

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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Received and published: 8 March 2018

Dear Brian,

Thank you for your time spent in reviewing this manuscript and the useful comments you provided. Original comments are given in bold, which are responded to point-by-point in regular font.

General comments

Overall the paper is a useful, relatively clear explanation of the FAIR 1.1 model,

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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.

Thank you for your largely positive comments overall. In addition to incorporating uncertainty there are several processes that convert emissions of non-CO₂ species to radiative forcing, which is a development from FAIR v1.0.

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 choice 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.

We agree that this could have been made clearer. In the introduction we now state:

“The model philosophy in FAIR is to represent these processes as simply as possible, and to be able to emulate the ERF time series in AR5 given input emissions. FAIR is written in Python and open source.”

Also, we have taken this opportunity to rewrite some of the other paragraphs in the introduction to improve readability and conciseness.

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

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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.

We have strived to make the treatment of each process a little clearer. We highlight in section 2.2 where a process is borrowed from MAGICC or elsewhere; where this is not stated, it has been derived by the authors.

Many of the processes in MAGICC (e.g. the carbon cycle) are not easy to summarise in a simple table, and some are not known to the authors (one example being the assumptions of natural emissions used in MAGICC), so we have not included this comparison. In further work we will investigate the different responses between the models in more detail.

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.

Thank you for this important comment. Following the comments from the other reviewer, some of the assumptions made for the scientific components of the model have now been improved, particularly for tropospheric ozone and aerosols. The consequences are that the constrained distributions of ECS and TCR are a little higher than previously and we don't see the full degree difference in RCP8.5 any more (it is about 0.5K).

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The last sentence of the abstract has been modified:

"The range of temperature projections under RCP8.5 for 2081–2100 in the constrained FAIR model ensemble is lower than the emissions-based estimate reported in AR5 by half a degree, owing to differences in forcing assumptions and ECS/TCR distributions."

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.

Thank you for this comment. In retrospect the AR5 figures are a "likely" (> 66%) range so we were in fact not comparing the same ranges. This sentence has been updated:

"The constrained estimates of equilibrium climate sensitivity (ECS), transient climate response (TCR) and transient climate response to cumulative CO₂ emissions (TCRE) are 2.93 (2.04 to 4.32) K, 1.59 (1.07 to 2.50) K and 1.44 (0.97 to 2.31) K (1000 GtC)⁻¹ (median and 5–95% credible intervals). These are in good agreement, with tighter uncertainty bounds, than the AR5 likely range, noting that AR5 estimates were derived from a combination of climate models, observations and expert judgement."

We have also updated the commentary in section 4.1 where these results are discussed.

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 out-

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come (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.

As stated above, this has now been changed.

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.

You have the correct intuition for how this works. Equation 3 parametrises what would be the integrated additional carbon loading after 100 years ($iIRF_{100}$) in response to a one-time (strictly infinitesimal) pulse emission of CO_2 at the current point in time in the model's integration. Knowing $iIRF_{100}$ allows the scaling factor, α , on the model carbon cycle response timescales to be calculated. We parametrise the continuous evolution of $iIRF_{100}$ in response to this purely hypothetical pulse experiment to evolve with the present climate state. Numerically, this is implemented in equation 3, by using T and the cumulative carbon uptake from the previous model timestep.

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 estimat-

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ing 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.

Note that we have now updated the natural emissions to exactly balance the concentrations given anthropogenic emissions. It is our understanding that MAGICC does something akin to the reverse, where they start with concentrations and back out natural emissions given the anthropogenic emissions.

We have experimented with a constant natural emissions rate in model development. We find that the trajectory of historical concentrations is unrealistic (especially for methane), and this problem is confounded by using time-varying atmospheric lifetimes. Additionally, natural emissions are uncertain and vary interannually. The timeseries provided in figure 2 are the model defaults, and the user can specify their own.

You are correct that as far as possible we wanted to use AR5 estimates to inform the model. For methane this was not possible; using the 12.6 year lifetime in AR5 gives future emissions that are too high, whereas using 9.3 years gives the expected results both over the historical period (for a reasonable level of natural emissions) and in future, where they agree fairly well with the RCPs. We give some justification for this at the end of section 2.1.2.

Added: "We prefer to use varying natural emissions with a fixed atmospheric lifetime of CH_4 and N_2O , firstly because this provides a good match to observed and projected concentrations and secondly because this is consistent with the simple model philosophy. Other methods of calculating concentrations of these gases are possible, for example using a fixed natural background emission and relating any differences between observed and calculated historical concentrations as an error term (either in the

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natural or anthropogenic time series or missing processes), or by adjusting the atmospheric lifetime of each gas over the historical period in order to match the observed concentrations at each time step.”

