We thank the reviewer for further comments, which improve the quality of the manuscript.

Review of Denissen et al., round II

I thank the authors for addressing each of my questions and suggestions in detail and agree with their responses, and therefore only reply to selected points below.

3.) [....]

We agree with the reviewer. We have inserted the multi-model mean incoming shortwave radiation trends in Figure 1b and show model-specific incoming shortwave radiation trends in Supplementary Figure 4.
We have elaborated on incoming shortwave radiation trends in the following lines in the results section (lines 181 – 186 and 204 - 206).
- Lines 181 – 186 “There is a widespread increase in incoming shortwave radiation in about 71% of the warm vegetated land area, with high inter-model agreement (Supplementary Figure 4), which can directly affect near-surface temperature through the surface energy balance. These trends could result from projected decreases in aerosol emissions (Nabat et al., 2014), or from changes in cloud cover. As daily maxima of incoming shortwave radiation roughly co-occur with daily temperature maxima, increased incoming shortwave radiation links more strongly to increased in maximum temperatures rather than mean temperatures (Qian et al., 2011), which are more strongly governed by the longwave radiation budget.“
- Lines 204 - 206 “Further deviations from a positive relationship between temperature excess and ELI might result from alternative processes such as (changes in) advection of warm air masses through large-scale circulation patterns and changes in incoming shortwave radiation (Supplementary Figure 4).”

I suspect the response to my comment is no longer consistent with the revised manuscript, as I cannot identify any downward shortwave radiation trends in Fig. 1b. I have two more remarks here, (i) “These trends could result from projected decreases in aerosol emissions (Nabat et al., 2014), or from changes in cloud cover.”, these effects could be separated by comparing all-sky (rsds) to clear-sky (rsdscs) downward shortwave radiation trends, but I don’t think this is necessary for the presented analysis. (ii), currently, the reader is informed that the link between ELI and temperature excess could be partly masked by “alternative processes such as [...] changes in incoming shortwave radiation”. Since this is a purely correlational analysis, however, I think it is also possible that we overestimate the role of ELI in causing temperature excess for the same reasons, since shortwave radiation definitely matters quite a bit as a driver of maximum temperature (e.g., Schwingshackl et al., 2018). I would like to leave it up to the authors whether they mention this caveat, and thank them for incorporating a downward shortwave radiation analysis.

(i) We agree with the reviewer here, and although further disentangling these drivers is interesting, it is out of scope for this paper.
(ii) We adapted the following sentence on lines 204-207:

“Further deviations from a positive relationship between temperature excess and ELI might result from alternative processes such as (changes in) advection of warm air masses through

large-scale circulation patterns, while positive relationships could be exaggerated by changes in incoming shortwave radiation (Supplementary Figure 4)."

- Some citations should be double-checked; e.g., "(Eyring et al., 2016))" comes with an additional right bracket.

All double brackets were checked and removed if possible.

Really not trying to be pedantic here, but I came across some new ones while reading the revised manuscript:

L. 69: ",(Denissen et al., 2020)),"
L. 308: ", (Denissen et al., 2022; Seneviratne et al., 2010)):"

We apologize for overlooking the remaining double brackets. We have replaced them as follows:

L. 69 "...a recently introduced ecosystem water stress index: the Ecosystem Limitation Index, or ELI (Denissen et al.,2020). This is a correlative …"
L. 308 "... the nonlinear relationship between soil moisture and EF (Denissen et al., 2022; Seneviratne et al., 2010).

Additional comments:
- Fig. 4: I am a bit puzzled by the fact that, although the observation-derived estimate (ERA5-Land) shows rather stark ELI changes compared to the CMIP6 model subset, this doesn't manifest with regards to temperature excess. As such, I am not sure whether this newly introduced 'temperature excess' is something that should already clearly emerge from historical, observation-based data. Of course, internal climate variability likely plays an important role here, but it seems quite difficult to reconcile these results given that for 4 out of 5 regions, ERA5-Land leaves the model envelope with regards to ELI, but stays well within concerning temperature excess except for the one region (NAM) where there is only a weak ELI signal. Based on this, I would argue that the picture is quite clear with regards to model projections until the end of the ongoing century,, but far less so in terms of historical data.

We agree with the reviewer here and address the difference in coupling between ELI and temperature excess in lines 267-273:

"In this historical time period and across most regions of interest, the CMIP6 trends for both temperature excess and ELI are generally more positive than negative, which corroborates a positive relationship between the two, as is also seen further into the future (Figure 3). This relationship is weaker in the observation-based estimate from ERA5-Land, where temperature excess mostly stays within the multi-model envelope and only increases monotonically in SAM, while ELI exceeds the multi-model envelope and increases in all regions of interest except NAM. This indicates a different coupling between ELI and temperature excess in ERA5-Land than in the CMIP6 models, which should be further investigated in the future."

- Fig. 4a, legend: it still says ERA5

Adapted to “ERA5-Land”

- Fig. 4b: units (K) seem odd

Adapted to “(-)”