Response to Referee #2

We would like to thank Referee #2 for the review, and will improve on the points brought forward by the referee. Below, we address the comments of Referee #2, with the referee comments written in italics.

1. Hypothesis 1: One of my major concern concern it that the TBMs (or LSMs) in Whitley et al. (2016), which are designed for global-scale, long-term climate projectoin models, are optimized to make a fair comparison to VOM, though they have more specific vegetatioin parameters.

We did not re-run any of the TBM simulations of Whitley et al. (2006) in order to compare to the VOM, we re-used the model results of Whitley et al. (2016), where the TBMs were applied independently of the VOM. Of course, the modellers involved in the study of Whitley et al. (2016) applied different levels of site-specific parametrisation, and we do not know in how far any parameters were optimized in the original study. With this hypothesis, we mainly try to assess where our optimality-based model stands, and if it still gives satisfactory results in comparison with models that were applied in what the specific modellers considered the “best” way possible.

2. Hypothesis 2 is too model specific. Does any other TEMs use this paramter? or what would other models learn from this? or does this hypothesis have any implication for plant adaptation or optimality?

Plant hydraulics is currently being implemented in TBMs as a major limitation for water use during drought, whereas in the VOM, it is only represented in the form of the water transport cost factor. The intention of this hypothesis is to test whether a general water cost factor across sites leads to reasonable results or if site-specific cost factors would yield significantly improved results. Therefore, assessing this cost factors is also highly necessary in order to assess the general concept of the optimality theory, as applied here. We will clarify this in the revised version.

3. Hypothises 3 is also model specific. It depends on model representations of the links between LAI, vegetation cover, light transfer and absorption, assimilated carbon allocation. This mansusript does not clear describe how these processes are represented in VOM.

We will clarify in the revised manuscript that the prognostic simulation of phenology (LAI and vegetation cover dynamics) is a central concern of vegetation models and optimality theory. We also assess here how the optimality theory, as applied in the VOM, can be improved, and therefore we should also assess the prognostic simulation of the projective cover. As requested, we will explain in more detail how phenology is simulated in the VOM and hence provide a better context for this hypothesis. Thank you for pointing out this gap.

4. Hypothesis 4 shoul be the focus of this manuscript. If this would have been well tested, it is enough to be a good paper. However, this would be greatly affected by the rerepsentation of subsurface soil moisture profile and further likined to capillary fringe. I am wondering why the authors did not use the results of Schymanski et al. (2015). So I strongly suggest to include groundwater effects.

We agree with the referee that groundwater effects are important, but none of the TBMs treated in Whitley et al. (2016), considered them, so we did not in this paper, either. However, we would like to refer to the accompanying technical note in GMD (https://doi.org/10.5194/gmd-2021-151), where we

more thoroughly discuss the effects of the groundwater tables and present a systematic comparison with the results of Schymanski et al. (2015).

5. It is not clear to me (I have to read also Schymanski et al., (2015)) that how VOM is optimized. "Maximizing the NCP"? what is the maximum NCP? how do we know the maximum NCP? Please expand Section 2.2.4 a bit to describe this process in model detail.

Thank you for pointing out this lack of clarity. We will add more details in this section. More specifically, the SCE-algorithm samples the long-term vegetation properties. With this, the VOM is run, and over the full period, the total CO2-assimilation minus the total carbon costs represents the Net Carbon Profit. The vegetation properties that lead to the highest NCP, after sampling and going through the full parameter space with the search algorithm, are kept and considered as the optimal vegetation properties.

6. Section 2.3.3, the cost factor for water transport (crv) should be described here. If not reading Schymanski et al., (2008), I did not understand its meaning.

We will add a specific paragraph describing the cost factor for water transport. In lines 122-129, we introduced the challenges related to this cost factor but we will also explain more in 2.3.3.

7. Conclusions: please generalize these conclussions through discussions. I do not care much about how VOM is better or not or how to improve it but more about how to implement improved understandings through VOM studies into the current TBMS that arewidely used in IPCC climate projections.

Point taken. We will re-write the conclusions from the perspective of general benefits of optimality modelling, and add more general implications of our findings. We will emphasize the identified deficiencies and possible improvements of the VOM, and relate them to the general understanding of the applied optimality theory.