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Interactive comment on "A modified impulse-response representation of the global response to carbon dioxide emissions" *by* Richard J. Millar et al.

Anonymous Referee #1

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The paper presents a modification of the impulse response function model of Joos et al. (2013) used in the last IPCC report to convert CO_2 emissions into atmospheric CO_2 concentrations. The modifications consist of (i) making the "decay" time parameters τ_i dependent on model state, and (ii) changes in model parameters. After determining the new parameters, the new model (which the authors call "FAIR") is exposed to several tests (response to different sizes of emission pulses, comparison with historical atmospheric CO_2 development, comparison with results from two other carbon cycle models (BEAM, MAGICC)). In addition the authors demonstrate that by varying model parameters, ensembles of scenarios can be constructed with their model that account for the IPCC likely range of transient climate sensitivity.

The authors motivate the need for their modified impulse response function model by claiming "*This extension is necessary because the use of a state-insensitive impulse-response model cannot simultaneously reproduce the relationship between emissions, concentrations and temperatures seen over the historical period and the projected response over the 21st century to both high-emission and mitigation scenarios estimated from more complex models.*" (p. 2, lines 23ff). While it is true that the Joos et al. (2013) model is not accounting for a state dependence, it got not really clear to me from the paper whether this unability of 'simultaneous reproduction' is true or not. But anyway, since this claim is essential for justifying the study as a whole, this point had to be demonstrated right at the beginning of the paper. But such a demonstration is missing. Accordingly, for me the authors failed to make clear why I should read their study.

Moreover, the evaluation of the new model remains vague. Once parameters have been found for their new model (p. 4, lines 6-8) (how have those parameters been determined?), the results of several impulse experiments are discussed in section 3, resulting in questionable claims on the quality of their new model like "*consistent with corresponding ratio in the most detailed ESMs*" (p. 6, lines 23f) (what means 'most detailed'?) and "*the FAIR model can capture the dependence of the pulse-response on pulse size*" (p. 6, line 28) (what means 'capture'? In comparison to what?) or "*it is encouraging that the FAIR model shows a close correspondence with a well-known and well-used simple model [=MAGICC] that has been used extensively to emulate the response of ESMs*" (p. 8, lines 7f) ('encouraging' is nice but not convincing). Generally, a clear strategy for model evaluation is missing. In particular, it is not well specified which model simulations are meant to be emulated by FAIR – the specification in the abstract remains vague ("*several idealised experiments performed with more complex models*"). If I understand the intention of the study right, those models contributing to the carbon studies in CMIP5 are meant to be well emulated by the new FAIR model. But this model ensemble is not showing up in the plots (except a few graphs including results from BEAM and MAGICC). Data for comparison would have been available in international archives, and if also pulse experiments with these models would have been necessary for comparison, Joos et al. could have been contacted for data from their 2013 study.

Generally, I think that the state dependence is an interesting addition to the original impulse response model, but the evaluation of the resulting new model is lacking rigor. I guess that by making the study more targeted and rigorous, the resulting paper would be very different. Considering all this, I suggest to reject the paper.