will replace figure 6c with the water stored in the root zone, instead of the upper 5 meters. In addition, we ran the VOM-0.5 with the "old" water use parameters now, as can be seen in the figure below. Here, it can be noted that the differences between the VOM-v0.5 (red) and the VOM-v0.5 with the water use parameters of Schymanski et al. (2015) (black), are relatively small for the perennial ET, indicating that the largest effect comes from the new soil structure. Nevertheless, also the water use parameters contribute, as the perennial ET slightly increased for the new VOM-v0.5 during the wet season, whereas the values with the old water use parameters remain lower.

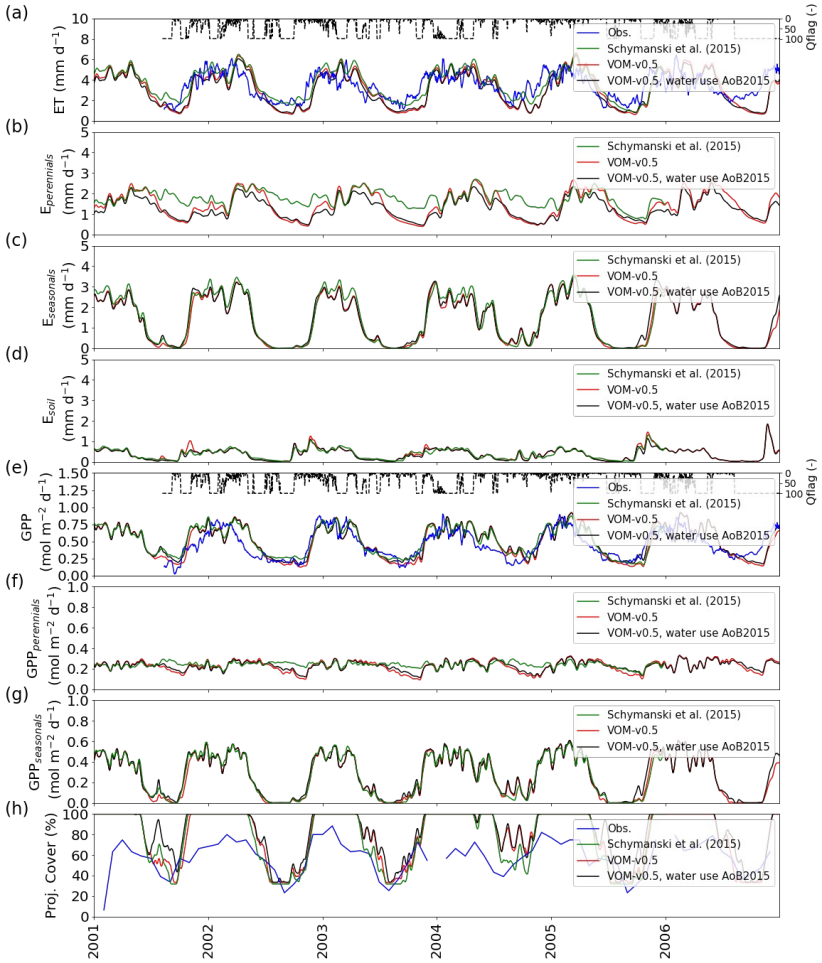

Figure 1. *Comparison for Howard Springs from 2001-2006 (subset from 1980-2017) between the results of Schymanski et al. (2015) (green) and the new results that implemented all changes (VOM-v0.5, red) and the VOM-v0.5 with the water use parameters of Schymanski et al. (2015), for a) ET, b) transpiration perennials (trees), c) transpiration seasonals (grasses), d) soil evaporation, e) GPP, f) GPP perennials (trees), g) GPP seasonals (grasses), all smoothed with a moving average of 7 days, and h) projective cover. The daily average quality flags of the fluxtower observations are shown in dashed lines with a value of 100 when a day is completely gap-filled and 1 when it is observed.*

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.

Thank you for this, we agree that there is more in this figure as we discuss. Hence, we will add a more thorough discussion on this figure, as well as a discussion about the effects of the water use strategies, as mentioned in the previous comment, and the missing groundwater table in the new runs.

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

Will be changed accordingly.

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.

Will be changed accordingly.

Technical corrections:

Line 82: Place comma between reference 3 and 4.

Will be changed accordingly.

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

We will add a reference to the first sentence.

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

We will rephrase this into: "...maintenance respiration, projected cover is linked to foliage turnover and maintenance costs..."
Note that we discuss here 1) photosynthetic capacity is linked to maintenance respiration and 2) projected cover is linked to leaf are 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.

We agree and will add these dots for readability.

Line 143: Should be "cost factor"

Will be changed accordingly.

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

We will remove this sentence based on the comments of Referee #1.

Line 226: Remove "at each site separately."

Will be changed accordingly.

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%.

We will increase the font sizes in this figure, and change the lay-out in a way that each panel contains a similar information content. In addition, we decided to split this figure into a figure with the changes in the fluxes, and a figure with the changes in the vegetation parameters (including the water use strategy parameters, related to other comments of the referee).

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.

Will be changed accordingly.

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

We will add the x-axis labels and will rename VOM to VOM-v0.5, following the comments of Referee #1.

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

We will add the layer depth to this figure.

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

Will be corrected accordingly.