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Geoscientific Model Development Discussions

Interactive comment on "FaIRv2.0.0: a generalised impulse-response model for climate uncertainty and future scenario exploration" *by* Nicholas J. Leach et al.

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Firstly, thank you for a thorough and extremely helpful review. Your comments and suggestions, particularly those relating to the carbon cycle explanation and the paper conclusions, have significantly improved the manuscript. Below are our point-by-point responses to you comments.

1. This is a very good point, and definitely not the impression we would want to give. We have re-phrased this paragraph along the lines suggested.

2. This is entirely fair. Similar to your later suggestion (#23), we feel that this discussion

of compromise when it comes to SCMs would be a good addition to our conclusion section. As suggested, we believe FaIR is very close to as simple as you can get without losing some emulation ability (or similarly to real Earth System response). However, this comes at the cost of a reduced connection to physical processes. Hence for some applications in which processes are crucial, FaIR won't be adequate. This being said, for much of the work done on scenario assessment, FaIR would be adequate - and possibly preferable as it is only five equations hence others could reasonably re-implement it if they wish to test outputs without relying on our implementation. In summary, your points are well taken, and hopefully we have reflected them in our revised conclusions.

3. This is a great suggestion and we've added in equations for Gu and Ga into the first equation set. We have also amended the caption in line with both your other suggestions.

4. Yes, this is correct. We will (as suggested) include some notes regarding implementation in the supplement. The sequencing does certainly suggest forward differences, as we have used - and probably also hints at what languages this particular author is most familiar with!

5. Short answer - yes, this feedback approach is something of a fudge (as an analytic approximation of the Millar et al. parameterisation which explicitly used the iIRF100). On the question of the impact of the 100-year timescale, under the FaIRv2.0 formulation, changing this to eg. 5/50/500 years would simply change the values of g0,g1 & the r coefficients (for say an optimal fit to a particular C4MIP model), and would not fundamentally change the behaviour at all - the optimal fit should be independent of this timescale choice. One motivation behind keeping it at 100 years would be for consistency with the previous iterations of the FaIR model (since the approximation is very good, r coefficients are easily comparable between previous iterations and those provided in this paper).

6. This is a very helpful suggestion - we have changed the text accordingly.

7. This is a fair comment. The main reasoning behind only including the general case was largely because this is how we've implemented it (returning to the point you make about including something on implementation). The way we'd recommend implementing is (at least for an array-focussed language like python or matlab) to use the fully general form and vectorise the calculation over all the gases, as this is likely optimal even if there are lots of redundant elements of 1s and 0s; it also makes the code somewhat more compact and (arguably) easier to understand since you don't have to write out all the different cases. Alpha is indeed (effectively) 1 for all the other gases. I say effectively because for all the other gases, we've set alpha equal to 100 & tau_1 equal to the actual lifetime / 100 (resulting in alpha * tau being equal to the actual lifetime). This is because for exceptionally long-lived gases, the g_0 & g_1 calculations become unstable (not representable within float precision), so we use this "trick" to get around this numerical issue. We'll make sure to try and emphasize how everything is really just a simpler (subcase) of CO2 better in the revision.

8. As you suspect, this is a mistake. Halon1202 emissions are zero in the RCMIP emission dataset for the SSPs, hence the flat FaIRv2.0.0 and MAGICC7.1.0 lines. We therefore exclude it from this figure, but still include it in the default parameter set.

9. Yes - in FaIRv2.0.0 we haven't parameterised in any sort of natural emissions (explicitly, though a quantity of natural emissions is implied by the pre-industrial concentration parameter). The real motivation behind this is transparency (& consistency with the CO2 gas cycle). If you want to include changes to the natural emissions of a particular GHG, then these would have to be included as a residual (over pre-industrial natural emissions) in addition to any anthropogenic emissions you put into the model.

10. This is correct - except that we actually don't even use the general form for N2O. We have re-worded parts of this section to try and emphasize that the equations underlying the majority of the gas cycles could be re-written in a simpler format than the general form given (ie. without an alpha parameter).

