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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.

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Leach et al. GIR

This paper proposes a set of equations to be used as a simple tool to compute temperate changes from given emission scenarios. This is certainly a worthwhile concept and this paper will make an important contribution to this goal. However, if the aim is to encourage wide uptake, considerable work is needed to make the paper more readable. The paper needs to explain the concepts more fully (one example out of many is the central role of iIRF100) but in a simpler way without assuming so much familiarity with the model.

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Discussion paper

My first point is rather minor, but it seems the justifications put forward for GIS are very similar to those originally put forward for FAIR. Did the authors find in developing FAIR that it became more complex than expected? Line 54 states "... representation of other greenhouse gases, significantly increasing the structural complexity of the FAIR model...". So is the main difference between GIS and FAIR the treatment of non-CO2 gases? Is this sufficiently different to be a new model?

My bigger criticism is on the "transparency" of the r0, ru, rT and ra coefficients. It is not at all clear that these translate readily into physically meaningful parameters that can be compared with and between models. This contrasts with the "Gas pools" and "Thermal boxes" that can be understood in terms of reservoirs and easily be compared eg with Joos et al. (2013) and Geoffroy et al. (2013). The formulation of alpha as a sinh is very non-intuitive, and so there is a concern that this will discourage the extent of the uptake of this model that the authors hope for. The defining principle in Millar et al. 2017 was the concept of the iIRF100. However, it was not obvious (or explained in Millar et al.) why this should have been a fundamental quantity for CO2, and it isn't explained here why this should be a fundamental quantity for methane and N2O. Table 1 in Millar et al. 2017 gives guiding analogues for the r-terms, but it is not clear that they mean anything physical for methane or N2O. For instance the text refers to natural methane emissions being accounted for in the fit to get the r0 term, but it is not obvious there should be any connection between emissions and r0.

Related to the above, the fitting procedures are not clear, particularly the value in fitting to the historical observations. With 14 parameters in table 1, it is not surprising that the model can fit the historical record well, but is it for the right reasons? It would be more useful to fit to idealised experiments (as is done in Joos et al., and Geoffroy et al., and in the C4MIP experiments for beta and gamma). Then it should be clear which terms in the models are being represented by parameters in GIS.

The sections describing calculations of e.g. species lifetime "Emission-driven historical simulations", and climate responses metrics (TCR, TCRE) "Idealised experiments"

need to be clearer as to what extent these quantities are inputs to the models and to what extent new information is provided through the fits to historical timeseries. Similarly for the emission metrics such as GWP.

It would be useful to compare SSP3-70 results from FAIR and GIS since this scenario has very different levels of ozone precursors. This would confirm (or not) that the treatment of ozone forcing in GIS was sufficient.

Line 49: What was “not quite adequate” about AR5-IR? This needs to be more explicit.

Line 80: This section needs to start with some introduction and explanation of the concepts rather than immediately diving into the equations.

Line 85: Equation (3) is very non-intuitive, it is the solution to Eq 7 in Millar et al. 2017, but it seems to overcomplicate very uncertain relationships. While iIRF100 might have been a useful concept in Millar et al. 2017, it is not at all obvious that it is the most useful formulation for GIS. This is particularly true for methane and N2O since later on the equation needs to be linearised. Why not just leave it in a linear form? What is “h”? 100 years?

Line 93: In what way is the analytical equation an approximation, what terms have been neglected?

Line 116: Since GIS is representing carbon-cycle models, it would seem much more sensible to fit to emission-driven models, rather than bottom-up emissions to observations. If there is any discrepancy between emission-driven complex models and observed trends, then that represents a process we don’t understand. Whereas in fitting to observations, any discrepancy will get folded into the fitted parameters in an unknown way and hidden. Can the rT and ru terms be related to the more physically relatable beta and gamma (either capital or lower case) of C4MIP?

Line 130: Presumably some fixed relationship between temperature change and water vapour change is used? This should be stated.

Line 131: As for CO2, this seems very dangerous. The suggestion from Smith et al. 2017 is that bottom-up emission estimates are not consistent with the observed concentrations. This fitting hides that by folding the inconsistencies into the fitted parameters. How many parameters are fitted? The sentence suggests the pre-industrial concentration is “specified” rather than fitted.

