General comments:

I compliment the authors on their thorough revisions to the manuscript and thank them for taking the time to run new simulations (see Fig. 4, S1.41, S1.56, S1.57) to answer my previous comments. They have addressed most of my comments thoroughly; however, I do require more clarification on the differing responses of perennial ET and GPP to soil texture (see comments on Lines 357-367 below). In addition, I have included minor grammatical comments and suggestions for making the text easier to read. I recommend this technical note for publication once these minor changes are addressed and look forward to the final product. Thank you.

Specific comments:

Line 8-9: I would replace "original results" with "original analysis of Schymanski et al. (2015)" and adjust the rest of the sentence accordingly.

Line 16: Is this an underestimation of ET and GPP compared to Schymanski et al. (2015) or to the observations? Or both? I think this means compared to Schymanski et al. (2015) as mean annual GPP is overestimated for AoB2015 and v0.5 (Lines 20-21).

Line 20-21: I would specify that GPP is overestimated by 17.8% and 14.7%.

Line 34: "…photosynthesis, water uptake and storage…"

Line 75-76: ", but an evaluation of the effect of this change on the original simulations was not included in Nijzink et al. (2021)." Were any of the effects mentioned in the above paragraphs included in Nijzink et al. (2021)? If not, I would remove this line.

Line 87: change "works" to "work"

Line 111: Remove "also" from "see also Figure 2"

Line 111-112: Run-on sentence. Add comma after "(grasses)" or rephrase.

Line 119: Replace "mol fraction" with "mole fraction"

Line 125: I would define what c_Rl is rather than saying it is a constant. I am guessing it is the cost factor for leaf respiration?

Line 127: I would define h_a and h_d rather than saying they are parameters.

Line 135: I would include the variable name and units after "Root water uptake"

Line 166: The equations for root surface area distribution are not defined anywhere. It is easy to imagine how the short term optimization works with M_a,s and J_max,25, but not with S_A,d. Can the authors either add an equation or discuss how roots are distributed in each layer?

Line 178: Where does the value of 0.22 comes from?

Line 192: Remove “also” from “see also Figure 2”

Line 197: Here and throughout the paper replace “Van Genuchten” with “van Genuchten”.

Line 207-208: I am okay with not delving into the full soil evaporation model, but would recommend a parenthetical comment referring to the section of the previous papers that explain the model.

Line 223: I appreciate the authors addressing my previous time-stepping comments. However, this conversion from daily to hourly brought up another question. Is a diurnal variation imposed when converting fluxes like temperature and radiation from daily to hourly? Maybe this is covered in Schymanski et al. (2009), but it may be good to briefly mention here. Otherwise the hourly Cowan and Farquhar calculations would not be very meaningful if the atmospheric conditions were constant over the day.

Line 263: Remove “also” from “see also Supplement S2”

Table 3: Is there a more specific name for the “water use parameters”? The exponent and multiplicative factor have the same description in Table 3.

Sect 3.1: I really like that the authors added further explanation of alterations to vegetation properties under each case as well as Figure 4. I have two (hopefully minor) recommendations for this section to help make it easier to follow:

1) I would indicate the case being discussed in each paragraph to help the reader to attach the text to Table 4. For example, in Line 334 it would be helpful to write “In contrast, changing the fixed atmospheric CO_2-levels (350 ppm) in the VOM-AoB2015 to variable atmospheric CO_2-levels (Case 2)…”
2) The authors have chosen to discuss the modifications to GPP and ET for each case separately from the modifications to vegetation properties. This can be a little tedious for the reader and may obfuscate the findings. For example, in Lines 335-339, the authors discuss how variable CO2 led to increased GPP, while 10 paragraphs later (Lines 378-382), they discuss how the variable CO2 yields larger perennial vegetation cover. The increase in GPP in the first paragraph is influenced by the vegetation modification in the second paragraph, so it makes sense to combine the two. I would recommend the authors assimilate Lines 375-402 into their respective case paragraphs in Lines 331-374. This will ease the reading by discussing each case completely once as well as make the connection between the modifications of GPP/ET and vegetation properties clearer.

Lines 357-367: I am still struggling with this explanation, which implies that increased vegetation cover and soil storage capacity benefit perennial GPP, while, simultaneously, higher suction head (lower matric potential) and decreased hydraulic conductivity reduce perennial ET. This seems contradictory as stomata are controlling both perennial GPP and ET, which means both fluxes should have a similar response (in sign at least) to changes in cover, soil storage, soil water potential and hydraulic conductivity. The only way different GPP and ET responses make sense, are if water use efficiency and/or photosynthetic capacity per leaf area changes. The authors have illustrated with new analysis (Fig. S1.56-1.57) that the new water use parameters in VOM-v0.5 do not have a large effect on this result. However, I think the answer may lie in the effect of the new soil texture on overall λ_p through the higher suction heads. The new soil suctions seem to create more efficient water use in the wet season compared to VOM-AoB2015 (Fig. S1.53d) and could lead to the different sensitivity of GPP and ET to changes in soil texture. To summarize, can the authors explain how perennial GPP and ET can have opposite responses to increased

vegetation cover/soil water capacity and decreased water potential/conductivity when they are both controlled by stomata?

Line 361-363: How does a larger soil moisture capacity and carry-over lead to higher suction heads? Is my understanding that higher suction head means more negative matric potential correct? If so, then wouldn't higher suction heads compound (and not be compensated by) the effects of lower hydraulic conductivity mentioned in Line 364? It appears from Figure S1.40c-e that although there is more water in the soil under the new texture, it will be harder for the roots to extract it due to lower water potentials and less conductivity. This intuitively explains the reduction in perennial ET even though there is greater vegetative cover, but does not address how there can be greater perennial GPP (see previous comment). Can the author's elaborate?

Lines 372-374: I am not sure the authors meant to make this a new paragraph, but it seems to be part of the previous paragraph.

Sect. 3.2: I still find the purpose of this section unclear. Currently, the section compares overall performance of the VOM-v0.5 (Case 12) and VOM-AoB2015 (Case 1) at predicting Howard Springs ET and GPP (Fig. 6). Next, the section explores the mechanisms for the model differences driven primarily by subsurface changes (Fig. 7). This section could be helped by clearly stating that you are comparing VOM-v0.5 (Case 12) used in the companion paper to the VOM-AoB2015 (Case 1) to the Howard Springs data. Then, let the reader know you are diving further into the model differences by exploring the compensating effects of the two most important factors from Sect. 3.1, soil texture and free drainage. Lastly, the primary focus in this section is on ET, but it would be helpful to say a bit more about GPP effects.

Line 405: I would write "…mean annual GPP changed from 17.8% to 14.7% overestimation." to be consistent with the ET description.

Line 414-415: What is the significance of this difference? Is it due to the fact you are comparing soil moisture at 5 cm to integrated soil moisture at 20 cm? I would expect the model to be wetter since deeper soil layers tend to be wetter than near the surface, where transpiration, soil evaporation and loss to deeper layers via gravity and suction occur.

Lines 420-425: This portion will need to be updated based on the response to Lines 361-367.

Line 429: Remove extra parentheses after "(Nijzink et al., 2021)"

Line 437 – 444: It would be helpful to reference the figures that illustrate these conclusions.

Line 447: Can the authors elaborate on why this effect is more important on highly permeable soils. I could see tighter soils having a larger capillary fringe and interacting with the root zone significantly.