

Dear Dr. Adloff,

Thank you for submitting your manuscript to Earth System Dynamics and for providing responses to the points which the two referees raise. On the basis of the critics by the referees alone, one may (have to) decide to reject the paper in its current form for publication. The use of the terms “sensitivity”, “feedback”, “gain”, and “feedback factor” would need improved clarification. It is a good idea to follow the corresponding terminology of the IPCC where possible and to describe where deviations from this terminology are made. Some of the confusion unfortunately is intrinsic to the climate feedback discussion, because in Hansen et al. (1984) and many subsequent papers of other authors the terms “gain” and “feedback factor” are used in the opposite way as originally phrased in electrical engineering. See Roe (2009) for a sound discussion of the feedback terminology. I discussed the manuscript with two further experts in the field to arrive at a conclusion. Their independent response was more positive than that of the two reviewers. The authors make constructive suggestions for improving the manuscript. My own impression is that the paper addresses a basic simple question, which should be clarified at some point. I think the underlying idea is a good one and a novel one. Weighing all factors, I would like to give the authors a chance to make a major revision of their manuscript. This decision I do with some reservations, because only a substantially improved manuscript would have a chance to pass a new round of reviews.

Some further recommendations resulting from discussing this manuscript:

The use of terminology is partly wrong (“climate sensitivity” as pointed out by the reviewers), or inconsistent with existing literature (“fertilisation” and “radiation” effects are usually referred to as carbon-concentration and carbon-climate feedbacks). It would be better if the authors stick to these well-established terms.

Importantly, the equation (6), and consequently (7) and (8) cannot be used here, because this would require the ocean feedbacks (also pointed out by #2). Therefore, Fig. 8 also does not make sense. One could argue that I_{ext} in (6) was the compatible emissions if the ocean sink was absent, but I am not sure whether this would make sense. In any case this would need a more detailed explanation and justification.

Page 4, line 14-15, and later page 7, line 13: The finding that $(\Delta C_{fert} + \Delta C_{rad}) = \Delta C_{full}$ is a very model specific feature. Both, larger and smaller sums have been found (see e.g. Gregory et al. 2009, cited).

The fact that $\Delta T_{rad} < \Delta T_{full}$ is a well known feature (see also Gregory et al. 2009 and references therein), and this needs to be mentioned.

If you decide to submit a revised version of your manuscript, please, take all points of the referees and these mentioned here into account. I would then send out this manuscript for a new review round. Thank you for using the open access way for publishing your work.

Best regards, Christoph Heinze

References:

Hansen, J., A. Lacis, D. Rind, G. Russell, P. Stone, I. Fung, R. Ruedy, and J. Lerner (1984), Climate Sensitivity: Analysis of Feedback Mechanisms, in *Climate Processes and Climate Sensitivity*, edited by J. a. T. Hansen, T., pp. 130-163, American Geophysical Union, Washington, DC.
Roe, G. (2009), Feedbacks, Timescales, and Seeing Red, *Annu Rev Earth Pl Sc*, 37, 93-115, doi: 10.1146/annurev.earth.061008.134734.