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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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This paper covers two topics, the description of a simple climate model and calculation of observationally-constrained climate sensitivity. This may be doing too much in one paper. The question of climate sensitivity is long-running and of high importance, so if the authors believe they have new insight into this it would deserve its own paper with a title that reflects this and probably not in a model development journal. Conversely if the climate sensitivity calculations are intended more as an illustration of the FAIR model then much of the detail is overkill. My review will be more focussed on the model development aspects of the paper.

The FAIR model has the potential for being a very useful tool that could be widely used.

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Discussion paper

Therefore the authors need to take care that it is constructed in such a way as to be generally useful and not just for RCP scenarios. For instance it should be set up to be able to take in CO2-only emissions rather than having to subtract the non-CO2 effects from RCP8.5. The paper needs to be clear as to whether this is a model that is suitable for use by the wider community yet.

For a few of the forcing agents (e.g. aircraft, land use) there is a convoluted methodology to recreate the original activity data from the RCP emissions. A tool like this should be designed to take activity data as its basic input. It is fine for this paper if the authors have recreated the activity data from the RCP in this instance to test the model, but if the FAIR tool were to be used in an aircraft or land use study it doesn't make sense to have to generate NOx or CO2 emissions from the activity data so that FAIR can then invert them to get back to the original activity data.

It is entirely inappropriate to use the AR5 ozone and aerosol ERF time series to back out the response coefficients by linear regression. These time series were generated by a few models (it may only have been GISS) that ran forward to generate ERFs. These time series were intended to illustrate the evolution of the ERFs, not as the last word. These are not the time series that were used to force any of the CMIP5 GCMs, nor the forcings diagnosed from CMIP5 (apart maybe from GISS). Hence the ability or not to recreate the AR5 time series using FAIR is meaningless since none of the GCMs used these. Even if these time series had been more rigorously generated it is not sensible to use linear regression to derive the response coefficients as the covariances are so large. I suggest using Stevenson et al. 2013 and Aerocom to derive response coefficients. Whichever method is used, the coefficients need to be listed in tables.

Specific comments:

Page 2, line 14: Ocean sinks will become less effective too. Is this accounted for in FAIR?

Page 2, lines 27-30: IPCC merely used the carbon cycle responses from Joos et al. 2013 rather than constructing anything new. The Joos et al. responses were in turn taken from fits to C4MIP so would have included any feedbacks for biospheric uptake and temperature inherent in C4MIP models.

Page 3, line 7: Replace “validated” with ”calibrated”

Page 3, line 11: It is not quite clear what “expected to be smoothed out in the global mean.” Is trying to say. Obviously the global mean is an average of the regional variations by definition.

Page 4, equation 1: State that Ri are masses in kg.

Page 5, equation 4: State that Ct are molar mixing ratios. Equation is missing a factor delta_t.

Page 5, lines 3-5: The natural emissions in fig 2 look very unrealistic. What do MAGICC natural emissions look like? Do they have a different way of addressing this?

Page 5, lines 19-25: The methane lifetime is a function of methane concentration and this dependence is not difficult to implement, see eg MAGICC description or IPCC TAR 4.2.1.1. For increasing emissions the concentrations increase more rapidly than for a constant lifetime. This probably explains the discrepancy in the methane for RCP8.5 in fig 4(b).

Page 6, section 2.1.3: This section needs an explanation of how to avoid double counting as the CO2 emissions are often based on the total fuel consumed rather than specifically how much is fully oxidised all the way to CO2.

Page 6, line 14: Myhre et al. 2013b did not show that ERF agrees with RF, rather they found that there had not been sufficient research to determine whether the ERF was different to RF. As the authors are well aware the PDRMIP project amongst others has compared RF and ERF more recently.

Page 7, section 2.2.2: Use “well-mixed greenhouse gases” to exclude ozone.

