Response to Referee #1

We would like to thank Referee #1 for the review, which we see as very helpful. The Referee brings forward several valid points that we will improve on in a revised version of the manuscript. Below, we address the comments of Referee #1, with the referee comments written in italics.

However, into details, I am not satisfied with the manuscript organization and the writing itself. Overall, I found there are many information currently included in the manuscript is unnecessary. The introduction is too long and contains many individual studies, which should be largely shorten with more highly-summarized findings/conclusion from existing individual studies. The detailed site description is also not needed, simply summarize the five sites with their specific properties listed in Table1. Table 2 is suggested to move to supplementary.

We will shorten the introduction, and condense it, with less individual studies, and more general findings. Similarly, we will also shorten the site descriptions. We will also move Table 2 to the supplementary information.

On to content, I think the part of dealing with water transport cost parameter is more or less deviates from the main line. I would suggest remove the second hypothesis but describe how this parameter was chosen (either prescribed following previous studies or locally parameterized) in the manuscript. Then, the overall structure of the manuscript become: 1) test the VOM using site observations and compare it with TBMs; 2) what happened if remotely sensed vegetation cover was used? 3) what happened if prescribed rooting depth was used. Followed by discussion. I understand that the water transport cost parameter is also related to the overall performance of the model, but if that is included, why not other model parameters? And also you will need to describe you model in detail to allow readers who do not familiar with the model understand the role of this parameter in the model.

The analysis of the water transport cost parameter was included, as this is the only parameter in the model, which could not be based on literature values so far. It was originally tuned to achieve reasonable results at only one of the sites, Howard Springs, and hence we find it important to investigate in how far the same value of this parameter can be used at the other sites along the transect. This is crucial for assessing the optimality theory, which is the main goal of this study. We will however explain the importance of this analysis more in the revised manuscript, and the necessity of this analysis for the interpretation of the other analyses.
We also believe that our current structure is not much different as the Referee suggests: 1) test the VOM using site observations and compare it with TBMs 2) find the underlying reasons for deviations with a) what happens if we vary the unknown parameter for the water transport cost? b) What happens if remote sensed vegetation cover was used? c) What happens if prescribed rooting depth was used? As the water transport cost parameter is the most uncertain in the entire model, it only makes sense for us to assess this parameter first, and then expand the analysis in order to find more underlying reasons for deviations from the observations.
Regarding the model details, we would like to refer to the accompanying technical paper in GMD (https://doi.org/10.5194/gmd-2021-151), as well as the previous papers with more details about the VOM.

Another question is why not include a scenario that consider both prescribed vegetation cover and rooting depth, in comparison to the scenario with both vegetation properties optimized.

This is a good idea, we will conduct the suggested simulation and present it either in the main paper or in the SI, depending on the importance of the insights gained by it.

Other comments:

Line 215ï1⁄4infiltrate -> infiltration

Will be changed accordingly.

Line 216: Why 30m? Is this the defined soil depth in the model? Not sure if the choice of this depth impacts the modelling results.

The 30m was chosen in order to represent freely draining conditions, with deep groundwater tables. See also the accompanying technical paper in GMD, where we found there is a strong influence. We will also clarify this here.

Line 225-230. This may present a source of uncertainty, as the observed fluxes are directly linked to the observed meteorological forcing at the sites, whereas the SILO data was used here to inform the model. Suggest to at least evaluate the used SILO data at each site against site-observed meteorological variables during their overlapping periods.

Please see Supplement S4 of our accompanying technical paper in GMD (https://doi.org/10.5194/gmd-2021-151). We found that replacing the daily meteorological data from SILO, with aggregated daily data from the flux towers, did not lead to strong differences in the results. We will clarify this here in the main manuscript.

Line 223 Is there any published paper supporting this? Otherwise, simply states this information is measured at each site.

We believe the referee means Line 232, where we will clarify the source of the soil data.

Line 260-263 and the following sections. If I understood correctly, the last two hypotheses are related to replacing vegetation properties with prescribed values and the second hypothesis is about water transport cost parameter. Please check.

We will correct this.

Line 246 and throughout: evapotranspiration is often written without a hyphen.

We are aware of this, but this is done on purpose. We try to emphasize here, that it actually involves two different processes: evaporation and transpiration. Hence, this is merely an abbreviation. See also our statements in lines 249-251.

Line 316-317: Where is the evidence for this? Figure 3. Please indicate where needed. In addition, in figure 2 at Howard Springs (but not for all other sites), there is a light green curve indicating the results of Schymanski 2015. What is the difference is model configuration between Schymanski 2015 and this study? And what is this for? It is not introduced.

This can be seen in Figure 3b, we will add a reference in the text.

We will also remove the lines related to Schymanski et al. (2015), as this analysis was moved to the accompanying technical paper in GMD.

Line 400. This is an overstatement. Looking at Figure 2, the VOM considerably overestimates GPP from observation and even compared with other TBMs.

We will rephrase this, we tried to explain in this paragraph that the VOM did not substantially worse or better than any of the other models.

Line 500. I do not agree with this hypothesis/statement. This may simply caused by the fact that the adopted VOM was not able to reproduce the actual rooting depth using the embedded optimality principles. Many previous studies have already demonstrated the importance of accurately representing rooting depth in the hydrological model to improve the modelled fluxes, for example, those by Kleidon and Heimann (1998) and more recently by Wang et al. (2016) and Yang et al. (2016).

Kleidon, A., and M. Heimann (1998), A method of determining rooting depth from a terrestrial biosphere model and its impacts on the global water and carbon cycle, Global Change Biol., 4(3), 275–286.
Wang-Erlandsson, L., W. G. M. Bastiaanssen, H. Gao, J. J€agermeyr, G. B. Senay, A. I. J. M. van Dijk, J. P. Guerschman, P. W. Keys, L. J. Gordon, and H. H. G. Savenije (2016), Global root zone storage capacity from satellite-based evaporation, Hydrol. Earth Syst. Sci., 20(4), 1459–1481.
Yang, Y., R. J. Donohue, T. R McVicar (2016), Global estimation of effective plant rooting depth: Implications for hydrological modeling. Water Resources Research, 52, 8260-8276.

We fully agree that it is important to accurately represent rooting depths, but we formulated the alternative hypothesis in our paper, which we intended to reject. We will formulate this part more clearly and also discuss the suggested references.