

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

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[12pt, letterpaper]article

Despite the reviewers recommendation we believe that the points raised can be relatively simply dealt with and would hope the making the changes outlines through the in-line responses to the specific points raised below would change the reviewers recommendation.

The authors motivate the need for their modified impulse response function model by

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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 inability of ‘simultaneous reproduction’ is true or not.

Figure 4 shows that the AR5-IR model (red) fails to reproduce observed concentrations over the historical period when integrated with historical emissions (panel a) and fails to reproduce historical emissions when emissions consistent with historical concentrations are derived from the model (panel b). A substantial error of nearly 30 ppm is seen in figure 4a, representing a large difference from the correct climate state. We also show in figure 5 how the AR5-IR model fails to produce similar behavior to the MAGICC model under the RCP8.5 and RCP2.6 scenario. The MAGICC model is approximately consistent with the response of the ESMs in CMIP5. We agree with the reviewer that this point could be made more prominently and would emphasize this point more in the introduction and change the ordering of our results section in a revised version of the manuscript. We also propose to introduce a new figure to demonstrate that the problem with the AR5-IR model is structural: despite the number of tunable parameters in this model, no combination of parameters can simultaneously reproduce the historical period and projected changes over the 21st century without the introduction of a state dependence. We will post a draft version of this figure on this discussion as soon as possible.

“it is encouraging that the FAIR model shows a close correspondence with a well-known

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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).

We agree with the reviewer that agreement between two simple models of the climate system is not a sufficient evaluation of model skill, but given how extensively the MAGICC model is used, an ability to reproduce its behaviour might be argued to be a necessary condition for any simpler model. We will clarify this in revision.

In particular, it is not well specified which model simulations are meant to be emulated by FAIR.

We intend for the FAIR model to reproduce features of the climate response to CO₂ as shown in ESMs (e.g. p. 2, l. 19-20). We have often referred to “more complex/comprehensive models” in the text to indicate that we are attempting to emulate behavior simulated by both ESMs and EMICs (e.g. p. 2 l. 4) but we agree that this wording is ambiguous, particularly in the abstract, and could be easily rectified in a revised version of the manuscript.

We propose to add an additional figure demonstrating how the FAIR model can be used to emulate the results of more complex models in the Joos et al (2013) model inter-comparison, but it must be emphasized that this represents only one of a large range of possible tuning exercises. No simple model or emulator should be assumed to be a substitute for more comprehensive models simply because it reproduces their behavior under a selected scenario or experiment: our objective here is to demonstrate that the FAIR model is potentially able to reproduce the results of more complex models, but for

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any particular application, the model should be tuned to relevant simulations. Hence we would argue that providing “definitive” parameter values for the FAIR model would be misleading. We will post a draft version of this figure on this discussion as soon as possible.

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’?)

We intended consistency to refer to the quantitative statement in the following clause, namely, “with its value of 36 years within the 34-47 years range of the ESMs”. We agree with the reviewer that the word ‘detailed’ adds no extra information, introduces confusion and can be simply removed in a revised version of the manuscript. We agree with the author that adding ranges of the pulse responses in ESMs from Joos et al (2013) on figure 3 would aid the discussion of figure 3 in the text, and could be easily implemented.

“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?)

We will replace the word “capture” with the word “reproduce” if that is clearer. A note will be added to the text on p. 6 to indicate that Joos et al (2013) found the iIRF100 to decrease by 40% between the 5000GtC and 100GtC preindustrial baseline pulse (this

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would be in addition to the ranges of response in Joos et al being added to figure 2 as discussed above).

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