Response to Reviewer 2, 02/08/2020

Our response is given in standard typeface, with the original review in italic.

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.

We respect the point that the reviewer is making here, and have a resolution for the revision: the model we present will be an update to the FaIR model, rather than a separate model. We believe that this reduces the confusion over the justification and implementation that GIR and FaIR shared. This update to FaIR, as discussed in our original submission, aims to make the model structurally simpler and more transparent, while still retaining the climate projection and emulation ability of the previous version.

While full specification of FaIR v1.5 and MAGICC7.0.1-alpha would involve a reasonably long list of equations detailing all of the parameterisations that differ by gas/aerosol species (see e.g. Appendix A of Meinshausen et al., 2011), here we provide a framework in which you can parameterise all of the key components with a single equation set. It is true to say that if a reader understands the carbon cycle within the model, then they understand every gas cycle, since the feedback parameters and equations are identical. This identical treatment of all gases is what enables the model to a) run extremely quickly and b) be converted into almost any programming language, including excel; something that is not true of FaIR v1.5 or MAGICC7.0.1-alpha. It is this same structural simplicity that should allow anyone with a reasonable working knowledge of a programming language to code up their own version of the model, rather than relying on the code we have written ourselves; attempting to do this for MAGICC7.0.1-alpha or FaIR v1.5 would require a great deal more effort than it does for this 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.

There are papers/projects in press (such as the Reduced Complexity Model Intercomparison Project, RCMIP) which attempt to do a thorough comparison between the full range of simple climate models; this is not our aim with this paper, and we feel it would end up unnecessarily lengthening the paper. Our aim is two-fold: 1) to provide a simple climate model which is straightforward to implement in a wide range of settings, and simple to understand due to the minimal equation set, and 2) to produce and emulator which can behave to within a reasonable approximation of any other model (SCMs, MICs and GCMs).

We argue the model presented fills a gap not adequately filled by the SCMs available today. Many of the models mentioned are significantly more complex than FaIR v2.0. While several of them simulate features that cannot be simulated with FaIR v2.0 (such as computing carbon fluxes into specific sinks), they can also not be written down in just 6 equations and coded up in just a few hours. This is the gap that we feel FaIR v2.0 occupies, something we shall try to make clear in the revision.

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.

Our revised paper is significantly different and improved compared to our original submission. This review was extremely informative in terms of guiding what we could improve upon in the revision.

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.

The statement in the abstract refers to the default GIR parameterisation procedure rather than the model used.

It has been argued that there are fundamental differences in the response of individual SCMs (Schwarber et al 2019), which are in reality largely the result of inconsistent parameterisation. For example, there seems to be a widely held belief that FaIR is "cooler" than MAGICC7.0.1-alpha. Some of these authors admit that this belief is due in part to their previous work (see Leach, et al, 2018); but the more recent literature referenced above has not helped. This is not true - it may be that the choices made by the modelling groups in parameterising the models have led to one model running cooler than the other, but the models themselves are not "hotter" or "cooler". We hope to make this point clear throughout the text.

This paper argues that releasing a model with only a single 'standard parameter set' is misleading because it implies the model results are intrinsic to the characteristics of the model equation set, and not down to the parameter set chosen. We therefore wish to be clear in the text that although we have chosen to tune the model parameters in a particular way, this choice is not the only possible option. This is what we meant by "other methods would be equally valid"; for example one could tune to the model output of a particular ESM rather than observations, or to ESM multi-model mean output.

We aim to demonstrate that this model is able to convey the full range of climate responses seen in complex models (CMIP6) and observations through parameterisation of six equations. Being able to write down these equations and the parameters used could make results between chapters/working groups of the IPCC process much more coherent, even if they then chose to use different models for more thorough analysis or to study particular feature of the climate system not included in this model. We are not advocating for the cessation of all other SCM research.

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).

The model equation in, for example AR5 is indeed an extremely simple and transparent tool for this purpose of calculating GHG metrics for the present-day state of the climate. However, as demonstrated in Millar (2017), the IPCC AR5 Impulse-Response model does not capture the full response to pulse emissions of CO2 observed in ESMs, failing to reproduce future and historical emission -> concentration pathways well in comparison to eg. MAGICC7.0.1-alpha6 or more complex ESMs due to the constant airborne fraction implied by the model; this deficiency was overcome by Millar et al through the introduction of a state dependence.

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).

The 'innovations' are stepping the reader through a timeline of developments in the FaIR model code, from first inception in the AR5 chapter 8 supplementary material, through a carbon-only model in Millar et al. 2017, to a full SCM encompassing the full range of GHGs in Smith et al. 2018. We felt that the wider context regarding Joos work on the non-linearity of the carbon cycle or Meinhausens work on complete SCMs working on the full range of GHGs like MAGICC was supplementary to the aim of this text, but in the revision some further context has been added.

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.

We shall expand our discussion of this equation and the relevant parameters in the revision, while still avoiding excessive repetition of the discussion already in Millar et al (2017).

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

We have edited the text to include some clarification for the chosen analytic approximation of the form for alpha in this case in the text. The figure the text refers to in the SI is figure 1, which shows the computed alpha value for FaIRv1.3 and for the updated version presented. They agree closely over a large range of iIRF100 values. The caption gives further insight into differences at low and high iIRF100 values.

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

This result refers to figure 2, where historical concentration timeseries are computed given PRIMAP hist emissions inputs. We have edited text to explicitly refer to figure 2 here.

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.

We will provide a comparison of the CH4 lifetime in FaIR v2.0 and Holmes (2013) in the supplement for reference. We note that the previous iteration of the FaIR model did not include any parameterisations of CH4 atmospheric chemistry.

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

We have revised the way in which uncertainties are computed in FaIRv2.0, retaining the behaviour of the previous iteration: uncertainties (with the exception of the carbon cycle) are introduced at the forcing step for probabilistic simulations. This comment is therefore no longer relevant.

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.

We disagree that we have explicitly tuned for only Paris-relevant scenarios; in general we have tended to err on the side of tuning to observations. Even though the errors relative to the Etminan formulae are larger for the higher-emission scenarios, the maximum error is very small compared to the total forcings observed in these emission scenarios. Relative to the many other uncertainties involved in the simulation of these worst-case scenarios, we suggest that this error is acceptable, given the caveat provided in the text. It is worth noting that the line-by-line forcing calculation in Etminan has an associated error of 10%, considerably larger than the error introduced through our tuning procedure. We will make sure to fully compare our forcing tunings to the Etminan OLBL data in the revision in a clear manner to demonstrate the potential cases in which our parameterisation breaks down.

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

The most common unit used for N2O emissions is usually expressed as TgN. However, this appears in most cases to mean Tg of nitrogen (N2). We decided to be explicit about this in the original submission, however we will use the standard nomenclature in the revision.

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