11. Yes - the parameters in S5 correspond to the more physically motivated parameters set out in (eg.) Geoffroy et al., 2013. The extreme-high values of C3 (& d3) do appear to represent fits in which the 3rd timescale adds little to the fit; and for these models it is highly likely that two exponentials would provide an identically good fit. The motivation behind using a three-exponential form (as opposed to the two-exponential form in previous FaIR versions) was drawn from Cummins et al., 2020 and Tsutsui, 2020. These two papers both suggested that using three exponentials provided significantly improved emulation relative to two. Summarising, the reasoning behind Cummins et al., 2020 and Tsutsui, 2020 was as follows. Improved response to large forcing impulse with three timescales (since the shortest timescale can be shorter), as the emulation can respond more rapidly to a sharp change in forcing, such as in the abrupt-4xCO2 experiment or a volcanic eruption as observed in GCMs. Cummins et al. obtained optimal emulation with three timescales rather than two for all 16 CMIP5 models they investigated.

12. This is correct, and we have amended this in the revision, taking default climate response parameters from the central estimate of the CONSTRAINED ensemble (as this would represent our best-estimate of the actual climate system response).

13. This is a very useful suggestion, and we now include a section in the Supplement about our implementation (python-focused, though should apply more generally).

14. This is an error on our part. All the numeric tables now specify quantities to three significant figures.

15. Yes- this is amended in the revision (for the idealised experiments we now measure time relative to initialising the model in year zero).

16. GFDL had variability in the CO2 concentration data (we are not certain why), which projected onto the FaIRv2.0.0 diagnosed emissions. Before, we smoothed the temperature series to obtain relatively noise-free diagnosed emissions. However, in the revision, we do not pre-process the raw data at all, resulting in diagnosed emissions

with significant variability inherited from the variability of the temperature series input (and CO2 concentration in the case of GFDL). We have done this for transparency, but if you feel it confuses the message of the figure then we can revert to smoothing the temperature & CO2 inputs?

17. See reply to comment 5 above.

18. This is very helpful - we've changed the text accordingly throughout.

19. Yes - the warming level is a stricter constraint than the rate. In the revision we've combined the two so they can no longer be imposed separately, making disentangling the contributions more tricky. The time period used in the constraint should not make too much difference as the global warming index is noise-free (though uncertainty due to internal variability is still incorporated) due to the underlying forced signal being noise-free. We now include a section on the sensitivity of the constraint to the observational dataset used in the GWI computation (in addition to the sensitivity to the priors), which hopefully helps to clear up some of the questions you have here.

20. We will amend the caption to clarify this. It is meant to represent the 2010-19 airborne fraction implied by the "anthropogenic carbon flows" figure (#9) in the global carbon budget paper.

21. Yes - unsure what's gone wrong here, but possibly a LaTeX compiling problem... will ensure the revision doesn't have this issue.

22. This is a great suggestion! We've added in extra rows for the scenarios that peak before 2100 (SSP1-19, 1-26 & 5-34).

23. These are some tricky questions - but nonetheless important, of course. We aren't very keen to make any suggestions of which model is "better"; and don't want to overlap too much with the much more comprehensive SCM comparisons in the RCMIP paper (Nicholls 2020a, b). We attempted to address some of these issues in the "response of simple climate models" section, though we agree that it would be good to have more

of a discussion around these issues in the "Conclusions" section. We have added an additional paragraph in the "Conclusions" which addresses some of the pros / cons of FaIR (& other SCMs) and how they might be used. On the point of whether FaIR is intended to represent the CMIP6 ensemble or a constrained version, we certainly agree that the answer is both. The CMIP6 representations could be useful for understanding how ESMs differ within the context of a very simple and transparent parameterisation. On the other hand, the constrained version is certainly more relevant in terms of scenario assessment and policy (especially in light of the quite high response of several CMIP6 models). We had attempted to outline this view in the "Uses of FaIR" section, but have re-worded this section to make these thoughts clearer.

24. We have now included a section in the supplement concerning the nonlinearities in FaIR. Since nonlinearities are introduced primarily through the CO2 and CH4 gas cycles, we focus on a series of CO2 / CH4 pulse experiments (against an SSP2-45 reference scenario). Although this doesn't completely characterise the nonlinearities (since by definition they are pathway-dependent), we think that it provides at the least some qualitative information about how the nonlinearities arise in FaIR; and also about some limiting cases for the default FaIR parameterisation, such as the response under a very large methane pulse ($\sim$1000GtCO2-eq), where the methane lifetime increases considerably and therefore significantly changes the behaviour of the model to CH4 emissions.

25. We've now added a brief implementation section outlining our own python implementation in the supplement.

26. Very good spot! Yes - completely correct and we've amended this in the revision.

27. This is a great idea. We're creating a permanent archive for the analysis (figures, model etc.) used here, and this will include a .csv of these key parameters, which we can point people to in the table caption.

Best wishes, Nicholas Leach & co-authors

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