

Interactive comment on “Earth system model simulations show different carbon cycle feedback strengths under glacial and interglacial conditions” by Markus Adloff et al.

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We thank the reviewer for his/her comments on the submitted paper. We seem to significantly disagree on the rationale behind our study and hope for clarification in this regard to make the discussion more constructive. Language and writing style are crucial in any publication and we are very concerned to read that language made it difficult for the reviewer to understand the submitted paper. We will review our paper in this regard, for which the detailed reviewer's comments will be helpful, and make sure to collaborate with native speakers as suggested by the reviewer. In the following, we address the three main content-related comments individually.

ad (1): Our paper is NOT framed around climate sensitivity, but around carbon cycle feedbacks. Indeed we talk also about climate sensitivity but from the experiment design it should be clear that this is not the equilibrium climate sensitivity defined by the IPCC and we do not employ this term in our paper. In fact, what we are looking at are transient sensitivities, of the climate system as well as of the terrestrial carbon cycle specifically. Transient sensitivities are well introduced in the climate community, but the term transient climate sensitivity is not used by the IPCC – the latter uses instead the term 'transient climate response' (IPCC AR5 Glossary). We understand from the reviewer's comment that we should not shorten 'transient climate sensitivity' to 'climate sensitivity' to prevent confusions. We also want to point out that in the definition of climate sensitivity in the context of carbon cycle feedback studies (see Friedlingstein et al., 2003 eq. 3, Friedlingstein et al., 2006 and Arora et al., 2013 p. 5293) a linear dependence of the sensitivity on CO₂ is assumed.

ad (2): The essence of the C4MIP protocol is in our opinion not whether simulations are concentration or emission driven, but the definition of sensitivities on the basis of three differently coupled transient simulations. This is also the IPCC view (Ciais et al., 2013, Box 6.4): in the framework of CMIP5 the C4MIP project has performed concentration AND emission driven simulations to derive the respective sensitivities. Moreover, we make very clear in the paper that we consider concentration driven simulations, e.g. on page 3, lines 11-12: "we follow the C4MIP experimental design (Ciais et al., 2013, Box 6.4) for concentration driven simulations", where we even emphasized this point by using italics. Concerning the reviewer's conclusion that our presentation shows "how little of the literature the authors appear to have read" we want to remark that such a broad statement is rather offending: It would be appropriate if the reviewer would point out more clearly and explicitly if he/she feels that a particular reference is missing or has not been sufficiently acknowledged.

ad (3): We disagree with the reviewer that LGM studies are only good for "better understand(ing) how physical and biogeochemical feedbacks combine to magnify a tiny

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change in the distribution of sunlight into the glacial-interglacial cycles". Besides the fact that a better understanding of the transient sensitivities of the terrestrial carbon cycle could improve our understanding of the Earth system's reaction to external forcings, we think that also other questions are of interest, namely to what extent feedbacks as quantified in the C4MIP way are different with different background climates. On the whole, we don't understand the basis on which the reviewer suggests where our interest should be in the first place. To get a more constructive feedback it would help us if the reviewer could explain why our research question is not properly stated or even not worth it studying.

From the comments of the second reviewer we understand that we caused confusion by referring to Friedlingstein et al. 2003 in our methods section. Indeed, the concept of carbon cycle sensitivities and the carbon cycle feedback originated from Friedlingstein's work but Gregory et al. 2009 altered the definition particularly of the carbon cycle feedback to include the overall feedback from the carbon cycle and not only its radiative part. Our calculations follow Gregory's altered definition to quantify the overall feedback. In the re-submitted paper, we will make sure to avoid this confusion.

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