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.

- 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)):”

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.
- Fig. 4a, legend: it still says ERA5
- Fig. 4b: units (K) seem odd