

Interactive comment on “FAIR v1.1: A simple emissions-based impulse response and carbon cycle model” by Christopher J. Smith et al.

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Overall the paper is a useful, relatively clear explanation of the FAIR 1.1 model, and its difference from FAIR 1.0, with a useful short summary of the FAIR 1.0 model. The paper also describes how model parameters are estimated by comparing outcomes to the observed temperature record, and then uses the derived parameter ranges to project future radiative forcing, concentrations, and global mean temperature change under the RCPs. This section serves to document, and exercise, the version of the model that incorporates uncertainty.

I have a few general comments followed by a number of more specific ones. First, the overall philosophy of the model could use better highlighting. It appears that choices

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about design of the various model elements are guided by the desire to represent modeling approaches and parameter values presented in AR5. This is not always clear, and since other approaches are possible, it makes it a little confusing in spots why some choices were made. An early, clear statement of the approach and its rationale would be useful.

Also, as noted below, some aspects of the model are insufficiently described. Also, it would be useful to more clearly indicate when new approaches to modeling specific species are being used, and when these are borrowed from existing simple models (MAGICC or others). It would be useful, for example, to provide a summary of similarities/differences between FAIR and the MAGICC model, since MAGICC serves as a key point of reference for FAIR and for the evaluation of results.

Last, the paper seems to downplay the difference between projected warming with the FAIR model and warming according to Rogelj et al as reported in AR5. But this amounts to a full degree difference in the 2081-2100 mean under RCP8.5, which is a very substantial difference. This difference in results should be more clearly indicated (quantified in the abstract), and its reasons (difference in ECS/TCR and historical radiative forcing) pointed out more prominently. Related to this, the sensitivity of ECS/TCR (and warming) to prior distributions seems also downplayed, by indicating they are of equal importance as other factors (temperature record, eg) which have a substantially smaller quantitative effect on results. This particular factor should be identified as especially important.

Specific comments:

Abstract

The comparison of the uncertainty bounds for ECS and TCE to those reported in AR5 is worth pointing out, but should be put in context since they are not based on the same type of analysis. The AR5 range takes into account multiple lines of evidence, not just the type of study here, with a simple model constrained by observations.

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The statement that the range of temperature projections under the RCP scenarios is lower in the FAIR model than those reported in AR5 is a significant outcome (especially depending on what the size of this difference actually is). The reasons for it (identified later in the paper) should also be included in the abstract.

section 2

eq 3 is not explained as clearly as it could be. In what sense is IRF-100 associated with 100 years? The description seems to indicate that it is the cumulative atmospheric carbon load over 100 years following a pulse emission, and it is being equated to an expression depending on temperature and cumulative carbon uptake, but at an unspecified date in the future. The equation should make clearer the time variable, start/end times of a 100 year period, etc. It would also be useful to give the overall intuition of this approach. I assume it is that it relates the impulse response function time constants, which are derived in conditions that do not allow for representation of dependence on sink saturation and temperature feedback, to a situation in which those processes are acting. This allows derivation of the alpha parameter representing those effects.

For methane and N₂O, it seems like the approach is to specify a lifetime, and then adjust natural emissions over time so that historic concentrations are reproduced. Why is this preferable to specifying natural emissions, and estimating the lifetime that best fits the concentration data, leaving an unexplained error term that could represent variations in natural emissions or other errors (missing processes, error in anthropogenic emissions, etc.)? This is an example of where stating the general philosophy of the model might have helped, if the rationale is to use a lifetime provided in AR5. At a minimum some discussion of options and choices here is warranted.

section 2.2.3: it is unclear how well the regression approach here captures the relationship observed in data or models. Some indication of the performance of this regression model should be given, along with best estimates of coefficients.

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2.2.4: it is noted that the functional relationship in eq 12 is from Meinshausen et al, however it is unclear if the rest of the approach (fitting to the AR5 ERF timeseries) is also the one taken by Meinshausen (or anyone else) in estimating parameters. Should be clarified what the source of the approach to the modeling and/or parameter estimation is, or whether it is new.

eq 15: as for eq 3, give some quantitative measure of how well this regression model explains the historical data (or show the scatter plot with the estimated model relationship)

2.2.9: The incorporation of the biophysical effect of land use change on forcing through albedo change may be useful, but it leaves out another important effect through changes to evapotranspiration, thus giving an incomplete accounting of biophysical effects. The authors cite one study, with one climate model, which drew conclusions based only on historical land use change, to justify including only the albedo effect. Other models will reach different conclusions about the relative effects of these two processes. Also, the effects are latitude-dependent, and the Andrews study they cite notes that the albedo effect historically has been dominated by high latitude Northern Hemisphere changes in winter (dependent on snow cover). Thus the approach of a single coefficient relating land use to albedo forcing is questionable, given that the model is intended to be applied to a wide variety of scenarios in the future with different latitudinal distributions of land use, and probably changing snow cover.

At a minimum all of these issues should be acknowledged and discussed, and the proposed approach relative to others (see eg Andy Jones paper at <https://link.springer.com/article/10.1007/s10584-015-1411-5>) should be justified. The quality of the approximation described by eq 17 needs to be quantified.

2.3 Temperature change: The intro to this section notes that the approach differs slightly from FAIR 1.0, but earlier in the paper FAIR 1.0 is described as a carbon cycle model. If it also includes a simple climate model, that needs to be corrected in the text.

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section 3: In this section some of the distributions from which parameter values are drawn are specified completely, others don't seem to be. All distributions should be fully specified, possibly in a table, and referred to from the text.

figure 4a: it would be useful if a separate plot with different scale could be shown for the historical period. With the y axis scale set to capture the full range in 2100, it is difficult to see any detail about the relation between the FAIR range and the observations.

section 4.5: a more explicit description of the substantial differences in temperature change in 2100 between FAIR and Rogelj et al should be included. The differences in median temperature change are up to a full degree. Also, the high end of the range is very substantially truncated in FAIR, which would be extremely important to risk assessments. The text notes that they are different but underplays the size of the difference.

Table 5: section numbers referred to should all be in section 5, not 4.

Conclusions: it seems to me that the estimates of ECS and TCR (and future warming) are substantially more sensitive to the assumed priors than to other aspects of the analysis that are tested. The text here puts them all in the same category of showing "mild sensitivity". The alternative priors lead to a range of ECS/TCR that would reduce the difference in the 95% level of projected 2100 warming by half, relative to Rogelj et al. No other sensitivity would have that large of an effect.

In addition, here again the difference in projected warming by 2100 are very different from those in Rogelj et al, which seems worth emphasizing here. A full degree of warming difference in RCP8.5 is a substantial change in outlook.

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