Line 146: What do these tuned parameters mean? In particular what does a r0 of 9.079 (years?) mean physically? The integral of a pulse of methane is equal to tau1=9.15 years, so it seems as if tau1 and r0 are degenerate. Similarly for N2O: r0 is just tau1(1-exp(-100/tau1)). Does r0 have any meaningful property that is different to tau1?

Table 1. These parameters all need units and guiding analogues.

Line 167 to 175: This explanation of the natural emissions needs expanding. Indeed in GIS, the natural emissions must be fixed at C0/r0 for methane and N2O. Smith et al. 2017 showed that the bottom-up emissions are inconsistent with the observed concentrations, so it is not clear how GIS can reproduce the historical concentrations from these emission (figure 2). How is Supplementary figure 2 generated if GIS can reproduce the observed concentration with constant natural emissions?

195: It is not clear whether the lifetimes presented here contain any new data, given the tau1 and ra are specified from the Prather and Holmes studies. Are the present and pre-industrial lifetimes just extrapolations based on those coefficients – if so, it is not surprising they agree. Again there is a suggested dependence on r0 - this one parameter seems to do a lot of work so there really does need to be a physical justification for it.

Line 220 -240: Again, this parameter fitting hides the science. The Etminan formulae are transparent, whereas the formulae in table 2 have different coefficients and additional terms. Are the non-primary coefficients significantly different from zero? The calculations for the f2 factors for the non-direct effects of methane on ozone and others need to be shown. I suggest sticking with the Etminan formulae and explicitly adding in

extra terms only when necessary to represent physical processes (CH4 - N2O overlap, ozone production). The calculations for these extra terms need to be provided.

Line 245: While ozone is historically correlated with CH4. The assumption of the same correlation continuing in future might not be valid. This could be quickly tested using the FAIR parameterisation for ozone.

Line 257: Where does this value of 60 MtSO2/yr come from?

Line 337: What is the full forcing for N2O? There is no additional forcing attributed to N2O in table 8.SM.6 of Myhre et al. 2013. The calculation used needs to be shown in full.

Line 339 “We find values comparable to the current literature”: These calculations of IRF100, iIRF100, AGWP100 and GWP100 and their methodology do not add value to the paper if all that can be said is that they are comparable to current literature. I suggest this section is removed. The values for CO2 come from the a_i and tau_i parameters which come from Joos et al, so it is not surprising that these agree with Joos. Similarly for the methane and N2O metrics, these are determined by the tau_1 and the f_i parameters which are derived from the same Prather, Holmes and Etminan papers as used in the literature. It is possible that the added temperature dependence of some of these parameters could affect the metrics - if the authors think this is worthy of discussion then the difference between the metrics for variable alpha could be compared with alpha=1 values (which is implicitly what is assumed in the literature).

Lines 354-373: Again it is not clear that TCRE is a new result from GIS, rather than a consequence of the parameters adopted in GIS. The text seems to suggest that the TCRE agreement with literature is a validation of the model, whereas it seems mostly driven by the same inputs as the literature (Joos -like carbon response, and Geoffroy-like climate response).

Line 368-369 “. . .lowers the upper end of the TCRE distribution and raises the median.”

If the authors are implying that using GIS can provide new information on these metrics, then this needs much more explanation on where this new information is coming from.

Line 380: The IIASA database has harmonised historical and future emissions, so it would make sense to use those rather than redoing this independently.

Line 388: CO2, N2O and CH4 concentrations are available on the IIASA SSP database (from MAGICC). Presumably the authors could quickly generate these from FAIR too.

Line 389: Why is the CO2 concentration slightly lower than FAIR given that GIS and FAIR use the same formulation for CO2?

Line 478: Given that the more complex chemistry and carbon-cycle models can't reproduce atmospheric concentrations from bottom-up emissions, it is extraordinary that a simple parameterisation of these complex models can do so.

Line 483: This GWP100 increase is not discussed earlier in the text. It shouldn't appear first in the conclusions. The value (35.3) is also different from any in table 4.

Line 484: The timing of peak warming is not discussed earlier in the text (apart from briefly for CO2). This shouldn't form part of the conclusions unless it is discussed more fully earlier. – is it not just that N2O has a longer lifetime ($\sim$120 years) than the $\sim$10 years for methane and $\sim$4 and 36 years for CO2, rather than any new finding from GIS?

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