Review comments on the revised version of the manuscript

Exploring precipitation pattern scaling methodologies and robustness among CMIP5 models

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submitted to Geoscientific Model Development.

Recommendation: Accept with minor revisions.

In my opinion, there has been substantial progress in the paper, but publication in Geoscientific Model Development would still require some revisions.

1 Specific comments

1. P. 8, l. 18–31: You examine the non-CO2 pattern here, but the procedure of calculating that is only presented in subsection 4.2. (Note that the caption of Fig. 8 also refers to subsection 4.2.)

2. P. 9, l. 9: Being a small difference of two large quantities, $4P_{RCP8.5} - 3P_{CO2}$ is sensitive to noise occurring in these quantities. This has been mentioned previously, but this expression would elucidate the issue particularly well.

3. In many journals, sections and figures in the Supplement file are numbered as S1, S2, etc. Check the convention of the present journal. If numbering is changed, also remember to update references to these figures and sections, both in the main text and in the supplement.

4. Supplement, p. 2, l. 9–11: Please check the correctness of that sentence. It is Fig. 6 that uses the "wrong" P while in Fig. 5 ΔT is extracted from the other group of models. If ΔT indeed is virtually similar for both groups, should the error be small just in Fig. 5?

5. I am not quite sure whether the discussion in section 4 of the Supplement is necessary. If the aim is to show that no universal non-CO2 pattern can be constructed, perhaps there would be more illuminating ways to show that. More detailed comments (only to be considered if this section will be retained):

   (a) P. 3, l. 25: Eq. (13) is a duplicate of (7). Perhaps it is not necessary to repeat, refer to (7).

   (b) It would be consistent to use the same amount of decimals in Eqs. (7) and (14). The coefficients would then take the form: 4.0, 3.0, 2.9, 1.9.

   (c) Eq. (20): state that this is only an assumption that you have employed (not an universal truth).

   (d) P. 4, l. 12: Does "the previous expression" refer to Eq. (18)?

   (e) I am afraid that there is an arithmetic error in deriving the equation given on p. 4, l. 12–13.

   (f) P. 4, l. 23: rationalize in more detail "which implies that"; moreover, specify what is the assumption that is violated.

2 Minor comments

1. P. 7, l. 14: Please specify whether “one method” refers here either to the epoch difference or regression method. If not, the text should be clarified.

2. P. 8, l. 2: Do “nonlinearities” refer to a nonlinearity of the response with respect to global mean temperature change?

3. P. 9, l. 16–19 (several occasions): Should $\overline{T}$ be replaced by $\Delta\overline{T}$? This issue also concerns the captions of Figs 8, 10, 11 and 12.

4. P. 9, l. 24–32: Here, it might be useful to refer to the rms differences given in Table 3.

5. Caption of Fig. 8: Should $\overline{T}$ be replaced by $\Delta\overline{T}$, and there be indices CO2 and non-CO2 for it?

6. Supplement, p. 3, l. 8: specify the years of “the latter part of the simulation”.