Page 7, section 2.3.3: This linear regression is not an appropriate way to derive the response coefficients since the historical emissions strongly co-vary. Deriving a negative correspondence with NMVOC is not merely an interesting detail, it is physically wrong and so undermines the whole procedure. This must also mean that some or all of the other coefficients are overestimated to compensate. While this method may give acceptable agreement for the RCP scenarios in fig 5(e) it would give incorrect predictions when applied to more idealised scenarios e.g. if the FAIR tool were used to assess the climate impact of biomass stoves. There are sufficient data in Stevenson et al. 2013 to be able to derive more physically credible coefficients. The coefficients need to be provided in a table and compared with other studies.

Page 8, section 2.2.5. The AR5 value assumed stratospheric water vapour added 15% of the Myhre et al. 1998 methane RF. It would add a lower percentage of the Eminan et al. ERF.

Page 8, section 2.2.6: It is dangerous to build in this back calculation of aircraft activity into a tool. It is much safer to use activity data as the input. If the authors have chosen to back activity data out from RCP datasets for the purpose of this paper that’s fine, but it shouldn’t be hidden within the tool.

Page 8, section 2.2.7: As with ozone, linear regression is not an appropriate way to derive the response coefficients. Using speciated RFari forcing from AR5 and Aerocom to divide up the total ERFari+aci is a more transparent method. The coefficients need to be provided in a table and compared with other studies.

Page 9, section 2.2.9: Again, it is dangerous to build in this back calculation of land use activity into a tool. It is much safer to use activity data as the input. If the authors have chosen to back activity data out from RCP datasets for the purpose of this paper that’s fine, but it shouldn’t be hidden within the tool. The forcing is missing a minus sign.

Page 11, section 3.3: Note the +/- 20% uncertainty in the CO_2 ERF reflect uncertainty in our best estimate of the CO_2 forcing, not how it is implemented in the climate models. The actual CO_2 ERF "seen" by individual GCMs may lie outside this range.

Page 12, section 4.1: It is plausible that there may be an anti-correlation between a models F_{2x} and its climate sensitivity (in K/(W/m2)). Is this accounted for in this study? Defining ECS and TCS in terms of F_{2x} rather than in K/(W/m2) might hide some of the model variation in F_{2x}.

Page 13, line number 15 (actually the first line!): Given that the FAIR parameters were derived from the historical GHG concentrations, it doesn't seem much of a test that it can reproduce them.

Page 13, line number 18 (3rd line): How can MAGICC reproduce the kinks in CO_2, but FAIR can't?

Page 13, line number 28: The authors recognise the problems with a fixed methane lifetime. It is not difficult to implement this to rectify this errors.

Page 13, section 4.3, lines 13-14: It's not surprising the linear regression reproduces the time series it was fit to. The future ERFs need to be compared to Stevenson et al. 2013, not MAGICC.

Page 13, line 15. It is not surprising that the model can reproduce the AR5 stratospheric ozone ERF as FAIR uses exactly the same formula as AR5 (scaling with EESC).

Page 13, line 17. The reason FAIR overestimates the AR5 stratospheric water vapour value is because it scales up the Etminan et al. methane forcing which is 25% larger than the Myhre et al. 1998 forcing.

Page 15, section 4.5: Since the methane forcing is 25% stronger in FAIR, presumably the TCR has to be lower to compensate. Does this explain the lower future projections?

Bottom of page 15, top of page 16: I don't understand this complicated method for calculating the TCRE to CO2-alone. Surely FAIR can be forced with just CO2 emissions and will output the temperature? If this is a CO2- alone calculation why does equation 22 account for the effect of non-co2 temperature changes?

Page 18, section 5.2: This section needs to be expanded to discuss the difference between relative sensitivities in terms of F2x and absolute sensitivities in terms of K/(W/m2). If F2x is lower then the absolute sensitivity must be higher and hence the larger response when including the non-CO2 forcings.

Page 19, line numbered 18: I didn't understand why with a smaller (magnitude) present day aerosol forcing the 2100 temperatures are higher. Surely smaller aerosol forcing means lower TCR/ECS?

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