Geoscientific
Model Development
Discussions

Interactive comment on "GIR v1.0.0: a generalised impulse-response model for climate uncertainty and future scenario exploration" *by* Nicholas James Leach et al.

Anonymous Referee #3

Received and published: 3 April 2020

I read the manuscript with great interest, but I am afraid to say that the new model that the authors presented seems to me a combination of existing models, or an expansion or a generalization of the FaIR model. The novelties do not come very clear to me throughout the manuscript unfortunately. To begin with, the carbon cycle is essentially the same with the one in FaIR, except for some changes in feedback-related parameters (Table 1). The table indicates that the CH4 and N2O gas cycle representations in GIR are more complicated than in FaIR, but these are already considered by other SCMs like MAGICC. The forcing equations for three gases are either the Etminan parameterizations or their simplification without gas interactions. The climate model is the Tsutsui model (3-box) published before, in comparison to a 2-box model in FaIR.

The whole things above left me wonder how come the model deserves a new name. Is this a marketing strategy to sell the model again? In my eyes, the model appears like a re-tuned version of FaIR. It is not my intention to make it ironic, but the only reason to justify the new name can be to avoid using the name "FAIR" any more, which was previously used to call a simple climate model developed by a different group (den Elzen and Lucas 2005; den Elzen and van Vuuren 2007).

Now, from a different angle, I would think that the model would be an innovation if it is really simple and workable. But the current manuscript indicates that this does not seem to be the case. The authors claimed so by emphasizing that the model can be expressed just in six equations, so deserved a new name (Lines 59-60). The model appears simple at surface, but a closer examination easily reveals that the equations are aggregated at a general level, hiding the complexity. In fact, equation (3) is very complicated, and its physical interpretation is not obvious. I doubt that general users that the authors intend to reach out appreciate this equation.

At multiple places in the manuscript, the authors insist simplification, e.g. "our core aim of simplicity" in Line 221. If this is a guiding principle for this model, the simplification should be more strongly enforced and the model should be designed accordingly. But if I don't get it wrong, the current model is actually more complicated even than FaIR because there are more parameters and feedbacks for CO2, CH4, and N2O gas cycle in GIR and also because the climate model has now three boxes (two boxes in FaIR). The authors certainly separate the gas cycles "to simplify" by removing the interaction of CH4 and N2O forcing and the CH4-O3 interaction. But gas cycles are still indirectly linked through seemingly complicated temperature feedback in equation (3). If simplification is really a guiding principle, the authors need to embrace it more and think further what the minimum representation to adequately represent the global response of the earth system to greenhouse gas emissions is (in Line 100, authors refer to the Supplementary Information for such discussion, but I was not able to find it). This question has been asked by many simple climate modelers, but the answer might

be different now, given the latest knowledge and the current political situation after the adoption of the Paris Agreement. If the intended model use is limited to Paris-relevant low temperature stabilization pathways, certain feedbacks and model features may not be needed, which simplifies the model.

I have a general impression that the discussion in this paper is placed in a narrow range of papers. Many SCMs exist, but throughout the paper the authors do not really discuss SCMs other than FAIR and MAGICC. Where relevant, the paper should touch on other SCMs and their model features including but not limited to ACC2, BernSCM, CICERO SCM, Hector, OSCAR, and WASP. Also the SCM built in DICE should also be incorporated in the discussion. In my view, some innovation claimed by this paper (e.g. see my comment on L 48 to 60) is a result of the ignorance of other previous papers. The discussion needs to be widened in scope.

In summary, a substantial amount of work is required to revise the paper, potentially including further tuning or development of the model. My judgement is that this manuscript should be rejected, with an opportunity for resubmission. I provide further comments below. But the comments are not given comprehensively because I expect that the paper will be in a completely new form after revision. I am sorry that I cannot be positive in this review.

Further comments

L 36 to 46: The discussion in this paragraph seems to contradict with the statement in the abstract: "other methods would be equally valid." This also contradicts with the fact that MAGICC has been solely used in some previous IPCC WG3 Assessment Reports. The issue has been rather the dominant use of MAGICC, whose codes are not publicly available. The authors could push GIR to be used for assessments. But this should not be privileged to GIR. This should be open to other models complementary. I therefore disagree with the idea of one common SCM.

L 48 to 50: The model equation to calculate GHG metrics has been transparent in

previous IPCC Assessment Reports, to my knowledge. I disagree with the statement "that model was not quite adequate to reproduce the evolution of the integrated impulse response to emissions over time." See Joos et al. (2013).

L 48 to 60: It is unclear what "all of these innovations" are. Innovations need to be discussed in a wider context of previous studies. For example, the non-linearity of the carbon cycle has been introduced by Joos et al. (1996); Hooss et al. (2001).

L 85: I don't think that general users would understand this equation. This is explained in Lines 93-95 by citing Millar et al. (2017), but this needs elaboration.

L 100: I cannot find the discussion on the adequacy of this analytic form in Supplementary Information.

L 114-115: I cannot find the result that the authors refer to.

L 134 to 135: Many international assessments (e.g. CCAC) indicate that the CH4 and O3 interaction is very important for climate and clean air policies. If the model drops this interaction, this needs to be done more carefully with an extensive set of sensitivity analyses to find out what the limitations are. Many SCMs capture CH4-O3.

L 206: In Fig 2, the uncertainty range for N2O is not shown.

L 233 to 235: If this model is made public, some people would use it for RCP8.5 by forgetting (or ignoring) that the model is tuned only Paris-relevant scenarios. This tuning strategy may be risky.

Supplementary Information Table 1: Is this a common way to describe the unit for N2O?

References den Elzen MGJ, Lucas PL (2005) The fair model: A tool to analyse environmental and costs implications of regimes of future commitments. Environmental Modeling & Assessment 10:115-134

den Elzen MGJ, van Vuuren DP (2007) Peaking profiles for achieving long-term temper-

ature targets with more likelihood at lower costs. Proceedings of the National Academy of Sciences 104:17931-17936

Hooss G, Voss R, Hasselmann K, Maier-Reimer E, Joos F (2001) A nonlinear impulse response model of the coupled carbon cycle-climate system (niccs). Clim Dyn 18:189-202

Joos F, Bruno M, Fink R, Siegenthaler U, Stocker TF, Le Quélé C, Sarmiento JL (1996) An efficient and accurate representation of complex oceanic and biospheric models of anthropogenic carbon uptake. Tellus B 48:397-417

Joos F, Roth R, Fuglestvedt JS, Peters GP, Enting IG, von Bloh W, Brovkin V, Burke EJ, Eby M, Edwards NR, Friedrich T, Frölicher TL, Halloran PR, Holden PB, Jones C, Kleinen T, Mackenzie FT, Matsumoto K, Meinshausen M, Plattner G-K, Reisinger A, Segschneider J, Shaffer G, Steinacher M, Strassmann K, Tanaka K, Timmermann A, Weaver AJ (2013) Carbon dioxide and climate impulse response functions for the computation of greenhouse gas metrics: A multi-model analysis. Atmospheric Chemistry and Physics 13:2793-2825