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

As acknowledged in our response to the first reviewer, we have now moved from using a regression-based approach to one that is informed by estimates from ACCMIP models (Stevenson et al., 2013). The evolution over the historical period is similar to AR5 up to around 1970 (fig. 5e in paper), after which the ACCMIP relationship results in a slightly stronger forcing than estimated by AR5 (but well within the uncertainty range in AR5).

The main difference is in the evolution of RCP8.5 past 2005 compared to the regression-based relationship which now shows a tropospheric forcing some 0.2 W m^{-2} higher than before. This is consistent with the modelling in Stevenson et al. (2013). The ozone forcing coefficients and year-2000 forcing values are provided in table 4.

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.

The function takes the same form as Meinshausen et al. (2011), as this was the best simple model that could be found in the literature. We do not know what the basis of their relationship is. By training our curve fit to the ERF AR5 time series we get a different parameter combination to Meinshausen et al. (2011).

We have updated this description:

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“ $a = -1.46 \times 10^{-5}$, $b = 2.05 \times 10^{-3}$ and $c = 1.03$ in eq. (12) are fitting parameters that are found by a least-squares curve fit between eq. (12) and the stratospheric ozone ERF timeseries from AR5; due to this data fitting approach, our parameters differ from MAGICC.”

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)

As with tropospheric ozone, the aerosol forcing relationship has been updated to use established model results from AeroCom (for ERFari) and a simulator of the Ghan et al. (2013) model for ERFaci. The relationship of how each species affects ERFari (in terms of forcing per Mt emissions) and the ERFari in year 2011 is given in table 4.

The underlying model that calculates ERFaci is too slow to run in FAIR for large ensembles so was emulated based on emissions of SO_x and primary organic aerosol (BC+OC). The relationship to precursor emissions and how it compares to Ghan’s model is shown in figure S1.

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

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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.

We appreciate our treatment of land use forcing may not include several important processes that occur in the real world that would only be possible by using an external gridded activity dataset (i.e. from LUH). However, the aim of the FAIR model is to produce a plausible projection tool with as little complexity as possible. The simpler the inputs to the model, the easier it will be for others to use it. To include gridded land processes would require something more complex than a zero dimensional model like an EMIC.

The basis for using this one coefficient was the observation that scaling with cumulative CO₂ land use emissions agrees remarkably well with the shape of the future forcing scenarios for the RCPs (compare dotted and solid coloured curves in fig. 5k) in MAGICC, whereas the fits to the historical data are not too bad. If MAGICC did use a more complex relationship, then it can be approximated very well with this simple formula. Although MAGICC may also contain errors and biases, we can show that the treatment of land use forcing in FAIR is no worse than in that model. We have expanded the discussion to include the points that you raise above.

The RMSE between the AR5 land use forcing and eq 17 is 0.012 W m⁻² over the historical period.

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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FAIR v1.0 has a temperature change component included. The 4th paragraph in section 1 is updated (now "FAIR v1.0 is well-calibrated to the carbon cycle and temperature response of earth system models").

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.

The ERF uncertainty ranges are given in table 1. Carbon cycle parameters are described in the text and are described as Gaussian, quoted as a mean and 90% uncertainty range. The section on ECS and TCR we have also re-written slightly and trust that it is now clearer.

For non-Gaussian ERF uncertainties the source of the original distributions are made more clear, e.g. AR5.

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.

An inset plot is now added to figure 4a which shows the historical period for CO₂ in more detail.

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.

The new relationships we use for tropospheric ozone and aerosol forcing result in smaller temperature differences between FAIR and Rogelj et al. (2012), particularly

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in the lower RCPs. For RCP8.5, FAIR is around 0.5 K lower than Rogelj et al. (2012). This is still significant and a sentence has been added: "The difference of 0.5 K in the median end-of-century warming in RCP8.5 could be particularly important in policy assessments."

We have taken this opportunity to improve the readability of section 4.5. Some superfluous or no-longer-relevant sentences have been removed.

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

Thank you for picking up on this reference to the old section numbering. It has now been updated.

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.

We agree: thank you for your suggestion. We have changed the description to highlight that the ECS, TCR and TCRE posteriors are fairly insensitive to the constraining dataset whereas they are more sensitive to the prior distribution.

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.

For the updated model, these differences are smaller (0.5K in RCP8.5), so we do not change the main description as it stands but add a few words that provides this comparison.

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-266>, 2017.

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